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OPINIONS OF THE PRESS

ON

WATSON'S PRACTICE OF PHYSIC.

IT would appear almost superfluous to adduce commendatory notices of a work which has so long been established in the position of a standard authority as "WATSON'S PRACTICE." A few extracts are, however, subjoined from reviews of the new and improved edition.

The fourth edition now appears, so carefully revised, as to add considerably to the value of a book already acknowledged, wherever the English language is read, to be beyond all comparison the best systematic work on the Principles and Practice of Physic in the whole range of Medical literature. Every lecture contains proof of the extreme anxiety of the author to keep pace with the advancing knowledge of the day, and to bring the results of the labours, not only of physicians, but of chemists and histologists, before his readers, wherever they can be turned to useful account. And this is done with such a cordial appreciation of the merit due to the industrious observer, such a generous desire to encourage younger and rising men, and such a candid acknowledgment of his own obligations to them, that one scarcely knows whether to admire most the pure, simple, forcible English—the vast amount of useful practical information condensed into the Lectures—or the manly, kind-hearted, unassuming character of the lecturer shining through his work.—*London Med. Times and Gazette*, Oct. 31, 1857.

Thus these admirable volumes come before the profession in their fourth edition, abounding in those distinguished attributes of moderation, judgment, erudite cultivation, clearness, and eloquence, with which they were from the first invested, but yet richer than before in the results of more prolonged observation, and in the able appreciation of

A few of the commendations with which previous editions have been honoured in this country are likewise added.

One of the most practically useful books that ever was presented to the student—indeed, a more admirable summary of general and special pathology, and of the application of therapeutics to diseases, we are free to say, has not appeared for very many years.—*N. Y. Journal of Medicine*.

To say that it is the very best work on the subject now extant, is but to echo the sentiment of the medical press throughout the country.—*N. O. Med. Journal*.

Of the text-books recently republished, Watson is

From the late Professor of Theory and Practice of Medicine in the University of Pennsylvania.

Watson's Practice of Physic, in my opinion, is among the most comprehensive works on the subject extant, replete with curious and important matter, and written with great perspicuity and felicity of manner. As calculated to do much good, I cordially recommend it to that portion of the profession in this country who may be influenced by my judgment.

the latest advances in pathology and medicine by one of the most profound medical thinkers of the day.—*London Lancet*, Nov. 14, 1857.

The author has evidently been at much pains to follow the course of modern research; the practitioner, and the student of medicine in its practical aspect, will equally feel indebted to Dr. Watson, for having found time to communicate to them so large an amount of novel information as is introduced into this new edition, in so pleasing and instructive a manner.—*Edinburgh Med. Jour.*, Nov. 1857.

Lecturers, practitioners, and students of medicine will equally hail the reappearance of the work of Dr. Watson in the form of a new—a fourth—edition. We merely do justice to our own feelings, and, we are sure, of the whole profession, if we thank him for having, in the trouble and turmoil of a large practice, made leisure to supply the hiatus caused by the exhaustion of the publisher's stock of the third edition, which has been severely felt for the last three years. For Dr. Watson has **not** merely caused the lectures to be reprinted, but scattered through the whole work we find additions or alterations which prove that the author has in every way sought to bring up his teaching to the level of the most recent acquisitions in science.—*Brit. and For. Medico-Chir. Review*, Jan. 1858.

very justly the principal favourite.—*Holmes's Report to Nat. Med. Association*.

As a text-book it has no equal; as a compendium of pathology and practice no superior.—*N. Y. Analyst*.

We know of no work better calculated for being placed in the hands of the student, and for a text-book: on every important point the author seems to have posted up his knowledge to the day.—*American Medical Journal*.

N. CHAPMAN, M. D.

PHILADELPHIA, September 27th, 1844.

LECTURES

ON THE

PRINCIPLES AND PRACTICE

OF

PHYSIC;

DELIVERED AT KING'S COLLEGE, LONDON,

BY

THOMAS WATSON, M.D.

FELLOW OF THE ROYAL COLLEGE OF PHYSICIANS, LATE PHYSICIAN TO THE MIDDLESEX HOSPITAL,
AND FORMERLY FELLOW OF ST. JOHN'S COLLEGE, CAMBRIDGE

A NEW AMERICAN,

FROM THE LAST REVISED AND ENLARGED ENGLISH EDITION.

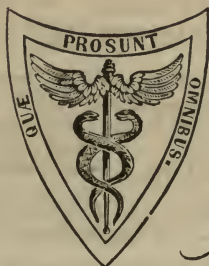
WITH ADDITIONS

BY

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AMERICAN PHILOSOPHICAL SOCIETY, ETC. ETC.

WITH ONE HUNDRED AND EIGHTY-FIVE ILLUSTRATIONS ON WOOD.



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PREFACE BY THE EDITOR.

IN the edition of which the present volume is a reprint, the lectures of Dr. Watson have undergone a thorough revision, and whatever of value recent research has added to our stock of knowledge in the various departments of medical science has been carefully incorporated in them. The lectures on fever especially have been greatly enlarged and improved: the positive distinctions that have been insisted upon by eminent pathologists between typhus and typhoid fevers, are recognized as being founded in truth. The extent of these additions is shown by the fact, that notwithstanding a very considerable enlargement in the size of the page, the work has been increased by about two hundred pages.

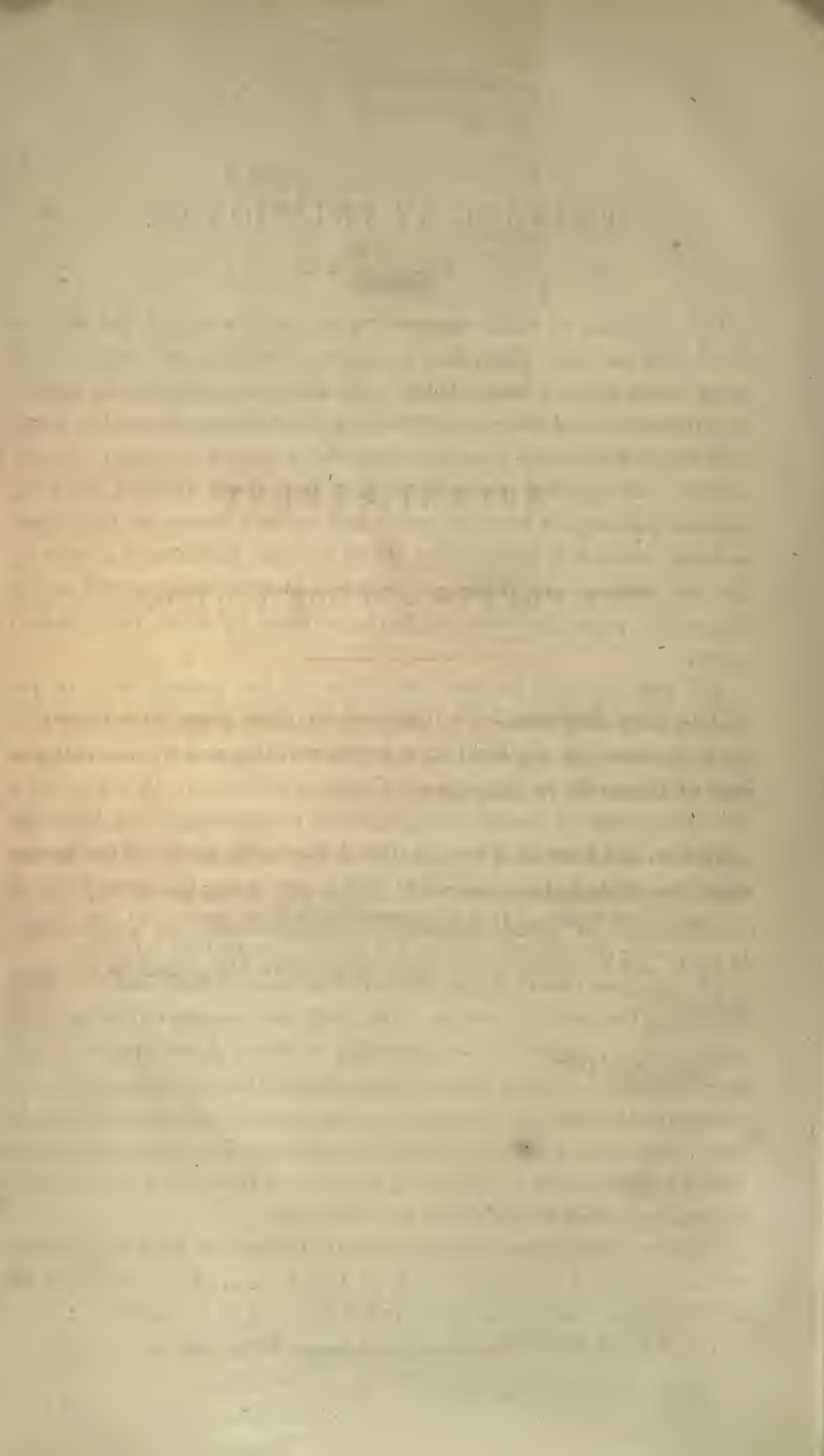
The very full and accurate exposition of the present state of pathology and therapeutics, in reference to the diseases embraced in these lectures, has rendered it unnecessary to augment materially the size of the work by frequent or extensive additions. In regard to a few of the forms of disease more particularly interesting to the American physician, the account given by the Author will be found somewhat defective, while he has omitted to notice one or two affections endemic to the United States. It is to remedy these deficiencies that the Editor, in preparing the present edition, has mainly directed his attention.

The intrinsic merits of Dr. Watson's Lectures are sufficient to ensure for them a favourable reception. For comprehensiveness of matter, accuracy of detail, candour in the discussion of those questions upon which a difference of opinion exists among physicians, and perspicuity and felicity in the manner of presenting the several subjects embraced in them, they stand unrivalled. If by the additions he has introduced, the Editor has succeeded in increasing the value of this edition in any slight degree, he will be amply repaid for his labour.

The few illustrations introduced by the Author have been considerably added to, from a conviction that, in this manner, the interest of the student in the descriptions given in the text will be enhanced.

The Editor's additions are enclosed in brackets [].

PHILADELPHIA, *July*, 1858.



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TO

THE FOURTH LONDON EDITION.

THAT these Lectures have remained for three years "out of print" has been caused by the necessity for their revision, and by the Author's want of leisure for revising them.

Anxious as he has been to make them less undeserving of the favour which they have hitherto met with, he has still, in excuse of their many imperfections, to plead unceasing demands upon his whole time, strength, and thoughts, by the more imperative obligations of his professional life.

September, 1857.

ADVERTISEMENT

TO

THE FIRST LONDON EDITION.

THE following Lectures were put together, with unavoidable haste, during the Medical Session of 1836-37, in which they were first delivered. They were repeated, with slight variations, for four successive years; the Author always meditating, but never finding time to accomplish, their thorough reconstruction and revision. They were afterwards printed, to fulfil a rash promise, in the pages of the *Medical Gazette*: and they are now published, in a collected form, at the request formally conveyed to him in writing, of many who had heard or read them, including several of his Colleagues at King's College.

Writing for mere beginners, and without any thought of future publication, the Author took no pains to note authorities as he went along. He may often therefore have used, without acknowledgments, not only the facts and reasonings, but sometimes, perhaps, the very words of others. This omission he regrets, but is now unable to supply. Neither has he leisure to correct, if that were desirable, the colloquial and familiar style in which the Lectures were originally composed.

Should they attract the notice of any who are no longer *in statu pupillari*, he would request such readers to bear in mind for whom these lessons were intended. They do not profess to present a formal and complete treatise on the Practice of Physic, much less to exhaust the various subjects upon which they touch. His chief hope is that they may prove useful as a text book for Students.

As they were passing through the press, such additions and alterations have been introduced as the Author would have made, had he continued to deliver the Lectures orally.

HENRIETTA STREET, CAVENDISH SQUARE,
September, 1843.

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LECTURES

ON THE

PRINCIPLES AND PRACTICE OF PHYSIC.

INTRODUCTORY LECTURE.

GENTLEMEN,

IN approaching any new course of systematic inquiry, there are certain points concerning which the inquirer should always be careful to satisfy himself. He should comprehend, distinctly, what it is that he proposes to learn; its subject matter, and its objects. He should consider whether he is about to adopt the most easy, direct, and effectual means for obtaining his purpose; and whether he is qualified, by the possession of the requisite preliminary information, for pursuing his inquiries with intelligence and profit. To these points, and to some others, as they are connected with the duties which have been entrusted to me in this College, I wish briefly to direct your attention on the present occasion. It will be my endeavor to furnish you, at the outset, with clear notions of the nature and the ends of that branch of study upon which you are now about to enter; to explain why it is taught, and how far it may be taught, by oral discourses; to point out to you what may reasonably be expected from me, and what, to render my attempts prosperous, will be required on your parts. Something also it is expedient that you should know beforehand respecting the general order and arrangement of the course: and a short explanatory comment upon some of the terms that we shall constantly be employing, will clear the way for the succeeding lectures, which forming, more strictly than the present, a part of the series, will also be more strictly didactic in their character.

The subject of our study is that wonderful thing, the animal body — and more particularly the human body; its construction and qualities; its actions and its sufferings; its derangements; its decay.

In this study, which affects the mind with a strong feeling of curiosity, not unmixed with awe, you have already advanced a certain way: for you have observed the outward form and configuration of the body; examined its internal composition and structure; and learned what is known of its various endowments, the working and the uses of its several parts.

This amount of knowledge was indispensable to your further progress. But it forms a portion only of what you assemble here to learn: or rather it is the necessary preparation for that ulterior knowledge which it is your main purpose to acquire. The sublimer speculations springing naturally from the researches in which you have as yet been engaged, have not, I trust, been unregarded. You cannot have looked into the mechanism of that intricate, but perfect work, — you cannot have contemplated its fulness of exquisite contrivance, its endless examples of means adapted to ends, its prospective expedients against future needs, its compensations for inevitable

disadvantages, its direct provisions for happiness and enjoyment, — without receiving the profoundest conviction of the being and the attributes of its Maker. It is upon human anatomy that Paley, in his unrivalled argument for Natural Theology, “takes his stand;” and sixteen centuries before him, Galen had felt that, in writing his anatomical treatises, he was composing a hymn to the Deity; that an exposition so indicative of the wisdom, the power, and the goodness of God, was an act of piety and praise. But beyond, though not above, these higher objects of a diligent investigation of man’s bodily fabric, we have another and still a noble end; and it is my business to take you one step nearer to that end. Hitherto you have been told of structure and of function. Henceforward our theme must be of health and of disease. Of health, that we may understand disease; of disease, that we may, under Providence, restore health. Our objects are to preserve the one; to prevent, remove, or mitigate the other.

What then do these contrasted terms denote?

Health we regard as a standard condition of the living body. But it is not easy to express that condition in a few words, nor is it necessary. My wish is to be intelligible rather than scholastic; and I should probably puzzle myself as well as you, were I to attempt to lay down a strict and scientific definition of the term health. It is sufficient for our purpose to say, that it implies freedom from pain and sickness; freedom also from all those changes in the structure of the body that endanger life, or impede the easy and effective exercise of the vital functions.

It is plain that health does not signify any fixed and immutable condition of the body. The standard of health varies, in different persons, according to age, sex, and original constitution; and in the same person even, from week to week, or from day to day, within certain limits it may shift and librate.

Neither does health necessarily imply the integrity of all the bodily organs: it is not incompatible with great and permanent alterations, nor even with the loss, of parts that are not vital; as of an arm, a leg, or an eye.

If we can form and fix in our minds a clear conception of the state of health, we shall have no difficulty in comprehending what is meant by disease, which consists in some deviation from that state: some uneasy or unnatural sensation of which the patient is aware; some embarrassment of function perceptible by himself, or by others; or some unsafe, though hidden condition of which he may be quite unconscious: some mode, in short, of being, or of action, or of feeling, different from those which are proper to health.

I use the word *disease* generically. Various terms in our language bear nearly the same meaning, and endeavors have been made to appropriate some of these more distinctively. Thus the word *disorder* has sometimes been applied to simple derangements of function, where no alteration of structure is seen, or can reasonably be inferred to exist; while the term *disease* has been restricted to maladies, which are attended with appreciable change of texture, or which run a short and definite course. I see no great utility, but, on the contrary, some risk of confusion, in tying ourselves rigidly down to such distinctions: indeed, we cannot always make them. During life it is often no easy thing to determine whether the parts, of which the functions are disturbed, preserve their integrity of structure or not: and even when the peccant organ is placed before our eyes after death, and the most careful scrutiny fails to discover in it any faultiness of texture, there may still be ground for suspecting that some material change, too subtle for detection by our senses, may have been wrought in its finer and more delicate organization. I shall take care to point out to you, as we go along, the cases in which we can trace organic change, and the cases in which we cannot; but, for the sake of simplicity, I shall call all deviations from the healthy standard, whether of function or of structure, by the generic term *disease*; and to avoid the perpetual and tiresome recurrence of the same word, I shall not scruple to employ the several terms *disorder*, *complaint*, *malady*, *distemper*, *illness*, as its synonyms.

The number of these deviations from the standard of health, (in other words, the whole number of *diseases*,) if we include all their differences in kind and in degree, is scarcely calculable; and the first thing requisite towards investigating the laws that govern their phenomena, is, that we should break them into groups, and dispose them according to some principle of order.

Now, there are various methods in which this first broad classification of diseases might be framed.

The most cursory examination of the animal economy suffices to show that it is made up, not merely of separate parts, but of several distinct systems. There is one set of organs for the mechanical circulation of the blood; there is an apparatus expressly designed for the repeated exposure of the blood to the air; a system for regulating the movements and the feelings of the body; another for receiving, preparing, and appropriating its nourishment; another for the elaboration of matters that are useful or essential to its functions; another for carrying off its impurities, and for removing its superfluous or effete materials; and another for the continuance of the species.

Now each of these systems is liable to changes of structure and interruptions of function, peculiar to itself; and these peculiarities must be taken into account, whatever may be the order adopted in treating of diseases in detail. But I shall not divide the subject, as some have done, into diseases of the circulating system — diseases of the respiratory system — diseases of the nervous system — and so on; for this, among other reasons, that there are many forms of disorder that affect all these systems in common, or simultaneously, and comparatively few that are strictly confined to any one of them.

Neither, in the lectures which I am about to commence, shall I classify diseases according to the several *tissues* of which the animal frame is composed. In speaking of diseases in general, it will, indeed, be both proper and necessary to explain in what manner the same morbid process may be modified by the nature of the special tissue affected. But as the entire body is more or less penetrated and pervaded by the intermixture of several of these tissues, so no useful nor lucid arrangement of diseases could be founded on this basis.

Nor shall I attempt to construct a nosological system by grouping together certain sets of symptoms, and calling each set, in its collective form, a disease.

To say the truth, I shall consider convenience and usefulness, in framing my plan, rather than an appearance of scientific precision; and if I make one principle of arrangement more prominent than another, it will be that which relates to the anatomy of regions, — the place and position of organs. At the same time, I shall not omit to borrow in part from some of those other methods to which I have just been referring.

Before, however, we treat of the *nature* of particular diseases, it will be requisite to give some general account of the different ways in which the various parts of the body are liable to be altered in structure, or disordered in function; and before we speak of the *signs* of particular diseases, it will be proper to take a general view of symptoms, and of their ascertained relations with the several forms of altered structure: for doubtless you are aware that, although diseases are not *constituted* by symptoms, they are, in the living body, *disclosed* by symptoms. Sometimes the symptoms are outward signals which alone reach our senses, and through which internal changes declare themselves; and we then have to decipher and to interpret those signals. Sometimes we *see* the morbid changes themselves on the surface of the body, or in parts within our ken. Some internal changes we can appreciate as surely by the touch, or by the sense of hearing; and of some we infer the existence from alterations in the chemical or in the sensible qualities of the natural excretions.

After death, diseases are often to be traced by visible changes of structure in the internal parts of the body. These changes are extremely interesting, as illustrative of morbid processes: they throw light upon what is past; they afford some guidance for the time to come. But, for obvious reasons, those signs which reveal diseases during life are, practically, of chief moment. In truth, the great object of our art is to prevent or postpone the disclosure of the others. The instruction afforded by the dead body comes too late to be of use in that particular case.

I have already intimated that the morbid physical conditions from which the symptoms flow, are not always to be detected, either before or after dissolution. Neither, when they are detected, is their connexion with the symptoms always evident.

Besides inquiring into the modes in which the various organs and textures of the body may be spoiled, and into the signals or symptoms by which the presence of disease may be ascertained, it will be expedient to premise something, in a general manner, of the *causes* of disease, both with a view to its cure, and, what is much

better, to its prevention. We shall also find it very useful to institute a short inquiry into the different ways in which death may take place—the different processes of dying.

There is one morbid condition or process, to which all parts of the body are liable, and which contributes so largely and so frequently to alterations both of texture and function, that it claims our especial attention when discussing the more general facts and doctrines of pathology: I allude to that change, or series of changes, which we comprehend under the term *inflammation*.

It will be necessary, therefore, in the preliminary part of the course, to give a general account of inflammation; and this account must chiefly be drawn from those of its phenomena which are most familiar to us—which we can see and handle; those which we witness when the disorder is seated in or near the surface, in the skin, in certain of the mucous membranes, or in the subjacent areolar tissue. Then we shall pursue the examination of its peculiar phenomena as they are presented in the other tissues of the body—the mucous, serous, fibrous, parenchymatous, muscular, and nervous tissues; and here the *general* principles of treatment applicable to inflammation may be laid down, with the modifications required according to the tissues interested.

In this part of the course may also be conveniently discussed the modifications of inflammation, and of morbid conditions generally, by the influence of certain *diatheses*, or peculiar dispositions of the body. Some constitutional morbid tendencies we shall find to be innate or hereditary; such are the scrofulous and the cancerous dispositions: others, again, are plainly acquired, as that in which the whole system is tainted for a longer or shorter period by the venereal poison.

Hæmorrhages, also—and serous accumulations, or *dropsies*—as they are liable to occur in all parts of the body, require to be treated of generally, before they pass under our notice in the list of particular maladies. There are certain facts and reasonings common to all inflammations, to all hæmorrhages, to all dropsies. By combining these “generalities” into one comprehensive statement, we help the memory, avoid needless repetitions, and find room for the exposition of principles.

Diseases themselves, in the mass, are sometimes distinguished according as they are *local*, or *general*.

Taking these epithets in their popular sense, we should say that local diseases are those which occupy a definite portion only of the body; general diseases, those which pervade the whole body.

But let us endeavour to obtain clear notions upon these points.

Certainly there are many diseases which, occupying a definite portion only of the body, leave all the remaining parts, and the system at large, healthy both in texture and in function. Such diseases we have no hesitation in calling local.

Again, there are many other diseases which, occupying a definite portion only of the body, yet occasion a manifest and serious disturbance in the functions of various other parts, and (it may perhaps be said) of the whole system. Inflammation of a small portion of the frame may give rise to much secondary or symptomatic fever; but here also we properly speak of the disease as being local; the secondary general disorder resulting from the local and primary, following it in point of time, and subsiding upon its cessation.

But there are still other forms of disease which show themselves, not like inflammation now in this and now in that part, but in many or most parts of the system *at the same time*. I will take the complaint called purpura, characterized by purple spots scattered throughout the body, as an example of what I mean. It is in truth a hæmorrhage affecting many or all the tissues simultaneously. For this reason it is commonly regarded as a general disease.

But if we look somewhat closer into the matter, we shall, I think, perceive that most, if not all, of those which have been thus reputed general, are, in fact, reducible to the class of local diseases. The fluids are as much parts of the body as the solids; and if it be true, as I believe it is, that the essential and primary change in purpura is a change in the blood, its characteristic phenomena will be apt to present themselves wherever there is blood circulating—that is, throughout the whole system. The disease is local, inasmuch as its original seat is in that particular fluid, the blood: it appears to be general, because the morbid blood is everywhere present.

The same observations apply to a large class of febrile contagious diseases ; to that state of the general system which is commonly called anæmia : also to certain spasmodic affections, where the seat of the actual disorder is in the whole nervous system.

What are called general diseases, therefore, are those in which the whole of some one system that pervades the entire body happens to be similarly deranged. Whether diseases can ever be truly said to be general in any more strict or absolute sense than this, is much to be doubted.

I have mentioned dropsy as a malady which, like hæmorrhage or inflammation, may occur in various parts of the body separately. It may also extend at once to all parts capable of receiving and retaining serous effusions : in other words, besides filling the large serous cavities, the effused fluid may occupy the universal areolar tissue. But even this apparently general dropsy will be found, upon careful investigation, to resolve itself, in most cases at least, into local disease within the thorax, or within the abdomen.

The diseases which, in the sense now explained, may be called general, I shall arrange among the diseases of those parts of the system from which they have been ascertained, or may be presumed, to arise.

The first part, then, of the course will embrace an outline of general pathology, with an especial reference to those morbid conditions which fall to the care of the physician. In its relations to surgery and to midwifery, pathology will be more particularly taught by the respective professors of those distinct though kindred departments of medicine. Do not, however, imagine that I take no interest in these, or that there can be anything different in the principles upon which the several branches of pathological knowledge are founded. The truth is, that you cannot, if you would, separate the one from the other. You can neither understand what may be called medical, without learning much which belongs as strictly to surgical pathology ; nor can you be ignorant of either, without being in many important respects deficient in the other also. But the open field of pathology is of wide extent, and although we may, and must, survey the whole, yet its artificial divisions, its inclosures and allotments, will be cultivated best, and most improved, by a division of labour.

Afterwards, separate diseases are to be described and considered : all such, at least, as admit of being individualized, or presented under a definite shape. And here, I repeat, I shall chiefly pursue an anatomical order, as being comprehensive and inartificial, and as tending to facilitate diagnosis. The diseases of parts which lie near each other are the most liable to be confounded.

I shall begin, therefore, with the diseases of the parts that appertain to the head and spinal cord, and then proceed in succession to those of the parts belonging to the neck, the thorax, and the abdomen ; to those of the joints, the muscles, and the skin. I shall not scruple, however, to deviate from this order, whenever, by doing so, I can promote your convenience or advantage.

With that portion of the course which relates to particular diseases, I shall also interweave certain pathological considerations, applicable not so much to the whole body as to the several great systems of which it is made up. Thus, when I come to the brain, I shall speak of the functions peculiar to the *nervous system*, and of the obstructions and disturbances to which those functions are obnoxious, by way of preface to a detailed examination of the various affections of the several parts of that system. Before discussing the diseases of the chest, I shall bring before you, in a general view, the manner in which the great functions of *respiration* and of *circulation* are liable to be impeded, or otherwise disordered. As preparatory to the consideration of the diseases of the abdomen, I shall treat, in the same way, of the functions of *nutrition* ; and of *waste*, which implies an interruption of those functions.

Still there would remain certain diseases, which would not necessarily find a place in this arrangement, inasmuch as their seat is uncertain, or only guessed at. Ague is one of these. Cholera perhaps another. It is quite unimportant whereabouts in the course such maladies are considered. I feel no concern about any imputations of imperfect or clumsy arrangement with which the plan that I propose to adopt may appear chargeable. I had rather *not* be cramped and hampered by attempting what abler heads than mine have failed to achieve, and what, in truth, I believe, in the present state of our science, to be impossible, a complete methodical system of nosology

My object will be to furnish as much instruction and information as I can, in the way that seems most likely to be practically useful to you.

Ague I shall take leave to include among the disorders of the nervous system; and with it, the important subject of malaria will necessarily engage much of our attention.

The great question of contagion I shall consider in connexion with continued fevers, which I rank among that remarkable class of diseases, the contagious exanthemata of Cullen.

Of sympathetic and of hectic fever, I must speak when upon the subject of inflammation.

This, then, is a sketch of the method I propose to follow. In the earlier lectures, with the general pathology, I shall endeavour to lay down principles. To these principles I shall continually refer, as occasions offer, both in those prefatory remarks with which I purpose to introduce the diseases belonging to the several great systems that contribute to form the body; and also in what I shall subsequently have to say concerning those diseases themselves in detail. In this way I hope to combine the advantage of repetition, which was the peculiar advantage of two short courses in a season, with that of greater completeness, which forms the recommendation of a single extended course. The same great advantage of repetition—or I should rather say of recapitulation—will be further aimed at in the stated examinations of the class.

Such being a summary of the topics to be embraced in the ensuing series of lectures, and of the order in which I hope to take up those topics, it seems proper that I should now say a few words in explanation of the scope and objects of the course. The prospectus informs you that it will comprehend the *Principles and Practice of Physic*. What is the true import and promise of these words?

By the *principles* of medicine are meant those general truths and doctrines which have been ascertained and established, slowly indeed, and irregularly, but still with considerable precision, by the continued observation of attentive minds throughout the entire progress of medicine as a science. These principles I profess to teach you. The *practice* of medicine, or the particular application of those general facts and doctrines, I shall *describe* to you; but I cannot profess to *teach* it in this room: nor can you learn it, except in a very imperfect sense, from my description of it. It is the science that I shall here endeavour to unfold. Skill and facility in turning that science to useful purposes I am unable to impart. These are qualities that do not admit of being communicated from one mind to another. The practice of physic, like every other practical art, is to be learned by its repeated exercise; by habit; by carrying its various acts into direct effect again and again; or, if they happen to require no manual dexterity, by looking on, and seeing them done again and again. There is this capital difference, however, between the art of healing and some other arts: that the blunders of early attempts may be both grievous and irremediable—may hurt or spoil the goodly and precious fabric they are intended to repair. There is this also peculiar to our art—that it proceeds upon observations made at the very time when its exercise is wanted; and that it requires *skill in observing* as well as *skill in acting*. You will find, what, perhaps, previously to positive trial, you might not suspect, that the senses—the eye, the ear, the touch—however sharp or delicate they may naturally be, require a special course of training and education before their evidence can be trusted in the investigation of disease. I do not know that these views are capable of being rendered plainer by illustration; for you must have observed a similar distinction between the science and the art in various other branches of human knowledge. The principles of navigation may be thoroughly comprehended by a person who scarcely knows a rudder from a cable, and who would not be trusted, nay, who would not trust himself, with the conduct of the simplest boat. A man may master the beautiful science of astronomy—may acquire the power of working upon paper its sublimest and most abstruse problems—and yet remain in complete ignorance of the method of adjusting and using a telescope, and unable to ascertain for himself the position or the movements of a single star. But place such a person night after night in an observatory—let him notice and imitate the proceedings of some one already skilled in examining the phenomena of the heavens—and he will soon acquire the requisite tact and facility himself. Just so it is with that branch of knowledge with which we are concerned. It is in the wards of a hospital, or in

the domestic chamber — it is among the sick and the dying — and there alone — that you can either thoroughly or safely learn to practise physic.

In what, then, you may fairly ask, consists the value or the use of lectures on the practice of physic, if the practice of physic cannot be taught by lectures?

The main object of systematic lectures, explanatory of the principles, and descriptive of the practice of medicine, is to prepare the hearer for observing to the best advantage the actual phenomena of disease and the power of remedies over it. They are intended to fit him for seeing with intelligence—to enable him to read, and understand, and interpret, the book of nature when it is laid open before him—in short, to qualify him for clinical study. One man shall travel into a foreign land, knowing nothing beforehand of its scenery or its climate, of its natural productions, its manufactures, or its works of art, and ignorant alike of the manners, customs, history, laws, and language of its inhabitants. Another shall visit it after having furnished his mind with information on these subjects by reading, and by conversing with men who have already passed over the same ground. Supposing the visit to be limited in each case to a certain, but not long period of time,—I need not ask your opinion as to which of these travellers will reap the greatest harvest of enjoyment and of profitable knowledge from his tour. Not less striking is the difference, in point of instruction and of interest, perceived by different students, upon their admission to the bedsides of the sick, according as they have been well or ill prepared for the multifarious spectacle of bodily suffering then first displayed before them. There are persons, indeed, who seriously, and I make no doubt in perfect good faith, warn the student against bringing to the contemplation of disease any preconceived opinions; who tell him that he must come with a free and unprejudiced mind, and see, and note, and judge of all things for himself. I also would have him exercise, and ultimately abide by, his own judgment; but surely if every man were to depend upon his own unassisted observation for his knowledge of disease, every man would be marvellously ignorant, and the science of medicine would stand still, or cease to be. “If no use be made (says Dr. Samuel Johnson) of the labors of past ages, the world must remain always in the infancy of knowledge.” In truth, a person who, without any previous information concerning diseases, should betake himself to a hospital with the design of impartially and resolutely investigating their phenomena, such a person, however clear and strong his intellect might be, would find himself, for a long time, more puzzled than instructed by what he saw around him. He would be perplexed by the shifting and seemingly contradictory characters presented by the same malady in different patients: or in the same patient at different times: and not less so by the outward resemblance of disorders essentially unlike. He could not but be confused by the multitude of symptoms that crowded upon his attention on every side; and at a loss to distinguish important facts from those which, for the chief ends of his pursuit, were trivial, or useless.

The business, therefore, of a lecturer upon the *Principles and Practice of Medicine*, or, as it is sometimes worded, *the Nature and Treatment of Diseases*, is first to fix upon some order in which to treat of the various subjects comprised in his course. The simpler and less artificial his arrangement, the better. The chief use of this classification is to facilitate the recollection of particular facts; and I have already told you that if I can distribute and connect the multifarious forms of disease in such a manner as that they shall appear plain to your understanding, and take a secure hold upon your memory, I shall not trouble myself nor you with a vain search after that phantom—a perfect methodical nosology. “In all such classifications,” writes Lord Brougham, “we should be guided by views of convenience rather than by any desire to attain perfect symmetry; and that arrangement may be best suited to a particular purpose which plants the same things in one order, and separates them and unites them in one way, when an arrangement which should dispose those things differently might be preferable, if we had another purpose to serve.”

Having settled this framework of his discourses, the next aim of the lecturer must be to collect and arrange from the voluminous and bewildering records of medicine, and from the necessarily more slender stores of his personal experience, whatever it may seem of consequence that his hearers should know concerning each distinct form of disease, as it comes before them for consideration: to state all the facts which are well ascertained, and which tend to explain its symptoms, to elucidate its origin, to

identify its nature, to direct its treatment, to accomplish its prevention : to sift the true facts from the false, the important from the trivial, the essential from the accidental : to analyse the relations of these facts, and ascending from particulars to generals, to point out those great principles and precepts which constitute the keys, both to the knowledge and to the management of all diseases of the same kind. It may even sometimes be his duty to notice and discuss mere theoretical opinions ; to express his own sentiments upon disputed or undecided questions ; and to admonish his audience against the danger of being led away by ingenious refinements, by the speciousness of novelty, or the boldness of speculation, from the more secure and settled results of careful observation improved by patient thought.

These duties of a lecturer on medicine are metaphorically, but aptly, expressed in the following passage from Lord Bacon : —

“Formica colligit, et utitur, ut faciunt empirici; aranea ex se fila educit, neque a particularibus materiam petit, ita faciunt medici speculativi ac mere sophistici; apis denique cæteris se melius gerit. Hæc indigesta e floribus mella colligit, deinde in viscerum cellulis concocta maturat, iisdem tamdiu insudat, donec ad integram perfectionem perduxerit.”

I may venture to paraphrase it thus : —

The lecturer must not be the ant, collecting all things indiscriminately from all quarters, as provender for his discourses ;

Nor the spider, seeking no materials abroad, but spinning his web of speculative doctrine from within himself ;

But rather the bee, extracting crude honey from various flowers, storing it up in the recesses of his brain, and submitting it to the operation of his internal faculties, until it be matured, and ready for use.

Such, gentlemen, are the main objects which I shall endeavour to keep steadily in view during the series of lectures I am about to commence ; and I should ill deserve the chair I have the honour to occupy, if I did not feel the great responsibility under which I speak to you. The subjects with which we have to deal are not matters of mere speculative curiosity or intellectual amusement—to be taken up to-day and dismissed perhaps with unconcern to-morrow—but they involve questions of life and death. The opinions you are now to form or to embrace, are for the most part the opinions upon which in after life you will confidently and constantly be acting. The comfort or the misery of many families may probably hang upon the notions that each of you will carry from this place. Therefore it is that I feel myself to be engaged in a very serious undertaking. Doctrines and maxims, good or bad, flow abroad from a public teacher as from a fountain, and his faulty lessons may become the indirect source of incalculable mischief and suffering to hundreds who have never even heard his name. These reflections fill my mind with an almost painful sense of the obligation imposed upon me, by my present office, of closely sifting the facts, and of carefully examining the principles to be derived from those facts, which I propose to employ for your instruction and guidance.

But amid all the responsibilities, gentlemen, both of teacher and of learner, the profession which you and I have chosen, or which circumstances have prescribed to us, is a noble profession, and worthy the devotion of a life-time. If you fit yourselves now for its high functions, and pursue it hereafter in earnestness and truth, it will probably conduct you to an honourable competence, and it will assuredly prove a salutary school of mental and of moral discipline. Trials, no doubt, belong to it, and difficulties ; but it has also privileges and immunities peculiar to itself. Affording ample scope and exercise for the intellect, it is conversant with objects that tend to elevate the thoughts, to temper the feelings, and to touch the heart. I have already reminded you how it brings beneath our minute and daily notice that most remarkable portion of matter, which is destined to be for a season the tabernacle of the human spirit, and which, apart from that singularly interesting thought, excites increasing wonder and admiration the more closely we investigate its marvellous construction. The sad varieties of human pain and weakness with which our daily vocation is familiar, should rebuke our pride, while they quicken our charity. To us are entrusted, in more than ordinary measure, opportunities of doing good to our afflicted fellow-creatures—of showing love towards our neighbour. Let us beware how we idly

neglect, or selfishly abuse, a stewardship so precious, yet so weighty. The profession of medicine, having for its end the common good of mankind, knows nothing of national enmities, of political strife, of sectarian divisions. Disease and pain the sole conditions of its ministry, it is disquieted by no misgivings concerning the justice or honesty of its clients' cause; but dispenses its peculiar benefits, without stint or scruple, to men of every country, and party, and rank, and religion, and to men of no religion at all. And like the quality of mercy, of which it is the favourite hand-maid, "it blesseth him that gives and him that takes;" reading continually to our own hearts and understandings the most impressive lessons, the most solemn warnings. It is ours to know in how many instances, forming indeed a vast majority of the whole, bodily suffering and sickness are the natural fruits of evil courses; of the sins of our fathers, of our own unbridled passions, of the malevolent spirit of others. We see, too, the uses of these judgments, which are mercifully designed to recall men from the strong allurements of sense, and the slumber of temporal prosperity: teaching that it is good for us to be sometimes afflicted. Familiar with death in its manifold shapes, witnessing from day to day its sudden stroke, its slow but open siege, its secret and insidious approaches, we are not permitted to be unmindful that our own stay also is brief and uncertain, our opportunities fleeting, and our time, even when longest, very short, if measured by our moral wants, and intellectual cravings.

Surely, gentlemen, you will not dare, without adequate and earnest preparation, to embark in a calling such as this; so capable of good if rightly used, so full of peril to yourselves and to society if administered ignorantly or unfaithfully. And even when you have made it, as you may, the means of continual self-improvement, and the channel of health and of ease to those around you, let not the influence you will thus obtain beget an unbecoming spirit of presumption; but remember that, in your most successful efforts, you are but the honored instruments of a superior power—that, after all, "It is God who healeth our diseases, and redeemeth our life from destruction."

LECTURE II.

Pathology—meaning of the term. Pathology, general and special. Morbid alterations of the solid parts of the body. Alterations in bulk. Hypertrophy—laws of its production—its effects. Atrophy—its causes and consequences. Changes in form. Alterations in consistence. Induration—its various kinds.

I PROPOSE to devote several lectures, in the commencement of the course, to pathology, as it relates to medicine.

And I must first of all explain to you what I mean by the term pathology.

Many persons speak of pathology as if it were the same thing with morbid anatomy. That is not the sense in which I purpose to use the term. Pathology is morbid anatomy, but it is something more.

A knowledge of pathology (in the full and proper acceptance of the word) implies indeed a knowledge of altered structures and of diseased conditions;—but it implies also an explanation of these—a knowledge of what precedes them, and a knowledge of what results from them.

It comprehends therefore the following particulars:—1. A knowledge of the material changes to which the several parts of the living body are subject in disease: 2. A knowledge of the processes or actions whereby these changes may be wrought: 3. A knowledge of the causes which may set these processes on foot: and 4. A knowledge of the consequences of the same changes, or of the symptoms they occasion.

On some of these points our actual knowledge is still scanty and imperfect. Yet a good deal of valuable information has been collected concerning each of them, and

this I shall endeavour to place before you as distinctly, and at the same time in as small a compass, as I can.

Pathology is general or special. General pathology treats of the morbid conditions which are common to the entire system, or to the whole of each of the several tissues that pervade and compose the system. Special pathology contemplates particular diseases. An acquaintance with general pathology prepares us for, and conducts us to, that which is special: and when I say that the earlier lectures of the course will be given to a consideration of the leading facts and doctrines of pathology, you will of course understand me to speak of *general* pathology.

I shall begin by inquiring what are the morbid changes to which the component parts of the living frame are liable: and I speak chiefly of sensible changes; leaving unnoticed for the present those unnatural conditions which are perceptible only through the microscope.

There are, then, various ways, capable of intelligible description, in which the different parts of the body may be sensibly altered by disease.

The solid parts may be altered in *bulk*; in *form*; in *consistence*; in *their intimate texture*, i. e., in the qualities and arrangement of their component particles; and in *situation*.

The fluid parts may also be altered in *quantity*; in *quality*; and in *place*.

And many of these alterations may exist in combination with each other.

Let us first consider the solids.

They may be simply altered in bulk without any change of texture; and that in two ways. They may become larger than is natural, or smaller than is natural. In the one case the change is called *hypertrophy*, and in the other *atrophy*.

We find the best illustrations of hypertrophy in the muscular system. The huge fleshy masses visibly prominent in the arm of a blacksmith or a pugilist, and in the leg of an opera dancer, afford familiar examples of it. In these cases the increased bulk, although it may be unsightly, as being out of proportion to other parts, is not disease, and does not interfere with the most perfect health. By constant exercise the muscles acquire preternatural volume, and weight, and power. It seems to be a law which prevails extensively in the animal economy, that increase of function should lead to augmentation of bulk. The function of the muscular system is contraction, and more frequent and energetic contraction begets an addition of substance. But the same principle obtains in various other parts and tissues. It is especially noticeable in some of the organs that are double. If one kidney wastes, or is spoiled by disease, an increase of function devolves upon the other, and by a beautiful law of compensation, the sound organ, without any alteration of its peculiar fabric, enlarges. The same is observed to be the case with the lungs. The law resembles, somewhat, one that is familiar to political economists, and is expressed by them in the maxim — that the supply of a marketable commodity is regulated by the demand for it. If, in respect to a muscle, increase of force be habitually needed, the necessity generates the requisite addition of bulk, which implies an augmentation of force. One kidney becoming inefficient, it is necessary that the other should secrete a larger quantity of urine; and this faculty is obtained by the enlargement of the secreting organ.

I say this law is of extensive operation in the living body: but it is not universal. It does not hold, for instance, in respect to the organs of the special senses. One eye does not become hypertrophic when the other is blind; nor one ear grow larger or longer because the other is deaf. And we see at once why the law in question does not apply in such cases. These organs differ from such as I mentioned before — from muscular and glandular parts — in this: that increase of their size would not promote or facilitate the purpose they are designed to serve. A muscular arm will strike a harder blow, and lift a heavier weight, in proportion to the greater bulk of its muscles: but we should gain nothing in distance or in distinctness of vision by the enlargement of an eye; nor should we hear more acutely or more clearly if our ears were of twice the ordinary magnitude.

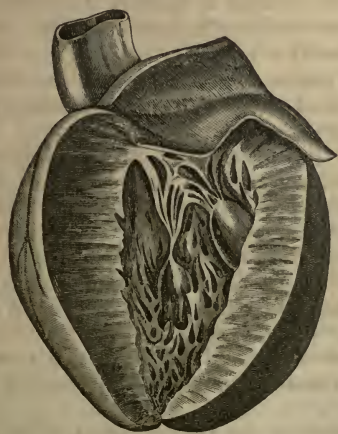
Hypertrophy of this unmixed kind — unattended by any change of texture — (and it is to this that the term should as much as possible be restricted,) is believed to depend upon more active nutrition of the part (*νπερ τροφή*). More materials are given up to the part by the blood, and assimilated, than are received back from the part into the blood to be taken out of the body. The nutrition exceeds the waste. That

hypertrophy does thus result from an excess in the process by which parts are nourished and built up, and not from a defect in the process by which they are continually unmade and removed, is rendered probable by the fact that an increased quantity of nutrient blood is sent to the part hypertrophied. Its arteries grow larger. This we perceive by comparing these vessels with others where no accension of bulk has occurred. This opinion is further strengthened by the converse effect produced upon an hypertrophied part, the thyroid gland for instance, by tying its principal nutrient artery. The magnitude of the bronchocele diminishes. It is curious that it should still be a matter of debate among pathologists, whether the nerves of the part partake also in its enlargement.

Now these examples of hypertrophy clearly have not the nature of disease. But hypertrophy is often plainly connected with disease, while still it is not itself a morbid process. Thus we have it in the hollow contractile organs, the office of which is to propel fluids:—in the heart when the progress of the blood suffers some mechanical impediment; in the bladder and in the intestinal canal, when their respective contents are somehow hindered in their natural course; or when, from some undue stimulus or irritation, these parts are urged for a long time together to excessive, or too frequent, action. I show you preserved specimens of each of these changes. You will find that muscular tissue may become apparent, under the influence of disease, where very slight traces of it, or none at all, were visible before. We sometimes observe this in the air tubes, the trachea and bronchi, when the respiratory functions have been long embarrassed; and in the gall-bladder, when the exit of the bile has been chronically obstructed. And it is worth remarking, that this new, or greatly exaggerated appearance of muscular tissue, which is the consequence of disease in the human body, is analogous with the natural and healthy structure of the corresponding organ in some of the inferior animals.

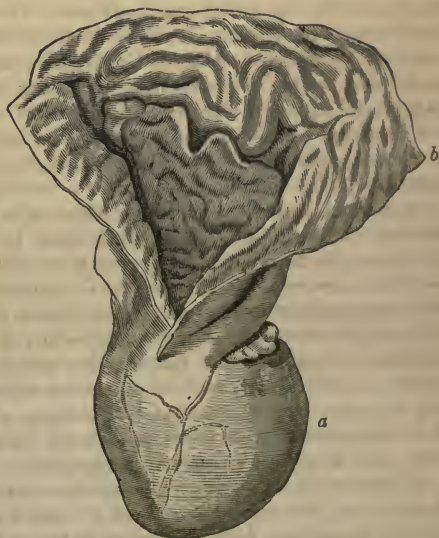
The several instances of hypertrophy that I have now been mentioning, if they are to be looked upon as morbid, are morbid in a particular and limited sense—morbid, merely as being associated with disease, but not so either in their own processes or in their tendencies. Many indeed of the writers who notice them, speak of the hypertrophy as constituting a source of disease, and a cause of danger to the patient. But I shall have occasion to show you hereafter, that in most cases it is really a compensatory change, and conservative of life;—a resource of nature by which impending danger is postponed, and existence prolonged.

FIG. 1.



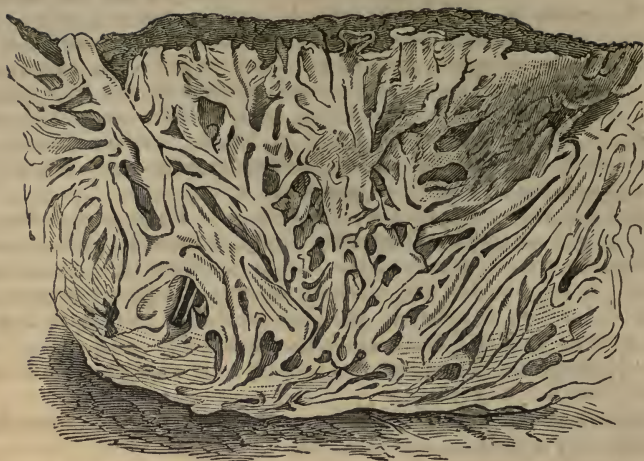
Hypertrophy of left ventricle of the heart.
From Dr. Gross' collection.

FIG. 2.



Hypertrophy of the Aorta. From Dr. Gross' collection. *a* the heart. *b* the aorta.

FIG. 3.



Hypertrophy of the muscular fibres of the urinary bladder. From Dr. Gross' collection.

It may be said of hypertrophy, that its relation to disease depends very much upon its seat. As regards the muscular system—in the voluntary muscles it is generally innocent, in the involuntary it is generally connected with disease; sometimes as a cause, much oftener as a remedial consequence, sometimes as both cause and consequence. One way in which hypertrophy may manifestly be a cause of disease is by the pressure of an enlarged organ upon the parts in its neighborhood, and a consequent interference with the functions or the sensations of those parts.

I am not sure whether, to those among you who are beginners, I make myself understood. An example or two will render my meaning obvious.

It often happens that the aortic orifice of the left ventricle of the heart becomes narrow and constricted, in consequence of disease in the semilunar valves there situate. Under these circumstances, it is requisite, for the due propulsion of the obstructed blood, that the ventricle should contract with increased force. Its walls accordingly become thicker and stronger. Here the hypertrophy of the left chamber is evidently a consequence or *effect* of the disease that previously existed at its outlet.

On the other hand, when the thyroid gland is enlarged, it sometimes presses so much upon the parts that lie behind it, as to impede the breathing, or the swallowing. In this case, the hypertrophy is the *cause* of consecutive disease.

Hypertrophy is exceedingly common in other tissues as well as in the muscular. Of its affecting the glandular system we have good examples in what I have just mentioned, the true bronchocele; in certain forms of enlarged prostate; in the thymus gland not unfrequently. Of a state of the brain which is considered to constitute hypertrophy, I shall speak more particularly when we come to the morbid conditions of that organ. Hypertrophy is also said (I am not certain with how much propriety always) to occur in the cutaneous, mucous, and vascular systems, in the bronchial, mesenteric, and mammary glands, in the liver, spleen, and pancreas. Of these parts I suspect that the enlargements to which the term hypertrophy has been sometimes applied, most frequently combine some alteration of texture with the increase of size, and therefore are not examples of pure hypertrophy.

You ought to be aware that hypertrophy of one or more of the component tissues of an organ may exist, while the others either remain unaltered, or are changed in some other way. It frequently happens that when one component part is thus over-nourished, it is so at the expense (as it would seem) of another which becomes atrophied. There are parts of the heart upon which a certain quantity of fat is usually deposited. It is not uncommon to meet with this fat in excess, and at the same time to find the muscular texture of that organ pale, flabby, soft, and wasted. What has been deemed hypertrophy of the female breast consists, almost always I

believe, in excessive development of its adipous tissue, without any enlargement of the gland itself — or even with its diminution.

Hypertrophy of the adipous tissue is often general throughout the body, producing obesity; and this may become so extreme as to amount to disease, when it is called by nosologists *polysarcia*. I have seen one fatal instance of this kind: perhaps two. The mother of a large family, whom I long knew as a slender and elegant woman, began suddenly to grow fat; and in about fifteen months, without any other discoverable malady, she gradually enlarged into a corpulent unwieldy monster. At length her legs and thighs became œdematous as well as fat, her lips blue, her breath was short, and her pulse feeble. One night she was found dead in her bed. The body was not examined; but her death was mainly owing, as I believe, to fat collected upon the heart, oppressing its movements, and at last stopping them altogether.

In the majority of cases the size of an hypertrophied organ is augmented; it has a larger superficies than is natural: and therefore I have introduced hypertrophy to your notice among the alterations to which parts are liable in *bulk*.

But it is not always so. There may be hypertrophy of an organ without enlargement — in at least three different ways: —

1st, In hollow organs, where the additional substance is deposited centrally, and the hypertrophy takes place at the expense of the cavity:

2dly, In any organ, whereof the hypertrophy is confined to one or more tissues, while the others are proportionably wasted: and,

3dly, Hypertrophy may even be consistent with no alteration of shape, or increase of bulk in any direction, the organ occupying exactly the same space and preserving the same absolute dimensions as before, but becoming more full of component particles, more compact, heavier. This state is well exemplified in certain cases of hypertrophy of bone; the spongy or cancellous texture of the bone disappears; its specific gravity is increased; it becomes hard, firm, and like ivory. The structure appears, to the eye, to be changed, yet remains the same, except in respect of its density.

I have told you that hypertrophy is usually a conservative and salutary change. We shall meet with many illustrations of this as we proceed. But I may take the present occasion for pointing out to you some of the beneficial tendencies of this change when it takes place in bone. For, since the diseases of the bones do not belong to my province, I may have no other opportunity.

You probably know that in the disorder called rickets, occurring principally during childhood, the bones are soft, and deficient in their more solid ingredient; so that they bend under the weight of the body, or the contraction of the muscles attached to them. After a certain period this disproportion in the constituent particles of the osseous tissue ceases; but the bones are permanently distorted, and, therefore, less adapted to their office, and less strong, than if they had remained straight. Now the natural remedy that ensues is very striking and beautiful. The bent bones become *hypertrophied* in certain places; they grow thicker, denser, harder, and consequently stronger, at the very concave part where the stress of the pressure is the greatest.

The following experiment showed the same thing in a somewhat different manner. An inch of the middle part of the fibula of a quadruped was cut out. A long time afterwards the animal was killed. The tibia was then found to have become considerably larger exactly in that part of it which corresponded to the defect in the fibula.¹

The same principle appears still more conspicuously in a case of disease related by Cruveilhier. He saw in the hospital at Limoges a young man who had lost (from necrosis with suppuration) the middle third of his tibia; of the larger of the two bones of the leg. The lost bone had not been reproduced, but the fibula, the naturally slender bone, had become thick and strong enough to support the whole weight of his body.

I was explaining to you that hypertrophy may exist, without enlargement. On the other hand there may be enlargement, without any change of structure, and yet no hypertrophy. The liver and spleen are apt to acquire a considerable increase of bulk from mere congestion and distension of their vessels by blood. An immense spleen will shrink into its proper size in a few hours, after hæmorrhage from the stomach, whereby the gorged venous system of the abdomen has been relieved. Dr. Townshend

¹ Mr. Stanley's Lectures, Coll. Surg.

mentions a remarkable example of the same kind respecting the liver. The inferior cava had been compressed by an aneurismal tumour, so that the passage of blood from the liver was greatly impeded. Under these circumstances the liver became so large as nearly to reach the crest of the ilium. Suddenly the aneurism burst, the pressure was taken from the cava, the hepatic veins were allowed to empty themselves, and before the body was opened for inspection, the liver had nearly resumed its natural situation and dimensions.

In the profound, yet clear and instructive views of this subject exhibited by Mr. Paget in his recent lectures (1847) before the College of Surgeons, the conditions which give rise to hypertrophy are stated to be chiefly, or only, three, namely :

"1. The increased exercise of a part in its healthy functions.

"2. An increased accumulation, in the blood, of the particular materials which a part appropriates in its nutrition, or in secretion.

"3. An increased afflux of healthy blood."

In the hypertrophy of the muscular tissue the first and third of these conditions coincide. The more frequent and vigorous contractions of the muscle accelerate the passage of the blood through its vessels, and so augment the quantity which flows towards and into them in a given time. The enlargement of the nutrient arteries is secondary to the hypertrophy; and in turn contributes to sustain and augment it.

But the increased afflux of blood may be primary. Of this Mr. Paget adduces instances, in the growth of rank hairs around the edges of sores which have continued long inflamed, and about old diseased joints; in the rapid increase of the spur of a cock when transplanted from the bird's leg to its comb; and (probably) in certain cases of congenital or spontaneous hypertrophy of a single member, of a hand or a foot, or of one or more fingers.

When one kidney augments in size upon the destruction or inaction of the other, we have coincidence of the first and second conditions. Mr. Paget thus explains the process. "The principal constituents of the urine are, we know, ready formed in the blood, and are separated through the kidneys by the development, growth, and discharge of the renal cells, in which they are for a time incorporated. Now when one kidney is destroyed, there must, for a time, be an excess of the constituents of the urine in the blood; for since the separation of the urine is not mere filtration, the other kidney cannot at once, and without change of size, discharge a double quantity. What then happens? The kidney grows, more renal cells develop, and discharge, and renew themselves. In other words, the existence of the constituents of the urine in the blood that is carried to every part, determines the formation of the appropriate renal organs in the one appropriate part of the body."

In the same manner the increased formation of adipous tissue may be ascribed to the presence of abundant hydro-carbon principles in the blood, which are the chief elements of fat.

A few isolated facts, bearing upon some points connected with this inquiry, may be worth mentioning.

In the first place, certain localities appear to be influential in the production of certain forms of hypertrophy. Thus bronchocele is very frequent among the inhabitants of certain districts: especially in close or marshy valleys at the feet of high mountains. Its real cause is to be sought in some condition, hitherto undetermined, of the air in those places, or more probably of the water, or of both.

2ndly, Certain congenital or acquired conditions of the system, tend to produce local hypertrophy. In that peculiar diathesis which we call the strumous—and of which I shall have much to say hereafter—certain parts of the body, as the upper lip, and the extremities of the long bones, undergo a kind and degree of enlargement that seems properly to fall within the definition of hypertrophy.

3dly, Certain habits of life have a distinct effect in promoting certain forms of hypertrophy. A full diet, with bodily inactivity, leads to hypertrophy of the adipous tissue. So general is this tendency, that we confidently act upon it in the fattening of animals. Shut a healthy pig up in a small sty, and give him as much food as he is willing to eat, and you ensure his rapid pinguessence. If you cannot so certainly attain the same result by similar means in the human animal, it is chiefly, I believe, because moral causes, and especially mental anxiety, will effectually counteract those

means. A healthy man, with a quiet mind, using habitually a full nutritious diet, and leading a sedentary life, will fatten, I apprehend, as unfailingly as a calf or a turkey. Sometimes, indeed, fat accumulates, to an enormous extent, in spite of abstinent habits, and very active exercise.

4thly, It is a curious fact that the removal of certain parts of the body, as the testicles from male animals, and the ovaries from females, increases the disposition to accumulate fat. The same tendency appears to be given, for a time, by the extirpation of the spleen.

Of the curative methods that hypertrophy may require it would be premature to speak at present.

The *bulk* of parts may be also *augmented* in various other ways. The hollow organs may be inordinately distended by an undue accumulation of their natural contents; or by matters that do not enter them in health. The solid organs may have their size increased by the presence of matter foreign to their natural composition, collected in their interior, or distributed through the interstices of their proper tissues, or deposited upon their surface: and in either case the functions of the part itself may be disturbed or suspended; or the functions of parts immediately contiguous to it may sustain damage from its pressure; or the functions of distant parts connected with it by dependency of office may be disordered; or all these consequences may ensue together. Numerous examples of them all will hereafter be brought under your notice.

Let us next attend to that condition which is the opposite of hypertrophy — to *atrophy*, namely, in which parts become notably smaller than natural, without other alteration of texture.

The two conditions contrast strongly with each other in their nature and origin, as well as in their physical character.

Hypertrophy depends essentially upon an increase — atrophy upon a diminution or defect, of the nutritive functions. You will find that atrophy plays an important part in altering the bodily organs, both in health and in disease.

Of the effect of atrophy in causing alterations consistent with health, I shall merely remind you of some instances, that you may the better comprehend its morbid operation.

There are parts of the body, as you well know, destined for a temporary purpose only. Upon the cessation of their especial function they dwindle, or disappear. We have examples of this in the thymus gland, and in those parts of the mechanism of the circulation which are peculiar to the foetal state. The atrophy here begins as soon as the child is born, and is not only consistent with, but necessary to, its perfect health. As life advances, we see the same principle at work, remodelling from time to time those structures of which the office has only a limited duration. After the child-bearing period in women is over, when the functions of the ovaries expire, these organs shrink, through atrophy. It is so with the testes of old men. Indeed, atrophy, to a certain extent, pervades all parts of the system in old age: the muscles diminish in size, the whole body is less plump, the bones lose a portion of their substance, and become brittle.

Even in the period of foetal life this process, by which parts are starved and stunted, sometimes displays itself. But here it is no longer compatible with the integrity and well-being of the system. The arrest or retardation of the nutritive function produces changes of great interest, and gives rise to various kinds of monstrosity. Harelip — fissure of the palate — certain malformations of the heart — are familiar examples of the consequences of intra-uterine atrophy.

Atrophy, considered as a morbid change, is conspicuous, no less than hypertrophy, in the muscular system. We see it in the voluntary muscles, whenever a limb remains long in a state of inaction — whether from palsy depending upon disease in the brain or spinal cord; or from pain connected with disease of a joint; or from perversion of the will, as in the self-inflicted penance of the Fakir. The same law, therefore, obtains here, which was previously announced; the development of a part is proportioned to the activity of its function. In most cases, I believe, the atrophy will be found to resolve itself into a deficient supply of healthy arterial blood. Building materials are not provided, or are provided inadequately. Mere inaction will produce atrophy; but it is probable that the inaction operates simply by abridging

the flow of arterial blood to the muscle. If (as some contend, and as I am disposed to believe) what is called a *change in the innervation* of a part tends sometimes to occasion its atrophy; if, for example, the altered state of the nervous influence has some share, beyond the inaction which it produces, in causing the atrophy of a paralysed limb—it still acts, I conceive, indirectly, and by reducing somehow the supply of healthy arterial blood. The nerves belonging to palsied and atrophied muscles are found to diminish in size. It is with the arterial circulation, however, that atrophy is most concerned. It is upon a diminution of the number of the smaller, and perhaps also of the capacity of the larger arteries, that senile atrophy often depends. We find atrophy of the brain accompanying certain diseased conditions of its main arteries. So the testicle withers when the spermatic artery is tied for the cure of varicocele.

Take notice how the laws of atrophy and of hypertrophy tally also in their exemptions, and are alike inapplicable to the organs of the special senses. I showed you that, although a kidney grows larger when the function of its fellow gland is lost, it is not so with an eye. Neither does the eye dwindle under mere disuse. Of this we have a remarkable illustration, as Mr. Simon has pointed out, in the boy who, born blind, was couched at the age of fourteen by Cheselden. His organs of vision were perfect in function and in bulk, after the almost total suspension of their office for so many years.

Pressure of any kind, permanently exercised either upon the large arterial trunks, or upon the capillary vessels, so as to lessen without completely preventing the supply of blood, will be found to give rise to atrophy, whenever the due quantity of blood is not furnished by the establishment of a collateral circulation. I say *permanently* exercised, because intermittent pressure has often the exactly contrary effect. It was a maxim of Mr. Hunter's, that pressure from without produces thickening; pressure from within thinning and absorption of parts. Of the former we see an example in the thickening or hypertrophy (*conservative* hypertrophy) of the cuticle on the soles of the feet in persons who walk much, and on the palms of the hands of those who labour with tools. But Mr. Paget has superseded this principle by one of wider extent and of more exact application. He has shown that it is not upon the direction of the pressure that its different results depend; but upon the circumstance of its being constant, or only occasional, whatever may be its direction. "All the thickenings of the cuticle are the consequences of occasional pressure—as the pressure of shoes in occasional walking, tools occasionally used with the hand, and the like; for it seems a necessary condition for hypertrophy, in most parts, that they should enjoy intervals in which their nutrition may go on actively. But constant pressure, whether from within or from without, always appears to produce absorption." He does justice to Mr. Hunter's sagacity, however, by remarking, that "nearly all pressures from without are occasional and intermittent, and nearly all pressures from within, arising as they do from the growth of tumours, the enlargement of abscesses, and the like, are constant."

Chronic inflammation is sometimes attended by the wasting of the part which it occupies. It acts, in all probability, by unfitting the capillary arteries for transmitting the requisite quantity of blood. Various diseases, by which the supply of nutriment to all parts of the body is checked at its source in the digestive organs, or by which some unnatural drain upon the system is kept up—by which, in short, the quantity of the nutrient fluid is diminished, or its quality impaired—produce a greater or less degree of *general* atrophy; but to this universal wasting we usually apply the term *emaciation*.

Atrophy, then, such at least as is morbid in its nature, may be the consequence of inaction, of abiding compression, of chronic inflammation, and of various diseases; but in all cases the defect of nutrition which constitutes the atrophy seems to be resolvable into a diminished supply of healthy blood through the arteries.

As in hypertrophy, so likewise in atrophy, the change may be limited to some one or more of the component tissues of a part:—and by these altered proportions of its constituent tissues the appearance of the part may be remarkably modified.

So, also, as hypertrophy may exist without any increase of absolute size, atrophy may occur without any decrease: as in the heart, when the cavities are dilated in the exact degree in which their walls become thinner. Bones, externally sound in ap-

pearance, have had their specific gravity so greatly reduced by internal atrophy, that they would float, like a cork, upon water.

It is a curious fact—which I mentioned in other terms before—that an atrophied part is sometimes plentifully encompassed by fat. But this is by no means a necessary accompaniment. Why it happens in one case, and not in another—whether the adipous hypertrophy is ever the cause of the atrophy associated with it, or the atrophy the cause of the hypertrophy:—these are questions which, in the present state of the science of medicine, do not admit of any positive solution.

It is scarcely necessary to observe that the changes of *bulk* which we have been considering, imply often, though not always, changes of *form* also. You may have one or two of the chambers of the heart greatly enlarged, while the others remain of their natural size. Of course this altered proportion modifies the shape of the organ.

Signal changes of form are produced also by inflammation, by pressure, and in various other ways. But, after all, modifications of figure are rather to be considered as *accidents* of disease than among its important *elements*; and I pass on to other alterations.

Various parts of the body are liable to be changed in *consistence*. They may become harder and firmer than before: or they may become softer. To the state of increased or unnatural hardness the term *induration* has been applied; the same word is used also to express the process of hardening. To the state of diminished consistence we give the name of *softening*. The French pathologists, who first noticed this condition as an element of disease, call it *ramollissement*.

You are already aware—those of you who have attended the lectures of the professors of midwifery and of anatomy—that a slow process of natural and healthy induration is going on throughout the body from the earliest period of uterine life to extreme old age.

There are several ways in which *unnatural* induration may take place.

Induration of an organ may happen, without any other alteration of its proper tissue, in consequence of inordinate fullness of its blood-vessels. This is apt to occur in the lungs, or liver, whenever the free exit of blood from these organs is in any way impeded. They become stretched, tense, resisting, hard.

In like manner induration of the hollow organs, or of cellular parts, may arise (without any change of their texture) from an undue accumulation of fluids within them:—of bile, for example, in the gall-bladder; of urine, in its receptacle; of gases in the stomach and intestines; of serosity in the cellular tissue.

In either of these kinds of induration the unnatural hardness may be temporary only, or it may be the permanent accompaniment of other disease. It is necessary that you should be aware of its occurrence, and of its nature. I say, of its *nature*, because this is not always understood. In the induration arising from the last circumstance I mentioned, viz., from infiltration of the cellular tissue with the serous or albuminous parts of the blood—from *œdema*, in short—the hardness has sometimes been erroneously ascribed to some other morbid condition. Dr. Carswell has shown that in the curious disease of new-born children who are said to be *skin-bound*, the hardness of the surface is the consequence of simple *œdema* of the subcutaneous cellular tissue. The same phenomenon is remarkable in *œdema* of the tongue. I believe the induration belonging to *œdema* will be found to be the greater, in proportion as the effusion is large and recent, and has taken place rapidly.

Again, induration may accompany, and be a consequence of, simple hypertrophy. Of this I have already shown you examples: especially in the eburnation, (as it has been called) of hypertrophied bone.

Induration of an organ may also result from the expression of its fluid, and the compression of its solid parts. We see this extremely well in the lung, when it has been thrust and flattened against the vertebral column by fluid effused into the pleura;

FIG. 4.



Atrophy of cellular structure of the thigh-bone.

or when it is still more tightly bound down by an investing layer of plastic lymph. In this way, therefore, induration may be consistent with atrophy. That the natural structure of the hardened lung is not always lost in these cases we know, because we can restore, to a certain extent at least, its bulk and spongy feel, by forcibly inflating it. The spleen sometimes exhibits the same kind of induration, under the constrictive force of an investing false membrane. I am mentioning samples only of these changes.

More frequently induration depends upon the presence, in the internal texture of parts, in the little spaces left between their component tissues, of fluid or solid matters which are not found there in the healthy state. Bony or earthy particles are sometimes laid down, and the part thus changed is said to be ossified. There are few parts of the body in which this kind of induration does not occasionally take place. It is especially common in the coats of arteries, and in the subserous tissues. Blood, or fluids separated from the blood, may fill and obliterate the natural interstices, and concreting, tend to consolidate and harden the part which they occupy. What is called hepatisation of the lung is a good instance. I need not tell you that the healthy lung is spongy and crepitant under pressure; in this altered state it no longer crackles between the fingers; its spongy character is lost; it resembles liver in its compactness and colour, and it is therefore said to be "hepatised." This is a consequence of inflammation; and induration of this kind is a very common consequence (as we shall see) of the same morbid process in various other parts and organs. Another instance of induration of the pulmonary substance we have in what is badly named pulmonary apoplexy. This is independent of inflammation. Blood is collected and coagulates in a part of the lung which should contain air — in the vesicles of one or more of its lobules; the lobules thus gorged with blood become even harder and firmer than when hepatised; but by a different process.

In the instances last mentioned, fluids after escaping from their proper vessels, *i. e.*, in technical phrase, after being *extravasated*, pass into the solid form, and thereby render the parts which they pervade harder and more firm. But fluids may concrete and harden *within* their proper vessels, and so lead to another form of induration. Thus the blood, under certain circumstances, coagulates in the living veins — nay, sometimes even in the heart itself: and we may hereafter have to consider the conditions under which this coagulation is liable to occur, and the serious consequences which it involves. The bile again, as you probably know, sometimes concretes, by a rude kind of crystallization, into what are called gall *stones*: and the passage of these calculi through the narrow ducts that connect the gall bladder with the bowel is apt to be attended with pain the most intense. The formation of *urinary* calculi is not exactly of the same kind.

Numerous specimens of all the changes I have been describing are on the table before you. You may examine them at leisure after lecture, or in the museum.

I have yet to notice another source of unnatural induration, in the deposition or growth of irregular masses of matter within the body, differing remarkably from any of the solids or fluids that enter into its healthy composition. These unnatural formations vary considerably in their nature and appearance, and in their consistence, at different periods. Sometimes they exist in distinct and separate masses, and whether hard or soft in themselves, cause induration by their pressure upon surrounding textures; sometimes they are diffused through or among the natural tissues of a part, which thus they indurate. All the varieties of tubercle, and of cancer, all those forms of disease which have been styled malignant, fall under this head.

These new and morbid products play a fearful part in disorganizing the bodily frame, and in embittering and shortening life. They will necessarily occupy much of our attention in the progress of the course. At present I merely point them out as illustrations of the manner in which the consistence of parts may be *increased*.

LECTURE III.

Softening; its causes and varieties. Transformations of Tissue. Changes of situation—in the Chest, of the Lungs, of the Heart—in the Abdomen and Pelvis, Hernia, Intus-susception, Prolapsus.

WE were occupied with that branch of pathological inquiry which relates to the various ways in which the several parts and organs of the living body are liable to be sensibly altered by disease.

We considered the changes to which the solid parts are subject in *bulk* and in *form*; and that alteration of their consistence which constitutes hardening or *induration*.

The opposite condition to this is *softening*, diminished consistence, a less degree of cohesion of parts and tissues than is natural.

This also is a change of which it is important that you should comprehend the nature, and causes, and varieties; and the share that it often has in breaking down the structure of organs, and in destroying life.

There is scarcely any tissue of the living body, in which softening may not take place. I shall here, however, as before, mention a few illustrations only of its occurrence, taking those instances in which the phenomenon is most evident, or is best understood.

Softening is perhaps never more strikingly obvious to our senses than when it affects the brain or spinal cord. We find portions of these organs manifestly softer than the rest. You are familiar with the usual consistence of the adult brain: you will find it sometimes reduced, in places, to the consistence of cream: a gentle stream of water suffered to fall upon the softened pulp suffices to wash it away, and a cavity is left in its place.

The cellular tissue—or let us rather call it, with Professor Todd, the *areolar* tissue, since minute anatomists now affirm that *all* the tissues in their embryonic state are *cellular*—the areolar tissue is another part in which softening is exceedingly common, although the change is not so readily perceived. This is the great connecting tissue of the body; and we are made sensible of its diminished consistence, when parts which it unites become separable with unusual ease. Thus you may sometimes, by exerting a very slight degree of force, strip off a serous membrane from the parts which it invests, or a mucous membrane from the surface lined by it. This ready separation is a consequence of the diminished consistence of the subserous, or the submucous, areolar tissue. The membranes themselves, in such cases, may be in a perfectly natural state.

Muscles, again, are often palpably softer than they should be; the fleshy substance of the heart, for example. Here the muscular fibre may itself have undergone a change of consistence; or the muscle may simply appear to be softened, in consequence of the softening of the threads of areolar tissue by which its fibres are tied together.

The mucous membranes very frequently present the phenomenon of softening. This is more commonly seen in the stomach than elsewhere. Instead of being raised from the subjacent tissues in large flakes, the mucous membrane, when seized between the blades of a forceps, breaks off in small fragments; or it may be crushed and mashed by the pressure of the finger, or washed away in shapeless pulp by a little current of water. This condition of its lining membrane is usually limited to parts of the stomach; but occasionally it is general.

Even the bones are liable to this change of consistence. There is a disease called *mollities ossium*, in which the bones even of adults become soft and pliant, and capable of being bent in any direction. Upon what these altered qualities are believed to depend, I will explain to you presently.

The accidental products to which I adverted when speaking of induration—especially some of the varieties of cancer—are sometimes remarkably soft, resembling

brain in consistence and appearance, or cream, or jelly. But in these cases we can scarcely consider the change as an example of softening of the textures of the body; it rather consists in the addition of parts that are themselves soft and half fluid.

Now softening may occur under very different circumstances. One very general cause of softening is inflammation. Every part, I believe, that is inflamed undergoes, in the first instance, a diminution of its consistence. This appears to be almost the necessary consequence of stagnation of the blood, the effusion of serosity, and the suspension of healthy nutrition. These are circumstances to which I shall recur. I cannot avoid alluding occasionally to things with which you are supposed to be as yet but little acquainted, and which will engage our particular attention as the course advances.

It would be a great mistake, however, to imagine that all softening results from previous inflammation. Doubtless it often proceeds directly and simply from deficiency of nutrition, and is then closely allied, as I said before, to atrophy. Thus softening of the brain is, sometimes, due to inflammation: we meet with it where the inflammation has been unequivocal, and was caused by external injury; but sometimes also it is quite independent of inflammation, and is owing to disease or obstruction of the cerebral arteries, whereby the brain, or a portion of it, is deprived of its full supply of arterial blood, and ceases to be properly renovated. Hence a loosening of its texture, a separation of its component particles, an approach to the fluid state. I shall, of course, hereafter endeavor to point out to you more particularly the means we possess of distinguishing these two forms of cerebral softening. They constitute morbid conditions of the highest interest.

I may observe, that we have an illustration of the principle now laid down, in that general softness, flaccidity, and slight cohesion of parts, noticeable in children, and others, who are imperfectly nourished. We find this general absence of the natural firmness coincident with paleness, and a thin watery condition of the blood. Magendie kept animals upon food unsuitable for them, containing no azote, and incapable of supplying sufficient nourishment; and one curious consequence was a loss of substance in the cornea, which melted down and disappeared.

There is another source of softening which requires to be mentioned—I mean the gastric juice, which has the power of dissolving not only food that is submitted to its action, but the mucous membrane of the stomach itself, and even all its tissues and coats. This cause of softening operates, however, in the dead body only; but its effects have often been mistaken for the consequences of disease; and therefore it will be necessary for me hereafter to call your attention to the circumstances under which those effects may be looked for, and to the points of distinction between them and other changes that are more properly called morbid.

Upon the whole, it may be said that every form and kind of softening in the living body—whether it proceed from inflammation, from disease or obstruction of the arteries, from insufficient sustenance, or from altered qualities of the blood—may ultimately (like atrophy) be resolved into suspended or defective nutrition.

Furthermore, as there is a hardness of parts resulting from repletion and distension, so there is a *softness* rather than a *softening*, from their emptiness and flaccidity: as of the breast immediately after the child has suckled; of the abdomen soon after delivery; of the integuments in those who, having been fat, have wasted, either from disease or from advancing age; and so on.

On former occasions, I thought it right to lay before you the views of M. Andral (which appear to have been adopted also by Sir R. Carswell) respecting what has been called the *transformation of tissues*. “In the proper place of one natural tissue (I remarked) we sometimes find another, which last is thus *unnatural* in regard to its situation, but natural in all other respects. The new tissue is such as we meet with elsewhere in the body, but it is not such as properly belongs to the place it occupies. Either the original tissue has been gradually converted into the new, or the original tissue has disappeared, and the new tissue has been substituted for it: that, for example, which should be cartilage we sometimes find to be bone.”

“In most cases the tissue that has been changed or displaced is in one of the two following predicaments:—

“Either its natural function has been for a long time suspended;

"Or, it has been accidentally called upon to fulfil a purpose for which it was not originally designed.

"In the former case it gradually approximates towards areolar tissue, which at length is all that remains.

"In the latter it assumes the characters of that other tissue of which it has taken up the office."

Now the analogy which M. Andral thought he could perceive between changes of this kind, and the changes that occur during the growth and progressive development of the human body, does not in reality obtain. More recent and more exact microscopical researches have shown that the several tissues do not commence by being areolar tissue—which is the sense in which M. Andral uses the word *cellular*—and therefore that in the dwindling of any given tissue into the areolar, there is no return, as he had supposed, towards the primitive state of the tissue so wasting. A muscle remaining for a long time in complete inaction, loses bulk, but does not pass from the condition of muscular into that of areolar tissue. When wasted to the utmost it may still retain its proper anatomical elements. The areolar tissue is quite as complex and advanced a tissue as the muscular. There is no true conversion of the one tissue into the other. It is commonly stated, indeed, that when a muscle comes accidentally to invest a dislocated joint, the dislocation remaining unreduced, it assumes by degrees the characters, together with the uses, of those tissues which naturally inclose the joint, and is *converted* from muscular into fibrous or ligamentous tissue—just as in the vegetable kingdom, the cut end of a willow *branch*, planted in the earth, takes up the office, and gradually acquires the form and properties of a *root*. But here again the analogy is more fanciful than real. The formation of a false joint implies no actual conversion of tissues. The muscular fibres shrink and disappear, while the areolar tissue augments, and is *transformed* only into the fibrous; these two, the fibrous and the areolar, being essentially and primarily the *same* tissue.

The change from cartilage to bone approaches more nearly than any other to actual transmutation; but even this resolves itself into a simple increase of one of the natural constituents of both the tissues concerned; phosphate of lime, which exists in healthy cartilage.

I spoke of local and of general additions of adipous tissue occurring in the body, as forms of hypertrophy. But fat is apt to be produced, by a sort of transformation, in atrophy also. Mr. Paget, indeed, whose remarks on this subject possess a very high interest, makes fatty degeneration to be one kind of atrophy. He describes atrophy without change of texture, (in which sense I have been using that term,) and atrophy with degeneration of texture. Although there is no necessary connexion between them, the two often exist together, but one of the two predominates. The degeneration proceeds under the ordinary conditions and causes of simple atrophy; and it is a common result of that imperfection of the formative process which accompanies the infirmities of old age.

In this form of atrophy the fatty matter is not deposited, as in hypertrophy, from the blood-vessels, and laid up in cells or vesicles; but it is apparently the result of some chemical change wrought in the affected tissue itself, throughout which the molecules of fat are irregularly distributed. It is a process and a mark of decay. It is met with in unexercised voluntary muscles, whether their action be suspended by paralysis or by the immobility of the parts which it is their function to move. It occurs under the deteriorating influence of disease, or of age, in that involuntary muscle, the heart; in the arteries, of which the muscular element is probably the first to suffer; in the bones; in the cornea, where it becomes visible even during life as the *arcus senilis*; and in various other organs of the body. What is called the fatty liver is an example. The altered liver is larger than natural, of a light tawny colour, of diminished specific gravity, retains the impression of one's finger, is tender, and tears easily: it greases the knife that cuts it, or bibulous paper in which it is wrapped. By boiling it you may obtain a concrete oil, which has all the characters of fat. Under the microscope, the molecules of fatty matter are recognised, in this and in other tissues, by their peculiar refraction of light. They sometimes run together into larger unequivocal oil-drops. Chemistry detects their nature by their solubility in ether.

What is very curious in respect to this morbid condition of the liver is, that we

can produce it, at will, in some at least of the lower animals. You know that the "*foie gras*," procured from certain birds, is an article of great luxury among epicures. It is obtained by a very cruel process. Geese, or ducks, are confined in baskets just large enough to contain them, but not large enough to allow them any movement: they are kept continually in the dark also; sometimes even, I am afraid, their eyes are put out, but this I should imagine to be a useless and superfluous piece of cruelty, it being the absence of light, and not the absence of the power of vision, which helps to bring about the desired effect. At the same time the birds are sedulously crammed with food. Under this discipline their livers acquire the requisite size, and greasiness, and the true flavour.

The history of these unfortunate fowls is not barren of instruction in respect to the more limited bad effects of full diet, want of exercise, and a short allowance of daylight, upon the "featherless biped," man.

Fatty degeneration affecting the muscular substance of the heart, and rendering that main instrument of the circulation soft, weak, readily stretched by the blood which it compresses, and easily torn, becomes a frequent and an intelligible cause of dire distress, and of death. Concerning this most perilous cardiac disease, our knowledge is of recent acquisition. In the larger arteries also, where it has long been known under the name of *atheroma*, the same form of decay leads to dilatations, to aneurisms, to fatal ruptures. When it occupies the smaller branches of the same vessels it tends to softening of the parts which those branches then fail to nourish and maintain, and to the escape of blood from their broken channels into and among the softened textures. This is a very common source of cerebral mischief—of apoplexies, and of palsies.

Following out Mr. Hunter's original views, Mr. Paget has satisfied himself that the singular disease of bones described by English writers under the name of *mollities ossium*, is also owing to this fatty degeneration. Nay, the same morbid change may pervade the whole body. In all ranks of life there are two well-marked forms of senile decay: and every one will at once, I think, recognise the fidelity of the following graphic sketch by Mr. Paget's pencil.

"Some people, as they grow old, seem only to wither and dry up—sharp-featured, shrivelled, spinous old folks, yet withal wiry and tough, clinging to life, and letting death have them, as it were, by small instalments slowly paid. Such are the 'lean and slippered pantaloon,' and their 'shrunk shanks' declare the pervading atrophy.

"Others—women more often than men—as old and as ill-nourished as these—make a far different appearance. With these the first sign of old age is that they grow fat; and this abides with them till, it may be, in a last illness sharper than old age, they are robbed even of their fat. These too, when old age sets in, become puffy, short-winded, pot-bellied, pale and flabby; their skin hangs, not in wrinkles, but in rolls; and their voice, instead of rising 'towards childish treble,' become gruff and husky.

"Now, these classes of old people may represent the two forms of atrophy—of that atrophy by decrease, and that by degeneration of tissue—to which we shall find nearly every part of the body liable. In those of the first class you find all the tissues healthy, hardly altered from the time of vigour. I examined the muscles of such a one lately—a woman, seventy-six years old, very lean, emaciated, and shrivelled. The fibres were rather soft, yet nearly as ruddy and as strongly marked as those of a vigorous man; her skin too was tough and dry; her bones, slender indeed, yet hard and clean: her defect was a simple defect of quantity.

"But in those that grow fat as they grow old, you find, in all the tissues alike, bulk with imperfect texture; fat laid between, and even within, the muscular fibres, fat about the heart, the kidneys, and all the vessels; and the bones so greasy that no art can clean them: the defect of all these is the defect of quality."

All these fatty changes are plainly morbid. The transformations that are effected in false joints are as evidently methods of accommodation and repair. The same may be said of the transformation—which is not conversion—of areolar tissue into synovial membrane. Synovial membrane consists chiefly of condensed areolar tissue. Sir Benjamin Brodie, in his book on Diseases of the Joints, gives instances of synovial membranes being formed, where none before existed. "In a young lady who had attained the age of ten or twelve years, labouring under the inconvenience of a club-

foot, a large bursa was distinctly to be felt on that part of the instep which came in contact with the ground in walking. In another young lady, who had apparently recovered of a caries of the spine, attended with a considerable angular curvature, a bursa appeared to have been formed between the projecting spinous process and the skin."

In like manner we find that sinuses, fistulous openings and tubes, in various parts, become lined, through the intervention of the areolar tissue, with a surface which in its appearance and in its properties resembles the mucous membranes. Like them it is with difficulty made to take on adhesive inflammation; and therefore it is that sinuses of this kind, and chronic abscesses, are often so troublesome to the surgeon, and require to be laid open before they can be abolished.

On the other hand, the mucous membranes, under peculiar circumstances, approximate to the skin in their physical aspect and qualities. When, for instance, a portion of the mucous lining of the rectum, or of the vagina, protrudes externally, is permanently exposed to the air, and subject to the friction of clothes or of neighbouring parts—that is to say, when it is placed under the same conditions as the skin—it assumes somewhat the characters of the skin: it gradually loses its red colour and approaches the tint of the skin, ceases to pour forth mucus, becomes dry, obtains even a sort of permanent cuticle, acquires firmness and density, and is less sensible to the contact and pressure of foreign substances. It is impossible not to perceive the beneficial nature of this transformation.

The greater number, then, of those interesting changes in the living body which have been classed under the head of transformations of tissue, have a restorative tendency. They exemplify the working of what the older pathologists discerned, and called the *vis medicatrix naturæ*. This is a phrase that has been much sneered at; but (as I conceive) very unjustly, and sometimes ignorantly. It is simply a short formulary, expressive of a great general truth, viz., that the animal frame is so constituted as to contain within itself the elements of repair, and of conservative adaptation. To a great extent it is a *self-mending* machine. Surely this is an admirable provision, and clearly indicative both of wise contrivance and of beneficent design.

The intimate texture of parts may be further altered—not simply by some modification or reconstruction of the ordinary tissues, but—by an absolute disappearance or confusion of all regular structure. This is usually a consequence, either of the effusion, in the natural interstices of the parts, of fluids, which afterwards pass into the solid state, or it is a consequence of the growth of solids which do not belong to the healthy body. In this sketch of general pathology I must content myself with thus briefly alluding to this source of morbid change.

I may as well observe here, that the alterations with which we have hitherto been occupied, of the solids of the body, fall, almost all of them, under the head of *lesions of nutrition*, as the French pathologists speak. That is to say, they commence and have their primitive seat, in that process and place where the blood, having reached the capillary system of vessels, performs its special purposes. It is in or through the capillaries that the fluids and solids accomplish their vital union. Each solid receives from the blood, and assimilates with its proper substance, material particles, identical in their nature with those of which it already consists. Each solid gives up also to the blood, and so dismisses, other particles, which before formed a portion of itself, but which have become unfit or superfluous. Now any departure from this continual building up and pulling down—any excess or defect of the particles added, or of the particles subtracted—any irregularity in the manner in which they are deposited—any variation from their right consistence, or in their kind and quality—in short any deviation from the regular process, as I have briefly described it—is called a *lesion of nutrition*.

The few changes already spoken of, and *not* included among the lesions of nutrition are:—

The distension of the hollow organs by an undue accumulation of fluids within them;

The coagulation of the fluids in their proper vessels, excluding however the capillaries;

The escape of the fluids, as such, out of and beyond their containing vessels; and

The solution of tissues, after death, by the chemical agency of the gastric juice.

None of these, properly speaking, constitute lesions of nutrition, although they sometimes lead to them.

Lastly, let us take a glance at the changes of *situation* to which the solid parts of the body are liable. They are sometimes of very serious import.

These changes of place—sometimes the consequence of disease, sometimes its cause, and not unfrequently the cause of death—respect chiefly the viscera; and most especially the viscera of the chest, abdomen, and pelvis. I omit dislocations of joints, as belonging exclusively to surgery.

In the chest, a whole lung may be displaced, and compressed against the vertebral column, by blood, or serum, or air, effused into the cavity of the pleura. An alteration of this kind, whereby one-half of the respiratory apparatus is rendered incapable of its peculiar function, cannot be otherwise than full of peril.

The very same causes operating on the left side of the thorax may dislocate the heart, thrust it over to the right of the sternum, where it may be felt, and heard, and seen, to pulsate. This again cannot happen without greatly disturbing the vital function of circulation, and putting life in jeopardy.

Yet neither of these serious displacements is necessarily fatal. Both admit, under certain circumstances, of remedial treatment; as I hope to prove to you hereafter.

In the abdomen and pelvis, the various forms of hernia may be adduced as involving very dangerous changes in the place and relative position of parts. Portions of the intestinal tube are apt to pass through accidental openings in the diaphragm—or between the edges of the linea alba surrounding the navel—or out at the abdominal ring—or through some other natural or accidental aperture. I need not tell you how fearfully life is compromised when, in consequence of such faulty position, the bowel becomes constricted—when its contents can no longer pass onwards, and inflammation, or gangrene, is present or impending. Even when there is no strangulation, the mere displacement, arising from the escape of some of the contents of the abdomen and pelvis from their natural limits, may be productive of much discomfort, deformity, and hazard. Of this the historian Gibbon presented a remarkable example. He had an immense scrotal hernia; so large it was, that it hung down very nearly as low as his knees. After his death it was found that almost the whole of the omentum, and the greater part of the colon, had descended into the scrotum, and had dragged the stomach after them; so that its pyloric orifice lay close to the abdominal ring.

Akin to hernia is that partial displacement of the bowel in which a portion of it passes, not through any natural or accidental opening, but into the bowel itself: just as one portion of the finger of a glove is sometimes pulled into the remaining part, by the withdrawal of one's hand. The contained portion of intestine is liable to be nipped and strangulated by the containing portion—and all the peril of hernia results, with much less chance of relief by art. This state of things is called *intussusception*.

Exactly of the same nature, though less alarming, is *prolapsus* of the rectum, or of the vagina. Here also a portion of the tube passes into the contiguous portion; but being near the extremity of the canal, the inverted part protrudes externally, and becomes, in most cases, a source of distress and suffering, rather than of danger. Inversion of the uterus is another example.

Thus much, then, of the changes to which the *solid* parts of the body are subject, in *bulk*, in *form*, in *consistence*, in *texture*, in *situation*.

You cannot fail to perceive the injurious effects which many of these changes in the various solids are calculated to produce upon the movements and working of the living machine; how some of them must impede or derange its natural action; some stop that action altogether.

Now the fluid parts of the body are liable also to alterations, which, if they be not always so obvious as those of the solids, are certainly not of less moment.

You are probably aware that, for many centuries, the fluids were supposed to be the primary agents in every form of disease; that all maladies were attributed to some acrimony or peccant state of the humours; and that however else the theories of medicine might vary and fluctuate, the *humoral* pathology, till a comparatively recent period, ran through almost all of them. At length, the absurdity of the hypotheses,

and still more the dangerous practice, which this doctrine generated, began to be manifest, and led to its total abandonment. Rather more than a century and a half ago, the foundation of the opposite doctrine appears to have been laid, by the writings of Glisson in this country, and by those of Baglivi in Italy; and presently the notion came to prevail throughout the schools, that all the morbid conditions of the body had their exclusive origin in the solids. The pendulum of opinion swung at once, as is usual, into the opposite extreme of error. It promises, in our time, to settle at the juster medium. Reviving under new and more faithful evidence, the humoral doctrine again asserts its rightful but modified claims upon our acceptance. That its old extravagancies still find favour among the ignorant, and are commonly adopted by the quack, are circumstances which illustrate the fact that the mischievous influence of unsound theories survives the duration of the theories themselves. The scientific physician of the present day can only wonder how exclusive solidism, or exclusive humoralism, should ever have found advocates.

LECTURE IV.

Morbid Alterations of the Fluids, especially of the Blood. Changes in its quantity and distribution. General and Local Plethora. Poverty of Blood. Active Congestion—its Phenomena—State of the Vessels as seen by the Microscope. Mechanical Congestion. Passive Congestion. Relations of these forms of Congestion to Inflammations—to Hæmorrhages—to Dropsies.

AFTER running over the principal alterations to which the solid parts of the body are liable, we were beginning to inquire into those no less important morbid changes which are apt to take place in its fluid constituents. I reminded you that, respecting the whole of this subject, pathologists had passed from one extreme of opinion to another; that for a very long period the *humoral pathology* prevailed in the schools, and that, in times not very remote from our own, it was entirely superseded by the opposite doctrine of exclusive *solidism*. It is strange that either misconception should have so long maintained its ground.

If we consider the definite relation subsisting between the solids and the fluids of the body, and the unceasing agencies which they mutually exercise on each other—how, for instance, on the one hand, all the solids are originally built up, and are afterwards perpetually sustained and repaired by materials furnished from the blood—how, again, on the other hand, some of the solids are continually employed in the reciprocal office of feeding and renewing the blood, while others are as constantly at work in decomposing it by the various secretions—we cannot avoid perceiving that distinctions of the kind I have mentioned, founded upon mere differences of consistence, are futile. Flesh and blood are almost convertible terms: their composition, the chemists tell us, is identically the same. To use the strong expression of Bordeu, *Le sang est de la chair coulante*. You may be certain that no notable alteration can take place in the solids of the body which will not soon affect in some way its fluids; and that every important change in its fluids must lead to, or proceed from, a corresponding and proportionate modification of its solids. The long dispute between the solidists and the humoralists was altogether baseless and unprofitable.

Mr. Paget, in the admirable lectures to which I have already referred, cites and adopts the proposition of Treviranus, that “each single part of the body, in respect of its nutrition, stands to the whole body in the relation of an excreted substance.” “In other words, every part of the body, by taking from the blood the peculiar substances which it needs for its own nutrition, does thereby act as an excretory organ, inasmuch as it removes from the blood that which, if retained in it, would be injurious to the nutrition of the rest of the body. For example, the polypiferous

zoophytes all excrete large quantities of calcareous and siliceous earths. In those which have no stony skeleton these earths are absolutely and utterly excreted; but in those in which they form the skeleton, they are, though retained within the body, yet as truly excreted from the blood and all the other parts, as if they had been thrown out and washed away. So the phosphates which are deposited in our bones are as effectually excreted from the blood and the other tissues, as those which are discharged with the urine."

This doctrine, if it be true, as I think it is, puts in a strong light, not only the constant relation and interchange subsisting between the solid tissues of the body and its fluids, in health, but their inevitable sympathies also, in disease.

The animal fluids are—the blood, the fluids that enter the blood, and the fluids that proceed from the blood.

The fluids that enter the blood are of two kinds.

1. Those by which it is renewed and enriched.

2. Those which enter it in order that they may be conveyed out of the body.

Now, although we cannot doubt that any considerable modification or defect of the fluids that feed and renovate the blood, and particularly of the chyle, must have a direct influence upon its composition and quality, we really know but little about them, except in their effects. We seldom have any means of procuring these the first products of nutrition so as to examine them, or to test their qualities; yet we can perceive causes that are likely to deteriorate or deprave those fluids (unfit aliment, impure air), and we know that, under the continued operation of such causes, the blood, replenished by these fluids, is actually and sensibly modified.

Again, we cannot doubt that some of the matters derived from the body itself, and taken into the blood in order to be conveyed away, may, and often do, directly alter and contaminate the blood, and act as poisons upon the system; matters, for instance, absorbed from parts of the body that are diseased, or dead and putrefying; in this way, doubtless, disorders which were at first strictly local may come to affect the whole economy:—matters, again, which, though harmless while merely transitory, and in minute quantity, prove noxious when retained and accumulated in the blood, in consequence of faulty or deficient action of the organs destined to eliminate them from the circulating fluid. The injurious effects of some of the substances which thus become deleterious,—as urea, of which the blood, during health, is continually purified by the kidneys; and bile, which is naturally separated therefrom by the liver; and carbonic acid, which it is the office of the lungs to excrete—will furnish topics of interesting inquiry hereafter.

The fluids that *leave* the blood may be considered under a threefold division.

1. Those which are directly expended in the growth or maintenance of parts, some of them becoming fixed and solid, and others retaining their fluid condition. Of these the principal alterations have been briefly pointed out among the *lesions of nutrition*.

2. Those that are employed in aid of some definite function of the body: as the saliva, the gastric juice, the bile, the pancreatic secretion, the tears, the synovia of the joints, and so on. Now, these may be secreted in excessive abundance, or in too scanty quantity, or of imperfect quality, or not at all: and all, or any, of these deviations from the healthy standard may be the result of very serious disease, or may cause very serious disease; and they will be spoken of hereafter when the disorders of the parts or functions connected with each shall be discussed.

3. Those which are separated from the blood merely to be excreted, as the urine, certain discharges from the bowels, and from the bronchi and skin. Some of these are extremely worthy of study, as furnishing, in their altered qualities, indications of disease; but they require no particular consideration in this part of the course.

Dismissing, therefore, for the present, all further account, as well of the fluids that concur to form the blood, as of the fluids that issue from the blood, let us inquire what morbid changes the blood itself is liable to undergo.

The blood, then, is subject, first, to remarkable variations in its *quantity*, both in respect to the whole system, and in respect to particular organs and tissues.

2. Closely connected with these differences of quantity is the variety which is observable in regard to the proportions between the several proximate constituents of

the blood. The changes that occur of this kind are sometimes strikingly evident to our senses. For example, we not unfrequently perceive that the blood drawn from a vein is thinner, manifestly more watery, less rich in red corpuscles and in colouring matter, than blood of the standard quality.

3. Again, independently of mere alterations in the relative proportions of its proximate constituent parts, the blood is liable to great change in its chemical composition, and, therefore, in its physical quality. This appears to be the case in scurvy, and in the analogous disease called purpura, and it is doubtless so in many other complaints.

The composition of the blood cannot fail to be affected by a deficient supply of the elements of nutrition from without; by diseases of the digestive organs, interfering with the process of chylication; by diseases of the organs of respiration, interfering with its change from venous to arterial; by diseases of other channels of excretion—the bowels, the biliary apparatus, the kidneys, the skin—interfering (as I have already hinted) with its appointed purification; nay, by disease in any part, if Treviranus' theory be allowed; by foreign contaminating matters, finding entrance (as they may when in solution, or in a gaseous form) through artery, vein, or any membranous substance, such as bladder and intestine: lastly, the composition of the blood may be altered, there is good reason to believe, by certain states of the nervous system.

But contenting myself with having indicated these latter changes, or sources of change, I shall defer giving a more particular account of any except those that relate to the *quantity* and the *distribution* of the blood.

I say the blood may undergo important alterations in its *quantity*. It may exist in too great abundance throughout the body; and it may exist in too great abundance in certain parts only of the body. These states have been recognised for ages. Sometimes they are called, respectively, general and partial *plethora*; sometimes general and local *congestions* of blood; people speak also of irregular *determinations* of blood to different organs; and, of late, the term *hyperæmia*, first invented by M. Andral in France, has been imported into this country, and much adopted here. All these words and phrases mean, in truth, the same thing; and their frequent recurrence in medical works, is, of itself, sufficient evidence of the frequency and importance of the conditions which they express.

If we comprehend rightly this subject of plethora or congestion, we shall be prepared to understand some most important morbid states, of which it seems to be in many, if not in all cases, the earliest approach—the initial step. Inflammation, hæmorrhage, dropsy, all acknowledge and imply a previous condition of congestion. “There is, probably,” says Dr. Alison, “no kind of diseased action of which any part of the living body is susceptible, which is not connected, sooner or later, with increased afflux of blood towards that part, either as its cause or its effect; and the immediate object of all our most powerful remedies is to act on these irregularities of the circulation.”

That the blood *may* be differently distributed in the capillaries at different times, we know by the variable colour of the surface, which depends upon the varying degrees of fulness of the cutaneous blood-vessels. The phenomenon of blushing, the red cheek of anger, the heightened colour of the skin under brisk exercise, are familiar facts illustrative of partial plethora of the capillaries, consistent with health.

There are reasons (which I shall hereafter lay before you) for believing that a similar sudden accumulation of blood, taking place in internal parts, may sensibly disturb their functions; causing transient fits of giddiness, insensibility, and sometimes death itself, when the congestion affects the *cerebral* blood-vessels; and attacks of difficult breathing when the capillaries of the *pulmonary* tissue are concerned; and even *these* attacks, for aught that I know, may end fatally.

It often happens that when certain portions of the surface, as the cheeks, are visibly redder and fuller of blood than usual, or when such symptoms as I have just mentioned denote the probability of some internal congestion, other parts of the surface, as in the extremities, are visibly paler: and there are, at the same time, corresponding and palpable differences of temperature.

Perhaps it may not be so obvious that the whole quantity of blood, throughout the body, is sometimes in excess.

That in the adult state, when the growth and development of the body have been completed, blood may be made in greater abundance, and more rich in the materials of nutrition than the wants of the body require, is not only conceivable, but true. We are able to assign circumstances in which this is likely to happen, and we find that under such circumstances it actually does happen. Full living, and a sedentary life, are causes likely to occasion general plethora—and they do occasion it. The full diet, so long as the digestive powers are perfect, provides more chyle, conducts into the blood a larger quantity of its proper pabulum. The sedentary life precludes that freer circulation of the blood, and that more liberal expenditure of it through the skin, and by means of the other organs of secretion, which would occur under more active habits. Persons thus circumstanced are apt to grow fat; the adipous tissue seeming, in these cases, to form a kind of safety valve for the diversion of the superfluous blood. Such persons have turgid and florid cheeks, red lips, red mucous membranes, and (not uncommonly) ferrety eyes. Their entire vascular system is preternaturally distended. If you open a vein, you find that they bear a copious abstraction of blood without fainting, and are even refreshed by it; and the blood drawn separates into a large and firm mass of coagulum, with but little serum. Keeping to the nomenclature we have already employed, we might say that there is here *hyper trophy* of the blood.

When inflammation arises in the subjects of this general plethora, it runs high, and requires active treatment. But they are not, as you might naturally expect them to be, and as many writers state them to be, peculiarly prone to suffer inflammatory complaints. There is general fulness of the vascular system, but no irregularity, nor any necessary tendency to irregularity, in the distribution of the blood.

You will observe that the relative proportion of the more solid to the more fluid constituents of the blood is increased in these cases of general plethora: the blood is not only more abundant, but it is richer also in fibrin, and in red particles.

The means to be adopted for redressing this unnatural and unsafe condition of the circulation, are those which common sense would suggest. The removal of a portion of the superfluous blood, a more restricted diet, a larger allowance of active exercise.

It will be worth our while to contrast this state of general plethora with its opposite—that in which the blood is scanty and poor—what Andral calls (though with questionable propriety) *anæmia*. *Oligæmia* is the cacophonous but more exact name assigned to it by Gendrin; but *poverty of the blood* is the ordinary English phrase for it, and the best of the three. This is a state which we can produce at will, by abstracting blood from the body in moderate quantity, but repeatedly, and at short intervals. It occurs also, frequently, in spontaneous disease, and from various causes; from a privation of the materials destined to replenish the blood; and in cases in which these materials appear to be turned to little account, as in chlorotic girls. We see it in those who habitually and often lose a certain quantity of blood, in disease; in persons, for example, who are subject to piles, and who bleed daily from the rectum; still oftener in women who suffer repeated hæmorrhages from the uterus. When the drain has been long-continued, these persons become very pale; even those parts which are naturally most red, as the lips and tongue, become almost white; their faces look like wax; and if still you draw blood from a vein, and allow it to coagulate, you will have a small clot floating in an abundance of serum, and that small clot will be of a light rosy colour; showing a great diminution in the proportion of fibrin; and a still greater deficiency of the red particles. The blood, as they say, is “turned into water.” It is a curious pathological fact, that the red particles require more time for their restoration than the other constituents of the blood. And I may mention to you now, what I shall have to repeat, that—in conjunction with the obvious curative measures comprised in arresting the habitual loss of the vital fluid, and in affording sufficient nutriment to the system—the preparations of iron, and the respiration of pure air, have signal efficacy, in renewing the red particles, and giving back again their native hue of redness to the cheek and lips.

In connexion with this subject, I would direct your attention to some interesting statements of Dr. Owen Rees’ in his *Gulstonian Lectures*, delivered before the College of Physicians in 1845, and subsequently published in the *Medical Gazette*.

According to Dr. Rees, the true condition of the blood, as it exists in the living

blood-vessels, is that of a liquid (the *liquor sanguinis*) in which the fibrin of the blood is dissolved, and in which coloured corpuscles float.

He shows, by satisfactory experiments, that these corpuscles are not soft solids, but closed bags or cells, containing a fluid:—that the contained fluid is of a red colour, while the investing membrane is white, or colourless.

Through this investing membrane, in obedience to the law of endosmosis, the fluids without and within the corpuscle reciprocally pass. Placed in a liquid of greater specific gravity than the average specific gravity of the liquor sanguinis, the corpuscles shrivel, and the liquid is much reddened. On the other hand, if the surrounding liquid have a specific gravity less than that of the liquor sanguinis, it is but slightly reddened, and the corpuscles plump up. In pure water they burst.

The iron of the blood resides in the colouring matter dissolved in the liquid which is enclosed in the colourless envelope.

The blood is fed by the chyle. The chyle, like the blood, separates, when removed from the body, into two parts—serum and crassamentum.

The serum of the blood contains no iron; the serum of the chyle contains iron in abundance. The crassamentum of the blood contains iron; that of the chyle only such a trace of it as may be accounted for by the adhering serum.

Again—the specific gravity of the chyle is far below that of the liquor sanguinis. Hence, on the mingling of these fluids, an endosmotic transmission of iron in solution will take place into the corpuscles.

It follows, that if the specific gravity of the liquor sanguinis be any-how lowered, or that of the chyle much increased, the supply of iron to the corpuscles will be so far impaired.

These considerations may hereafter be found applicable to the elucidation both of the nature, and of the treatment of certain forms of disease.

[Dr. Hughes Bennett has recently directed attention to a diseased condition of the blood, in which there is a morbid increase of corpuscles, resembling the white or colourless ones naturally present. This condition has been denominated *leucocythæmia*, or white cell blood. It is not of very common occurrence, but more so, perhaps, than is generally supposed. When a drop of blood, drawn during life from an individual affected with leucocythæmia, is examined microscopically, the red corpuscles appear but little changed from their natural condition, and often arranged in rouleaux, leaving intermediate spaces more or less crowded with white corpuscles. The latter bearing a proportion to the former, varying in different cases from one-third to one-half: but upon this point, in consequence of the limited number of observations that have yet been made, it is impossible to speak with precision. Many of the white corpuscles are considerably larger than the natural size. They have more coarsely granulated contents than the normal ones, with a single, double, or tripartite internal nucleus. The envelop and nucleus are distinctly brought into view by the action of acetic acid, which renders the granular contents transparent. Occasionally, a crescentic nucleus is to be seen in the cells, and some free nuclei are observed, also, between them.

In fatal cases of leucocythæmia, the blood is often found imperfectly coagulated—sometimes grumous, and of a dirty brown colour. Where decolourized, the coagula have not the aspect of healthy fibrin, but are of a more opaque dull yellow, and, when broken up, resemble thick creamy pus. Their peculiar aspect is due, probably, to the very numerous white corpuscles they contain. In the case in which the blood was analyzed, the fibrin exceeded the normal amount. The increase is probably more apparent than real, in consequence of numerous white corpuscles being included in the fibrin. The red corpuscles are invariably diminished—the solids of the serum but little altered. It is chiefly in the spleen, liver, and lymphatic glands, that morbid changes are observed. The spleen is often very greatly enlarged, apparently by a true hypertrophy of its nuclear structure. It has, however, been found healthy. The liver is far less frequently enlarged; but its texture is more or less altered. In the majority of the

FIG. 5.



Blood in Leucocythæmia—four of the white corpuscles have been treated with acetic acid. From Dr. H. Bennett's work.

cases reported, the lymphatic glands seem to have been enlarged or cancerously diseased.

The morbid condition of the blood under consideration has been much more frequently observed in males than in females; more often in adults, and in those of advanced age, than in youth.

The respiration is often interfered with in consequence of the abdominal distension; diarrhoea is a frequent symptom; vomiting is less often present; hæmorrhage, from various parts, was observed in the majority of cases; in one instance, it was attended with purpura hæmorrhagica; dropsy was present in about one half the cases, general attendant upon abdominal tumours; some slight febrile disturbance is not unfrequent, but of short continuance; anæmia is commonly well marked, and, in the fatal cases, emaciation is described as extreme. There seems no reason to suppose that the affection is in any way directly connected with ague or the malarious poison.—C.]

In general plethora every part is preternaturally full of blood, and the blood itself is full of the elements of nutrition. General plethora therefore implies, *in one sense*, local plethora of every organ and tissue. In strictness, however, local plethora is only predicable of a part that contains more than its share of red blood.

Now the converse of this is not true, as it might be expected to be, of the opposite condition. A deficiency in the whole mass of red blood contained in, and circulating through, the body, does not protect the *parts* of the body from congestion—from having an undue quantity of blood sent to them. Far from it. Local determinations of blood are *very common* in persons in whom the mass of that fluid, and the proportion of its nutritive materials, have been considerably diminished by disease, or by hæmorrhage.

This remarkable tendency, under such circumstances, to an unequal distribution of the blood in the capillaries, admits (I think) of the following explanation. A due supply of healthy blood is requisite for the steady and equable performance of the functions of the brain and nerves. When this supply is defective, or uncertain, those functions become disordered and irregular, and, in their turn, influence the various solids, disturb their action, and derange the balance of the circulation. That capillary blood-vessels may be filled to excess, or completely emptied, by causes operating *through the nervous system*—by moral emotions, for example—we are sure from the phenomena just now adverted to, the blush of shame or anger, the paleness of fear; and there can be no doubt that *morbid* congestions, which sometimes are separated from those that are consistent with health by very slight shades of difference, are often determined through the agency of the same nervous system. And persons endowed with great sensibility or irritability of the nervous system are very liable to partial and irregular congestions of blood.

But this is not the only way in which local congestion may arise.

We can produce it, upon the surface of the body at least, at pleasure, and that in various ways; by friction, by exposing the part to a high temperature, by certain stimulating applications, mechanical (as a cupping glass), or chemical (as a mustard poultice): we produce an injection of the small cutaneous blood-vessels; there is, evidently, more than the usual quantity of blood attracted to the part, or detained in the part—a degree of redness, which soon subsides if the cause of it be withdrawn in time.

Congestion thus occasioned is not inflammation, but it is the first obvious step towards that complex process; and for this reason it deserves all your attention. Apply the exciting cause a little longer, or increase, in a slight degree, its intensity, and the phenomena of inflammation begin to manifest themselves.

I said we can excite local congestion, when we please, upon the *surface* of the body: but there can be no doubt that a similar state may be produced by analogous causes, in internal parts. Look at this representation of the stomach of a dog (*one of Dr. Roupell's plates*). You see one portion of it of a bright red colour, actively and vividly congested. This was the consequence of a dose of alcohol. We may be certain that something of the same kind is the result, in the human stomach, of every visit to the gin shop.

Local congestion thus produced, or of this kind, is said to be *active*. M. Andral, whose nomenclature has come much into fashion of late years, calls it *sthenic*, or

active hyperæmia. The arteries, perhaps, have more to do with it, in the first instance, than the veins. But it is in the capillary vessels, which are distinct from, and interposed between the minute arteries and veins, that further changes are wrought, when the process advances a stage beyond mere local plethora. What has been observed, by the aid of the microscope, with respect to the blood-vessels, I will endeavour to describe to you.

I take the account I am about to give you chiefly from Kaltenbrunner, a German pathologist, who has recently investigated the subject experimentally, and whose observations are believed to have been most carefully and skilfully conducted, and their results no less faithfully narrated. His observations were made upon the circulation as it appeared in the web of a frog's foot, under a powerful microscope. It would be idle, and something like committing a fraud upon you, were I to lay any stress upon my own knowledge or experience in this matter, for I cannot pretend to any great skill in the use of that instrument, and my opportunities of noticing, by its help, the phenomena of the circulation, have been too few to render their results of much value. Yet it may be in some degree satisfactory to you to know that I am not blindly repeating the remarks of others, and that what I *have* witnessed is perfectly in accordance with the statements of Kaltenbrunner, and affords me a strong assurance of his accuracy and fidelity. There is another reason, too, why I consider him the more trustworthy—he has no theories to which he might be disposed to bend or accommodate his facts.

Before I detail to you his account of the phenomena of congestion, I may briefly describe the scene which presents itself when the web of a frog's foot is looked at through a good microscope. It is a most beautiful and wonderful spectacle, and particularly interesting to those who, like ourselves, are desirous of gaining some insight into the healthy and diseased states of the circulation. It is a sight which I hope and believe you also will have many opportunities of seeing in this place. You perceive, then, occupying the circular field of the instrument, a number of blood-vessels, through which the blood, with its corpuscles or globules, is in active motion: and you see at once that there are three different kinds of vessels before you. First, you notice the blood shooting swiftly along tubes which divide and subdivide into smaller and smaller branches, each branch (speaking generally) going off at an obtuse angle: these are plainly arteries. Then, in another part of the field of view, you see the blood moving in the contrary direction, more slowly, in larger trunks, which are formed by the continual union and accession of smaller and tributary vessels of the same kind, that meet, for the most part, at acute angles; these you know to be veins: and all the intermediate and surrounding surface in view is occupied with other vessels or channels, which connect themselves with the ultimate ramifications of the arteries on the one hand, and with the primary radicles of the veins on the other, but which differ from both arteries and veins in these particulars—that they interlace and anastomose in all parts, in a very irregular manner, and at all angles, and that they retain everywhere the same uniform size. They neither collect into larger and larger trunks, nor separate into smaller and smaller branches, but are disposed like the threads forming the meshes of a net, except that the interstices are irregular in size and shape. These are the true capillaries, intermediate between the arteries and the veins, and perfectly distinct in character from each, but communicating and contiguous with both.

If now you press upon the animal's leg, so as to obstruct the circulation a little, the motion of the blood is retarded, especially in the capillaries. You see the red globules following one another slowly. These so-called globules are not really, in their standard degree of distension, little spheres; but circular discs, or flat cells. Sometimes one of them sticks to the side of a capillary channel, and dams up the current; other globules accumulate behind it, till at last they all pass on again together.

Now Kaltenbrunner irritates the web by pricking it, and soon afterwards the following appearances present themselves:—There is an increased afflux of blood to the part, so that arteries, veins, and capillaries, receive a column of blood two or three times as great as usual; the velocity of the blood is accelerated; the sides of the distended vessels seem to tighten round the stream of blood which they contain. With this alteration of the circulation, the natural functions of the part begin to be modified. The change of the blood from arterial to venous is interrupted. The

globules, passing with great rapidity through all the vessels, retain the characters of arterial globules even when they arrive at the veins; they present a bright color, show a tendency to stick together, and often form little clots, which pass through the capillaries and become visible in the veins.

One of the natural functions of the web is the secretion of a kind of lymph; but this secretion is now suspended. The parenchyma itself begins to be slightly tumid, and assumes a brighter tint than common.

All these phenomena begin from a circumscribed spot, of which the circumference gradually expands as the affection increases; and they cease insensibly at that circumference.

This is active congestion.

A certain period always intervenes between the first action of the irritant cause, and the commencement of true congestion. This period, the occurrence of which you will be good enough to bear in mind, Kaltenbrunner calls the period of *incubation*; the period in which the congestion is *hatching*. It seems probable, from observations since made by Mr. Paget, that the state of the blood-vessels during this period is a state of contraction or closure.

Active congestion, as such, does not continue long. It either passes on into inflammation, as I shall hereafter explain, or it begins to decrease. When it has been very slight, the quantity of blood, and the rapidity of its movement, diminish gradually from the circumference towards the centre; and in this way the congestion insensibly vanishes.

But, in other cases, when it has not been so slight, the congestion terminates by an evident crisis, which Kaltenbrunner thus describes:—The blood, receding from the circumference of the congested part towards the centre, gives out, by exhalation, a liquid. The exhalation takes place by fits, and here and there, through the sides of the capillary tubes, and generally on the surface of the organ. The moment of exhalation is very transient; but it is repeated often, and in different spots, until the congestion has disappeared. It is evidently critical, for the congestion is relieved and extinguished in proportion as the exhalation is repeated.

I shall follow these consequences of active and continued local congestion no further at present; but merely remind you again that the changes I have last mentioned constitute the earliest appreciable modification of structure leading or belonging to inflammation. What we thus may see (and it is what I myself have had some opportunities of seeing) in the transparent textures of animals, we reasonably infer to take place, under analogous circumstances, in those parts of the body which are internal and opaque, and consequently hidden from our view.

I will just observe, also, that as active congestion is the parent of inflammation, so it sometimes causes hæmorrhage, and is relieved by it. But comparing this form of congestion with another which I am about to mention, the connexion of hæmorrhage with it is, relatively, unfrequent.

One obvious mode of remedying this congestion is the mechanical abstraction of blood from the loaded part. But it is seldom that this measure alone suffices; and sometimes it would be ultimately hurtful to adopt it. The state of the constitution may be such, that the disposition to local plethora would be increased by the loss of blood. Undue susceptibility and disordered action of the nervous system are liable to be aggravated by bleeding; and in proportion as the nervous functions are irregularly performed, does the tendency to unequal distribution of blood in the capillary vessels augment. We have daily examples of this in hysterical young women. It is not, therefore, the mere congestion that we have to consider; we must look deeper, for its cause. Leave in the finger a small thorn: the blood will be collected there in consequence of its irritation, and will continue to collect in spite of depletion. But extract the thorn, and your remedial measure of taking away blood is at once successful. So it is also with internal congestions of blood—of which the exciting and sustaining cause is not always so well known.

Contrasted, in some important particulars, with active congestion such as I have been describing, is that morbid fulness of the capillary vessels which arises when the return of the blood from them towards the heart through the veins, is impeded by some *mechanical* obstacle. With this *mechanical congestion* the veins are exclusively concerned.

Congestion of this kind may be strictly local. It may be confined to a single limb, when the principal venous trunk belonging to that limb is compressed, or otherwise diminished in size; and when no collateral and compensatory channels for the returning blood have been established. If there be disease of the liver, of such a nature as to prevent a free passage of the blood through that organ, congestion will ensue in all those parts of the capillary system from which the blood is conveyed by the veins that ultimately concur to form the vena portæ. The force of gravity alone is sufficient to produce venous congestion, and consequently congestion of the capillaries, in parts of the body in which, under ordinary circumstances, the circulation through the veins is aided, instead of being opposed, by that force. If, for instance, the head be suffered to hang downwards for a certain time, we see the unequivocal signs of such congestion in the tumid condition and the purplish red colour of the lips, cheeks, eyelids, and ears. When an impediment to the free transmission of blood exists in the heart itself, a tendency to stagnation is produced, first in the venæ cavæ, then in the smaller ramifications by which these veins are fed, and at length in the general system of capillary vessels: and thus even general congestion may proceed from a fixed mechanical cause; the parts that are the most vascular being also the most readily and the most completely gorged.

There is yet a third form of local congestion, differing, in some respects, both from active and from mechanical congestion. The capillaries become loaded, and the course of the blood in them is languid and sluggish, without any previous increased velocity of the blood in the arteries, and independently of any mechanical obstacle in the veins. To this form of congestion the term *passive* is applied. Andral denominates it passive or asthenic hyperæmia. I will tell you the class of facts from the observation of which the real existence of this passive plethora has been ascertained.

In persons enfeebled by age, or by disease, the lower parts of the legs, the insteps and ankles, and the skin which forms the surface of old scars, are often habitually purplish, or violet-coloured. There is congestion of dark blood in those parts. You may, perhaps, be ready to ascribe this to the mere influence of gravity upon the blood, but this cannot be the whole explanation, because the force of gravity is the same with all persons, and at all ages. A horizontal position of the limb will perhaps diminish the livid redness, or may even sometimes entirely remove it. But the depending position ought not to cause it, and would not cause it, if the blood-vessel were in a healthy condition. Neither can the difference of posture be any source of *irritation* to the congested part. The capillaries themselves appear to have lost, in a great degree, their natural elasticity; they easily dilate under the pressure of the blood, which, being thus retarded, accumulates in the part. The employment of friction, or some stimulating application, will often remove this congestion.

I say all this is often to be noticed when there has been no cause of irritation operating upon the part, and no preceding state of active congestion. But it is important to mark the very frequent connexion that exists between these contrasted conditions. The one very often succeeds the other: the vessels become dilated under the force of the active hyperæmia, and, the irritation ceasing, they do not at once recover their tone, but remain passively loaded and distended. They are frequently left in the same state upon the subsidence of inflammation.

Take another illustration from what you may any day witness in respect to indolent ulcers. You will find that the large, flabby, and livid granulations which they present, may be made to contract and to assume a more healthy and florid hue, by local stimulants: these evidently act by quickening the previously languid circulation, and unloading the congested capillaries.

Observe, again, what not unfrequently happens in regard to the eye; a little organ indeed, but one that supplies us with more striking lessons in pathology and therapeutics than any other portion of the body. You know that the conjunctiva and sclerotica, through which, while healthy, colourless fluids alone circulate, are traversed, under various forms of disease, by innumerable vessels bearing red blood. Now, it is notorious that, in certain cases, the application of any stimulating substance to the surface of the organ will increase the existing redness, multiply the number of visible vessels, and aggravate the complaint. These are cases of active congestion, dependent upon irritation that is still subsisting. But it is equally well known to practical men that the blood-vessels of the eye are liable to congestion of a

very different kind. They are seen to be distended, somewhat tortuous, almost varicose, and the redness has a browner tinge, and is less vivid, than in the former case. In this kind of vascular fulness,—or in this *stage* of it, for it sometimes succeeds to active congestion,—emollient applications do harm rather than good, while strongly astringent and even irritant substances will often promptly dissipate the vascularity. These, again, are cases illustrative of congestion of the asthenic or passive character. The strong topical irritants restore to the feeble and relaxed vessels their natural elasticity, stimulate them to contract upon their contents, and to force onwards the red blood, which they cease to admit from the arteries; and the redness vanishes.

In the production of *active* congestion the arteries appear to be principally concerned: in the production of *mechanical* congestion, the veins. In *passive* congestion the capillaries—which, strictly, are neither arteries nor veins, but lie between the arteries and the veins—are the vessels chiefly in fault.

If we turn our thoughts from the visible textures of the body to those which are hidden internally, we shall find reason to believe that these also are equally liable to similar conditions of passive congestion. Take those exceedingly vascular organs, the lungs, through which the whole of the blood circulating in the living body has to pass. The lungs, as might be expected, are *very* liable to congestion and engorgement of their capillary vessels. Ofttimes this is clearly active, and the result of some irritating cause. But it is not always so. Many of you recollect the epidemic disorder called the influenza, which was so prevalent here in the spring of 1831, and again in the early part of 1837. Among the most constant and striking characters of the disease were the symptoms of pulmonary catarrh; and it was remarkable how long, in some persons, these symptoms persisted. After the pulse had regained its natural frequency of beat, and when all fever had ceased, the patient would continue to breathe with constraint and some labour, to wheeze a little, to cough, and to expectorate mucus. As all febrile disturbance had subsided, and no further benefit seemed to flow from adhering to what is called the antiphlogistic system, it was a reasonable conjecture that this disappointing obstinacy of some of the symptoms might depend upon a lingering but passive congestion of the pulmonary mucous membrane. And the nature of the *juvantia* showed the correctness of this conjecture. Tonics and stimulants, so far from aggravating the pectoral symptoms, speedily removed or abated them.

You cannot fail, I think, to perceive the important bearing of these distinctions between active and passive congestion upon our notions of disease and our choice of remedies. These distinctions are not to be discovered by the knife of the anatomist. You must take care not to confound a knowledge of pathology, in the proper sense of that word, with a knowledge of morbid anatomy. Pathology comprehends not only the visible changes of structure which accompany disease, and are disclosed by death, but the processes by which those changes are effected in the living body, and the laws which govern those processes.

There is one important law ascertained in respect to both active and passive congestion; viz., that it is apt to *recur*; that those parts are most likely to suffer it (or inflammation, which implies it) that have suffered it before. We may often turn our knowledge of this general fact to good account, in what is termed the *prophylaxis* of disease—in devising means for warding off disorders.

I have stated that *active* and *passive* congestion sometimes occur in succession, the latter being a sequel of the former. So, also, it may be said of *passive* and of *mechanical* congestion, that they often exist *together*. If the capillaries of a part or organ be much enfeebled, the mechanical effect of the gravity of the blood may suffice to bring them into a state of congestion. It is thus that Andral explains the occurrence of a gorged condition of the posterior portions of the lungs (evinced by symptoms during life, as well as by inspection of those parts after death), in persons who, having laboured under no previous pulmonary affection, have been confined to a supine position by long-continued disease or debility. This state of the capillaries is called by Lermnier the “engorgement of position;” and by Laennec, “the pneumonia of the dying.” It neither proceeds from irritation, nor has it the essential characters of inflammation; although it is apt to be considered an evidence of inflammation by the mere morbid anatomist.

Again, as active congestion, when continued or intense, is antecedent and condu-

cive to inflammation; so is mechanical congestion, when it reaches a certain point, the prolific source of hæmorrhage, and the almost constant precursor and immediate cause of a large class of dropsical accumulations.

I spoke a little while ago of general plethora, as a state in which the whole mass of blood circulating in the body is excessive in quantity, and rich in quality—full of colouring matter, thick with globules. But the blood, as a mass, may be in excessive quantity, yet poor in its materials, serous, deficient in globules, and fibrin, and colour; and in this condition of the blood also, as we shall hereafter see, dropsies are apt to arise.

We have now, therefore, laid the foundation for the better understanding of those three great classes of disease—*Inflammations, Hæmorrhages, and Dropsies.*

There is no region or organ of the body exempt from these diseased conditions and their consequences; and of each of them some general account must be given, before we come to consider the special diseases incident to the several parts and organs.

But previously to entering upon this general account of inflammation, of hæmorrhage, and of dropsy, we have still some other preliminary matters of importance to discuss. The causes and modes of death. The causes of disease. A sketch of the nature, classification, and import of symptoms.

Our inquiries hitherto have related to the manner in which the physical conditions of the various parts of the body are capable of being altered in disease, and their functions disturbed or suspended. But how it happens that some of these alterations of structure, or interruptions of function, are incompatible with the further continuance of life, and put a stop to the working of the whole machine, is an inquiry of no less interest, though of a somewhat different kind.

LECTURE V.

Different modes of Dying. Pathology of Sudden Death. Death by Anæmia, its Course, Phenomena, and Anatomical Characters. Death by Asthenia, its Course, Phenomena, and Anatomical Characters. Syncope. Death by Inanition. Death by Apnœa: Death by Coma: their Course, and Phenomena, and the Anatomical Characters common to both. Application of the Principles obtained from the investigation of the Phenomena of Sudden Death, in elucidating the Symptoms and Tendencies of Disease.

I PROPOSE to devote the present lecture to the following inquiry:—wherefore it is, and how it is, that some of the corporal changes which we have been considering, or the diseased conditions connected with those changes, come to be incompatible with the further continuance of life? how is it that they put an end to the working of the living animal machine? why the machine should not continue to work, though perhaps imperfectly, notwithstanding such changes?

When our watches stop, we take them to a watchmaker to ascertain why they have stopped. The watchmaker knows that there are various ways in which the movements of the instrument may have been arrested. The main spring may have broken; or the little chain that connects the barrel with the fusee may have parted; or the teeth of some of the wheels may have become inextricably entangled; or the watch may have ceased to go (as the saying is) simply because it has not been wound up. Now the examination which the watchmaker undertakes in respect to the watch, I am desirous of making in respect to the human body. I am going to inquire into the several processes and modes of dying—the steps, or ways, by which the vital functions of the body are extinguished. A very little experience in the sick chamber, or in the wards of a hospital, will suffice to teach you that, although all men must die, all do not die in the same manner. In one instance the thread of existence is suddenly

snapped; the passage from life, and apparent health perhaps, to the condition of a corpse, is made in a moment: in another the process of dissolution is slow and tedious, and we scarcely know the precise instant in which the solemn change is completed. One man retains possession of his intellect up to his latest breath: another lies unconscious, and insensible to all outward impressions, for hours or days before the struggle is over.

We seek to ascertain the mechanism and the laws of these mysterious differences.

The inquiry is not one of merely curious interest, but has a direct bearing upon the proper treatment of disease. It will teach us what we have to guard against, what we must strive to avert, in different cases. In speaking of particular diseases, I shall constantly refer to the facts and reasonings which I am now about to lay before you.

In pursuing this inquiry, we need not go into any deep physiological disquisition respecting the conditions that are essential to life. It is sufficient for our purpose to remark that life is inseparably connected with the continued circulation of the blood. So long as the circulation goes on, life, organic life at least, remains. When the blood no longer circulates, life is presently extinct: and our investigation of the different modes of dying resolves itself into an investigation of the different ways in which the circulation of the blood may be brought permanently to a stand.

Observe the ample provision that is made, in the construction of the body, for carrying on and maintaining this essential function. First, there is an extensive hydraulic apparatus distributed throughout the frame, — consisting of the heart and other blood-vessels. Next, there is a large pneumatic machine, forming a considerable part of the whole body, — composed of the lungs, and the case in which they are lodged. Lastly, the power by which this machine is to be worked and regulated is vested in the nervous system. Each of these systems must continue in action, or the circulation will stop, and life will cease. The functions they respectively perform are, consequently, called vital functions: and their main organs — the heart, the lungs, the brain (by which I understand the intercranial nervous mass) — are denominated vital organs. The functions of any one of the three being arrested, the functions of the other two are also speedily extinguished. But the phenomena of dying vary remarkably according as the interruption begins in the one or in the other organ. Hence Bichat, who in his *Recherches sur la Vie et la Mort*, laid the foundation of the distinctions I am about to describe, spoke of death beginning at the *head*, death beginning at the *heart*, and death beginning at the *lungs*. This nomenclature is, however, unsatisfactory and insufficient, as you will presently perceive.

That the heart may continue to propel the current of the blood, two things are necessary: first, a certain power or faculty of contracting; and, secondly, a sufficient quantity of blood in its chambers, to be moved, and also to stimulate them to contract. If this, the proper stimulus to the internal surfaces of the heart, be withheld, or much deficient, it will soon cease to beat. There are plainly, therefore, two ways in which death might be said to begin at the heart; and these require to be distinguished.

The respiration is entirely subservient to the circulation of the blood. The two organs, the heart and the lungs, respond intimately to each other. The whole of the blood is sent by the right heart to the lungs, simply that it may there be submitted to the chemical action of the atmosphere. The respiratory apparatus is added to the body for the sole purpose of thus repeatedly ventilating the blood.

To this purpose also (setting aside all accidental impediments) two things are requisite: first, circumfused air to enter and depart at short intervals; and, secondly, alternating movements of the chest, to cause its entrance and exit.

Now these movements, although they admit of being regulated by the will, are essentially involuntary. The ordinary acts of respiration depend upon a certain condition of the medulla oblongata. If this condition fail, the mechanical part of the respiratory process, and, consequently, the chemical part also, is arrested.

The respiration hangs, therefore, directly upon the nervous system.

On the other hand, the action of the heart is not directly or necessarily dependent upon any constant nervous influence proceeding from the brain or spinal cord. The circulation goes on in an acephalous foetus; it may be kept up, by maintaining artificial respiration, in a decapitated animal: nay, even when both brain and spinal cord have been abstracted from the body.

But though the cerebro-spinal nervous influence is not necessary to the movements

of the heart — further than as it is necessary to the respiration, and to the introduction of nutriment — it has been clearly ascertained that very sudden and extensive injury or shock to the great nervous centres may instantly paralyse the heart, and so stop its action.

Of the intercranial mass, then, it may be affirmed that there are certain states, which, without directly affecting the heart, bring the motions of respiration to a pause: and that there are certain other states which act directly on the heart and arrest its play. That is, there are two different ways in which death might be said to begin at the head.

Hence, I say, the nomenclature employed by Bichat is defective and inaccurate.

In order to see clearly the steps by which the circulation, and with it life, finally terminates, in the various forms of dying, we must study the problem under its simplest forms. We must examine the cases in which the vital functions are, each in their turn, *suddenly* stopped, by some known cause, operating upon this or that vital organ. We must take advantage of the experiment (if I may so call it) which is performed before our eyes whenever a healthy man is cut off at once by external violence, or by poison, acting directly upon a particular organ or system of organs. The inquiry might be assisted, and, indeed, it has been mainly carried on, by experiments made upon living animals of a similar conformation with man. But the pathology of sudden death is happily now too well understood to require any further recurrence to that painful mode of “interrogating nature.”

Death, when it results from disease, is usually complicated. Many parts are affected, and different functions languish, and various disturbing causes are in operation, at the same time. Occasionally, however, the process of dissolution is as simple and obvious as in death produced by violence; and in most cases some primary and predominant derangement may be traced of this or that vital function; and a tendency is more or less clearly manifest to one or the other of the modes of dying, which we may now proceed to consider in succession.

And first let us examine that form of death which is caused by a want of the due supply of blood to the heart. This is called, with much propriety, *death by anæmia*.

The best examples of death taking place in this way are those in which it is the consequence of sudden and profuse hæmorrhage. The circulation fails, not because the heart has lost its power of contraction, but because blood does not arrive in its chambers in sufficient quantity.

We assure ourselves of this in two ways. In the first place, when the body of an animal is examined immediately after death from sudden and copious loss of blood, the heart is not found dilated and full of blood, as it would be if it had ceased to act from a want of power to contract upon its contents; but it is found empty, or nearly so, and *contracted*. Secondly, this conclusion is confirmed by the reverse experiment: by the effect, I mean, of the *transfusion* of blood. It is a fact well ascertained, first by experiments made upon animals, and afterwards by most happy trials upon the human subject, that in cases of apparent death from violent hæmorrhage the suspended functions may be restored by conveying a timely supply of blood into the vessels of the seemingly dead animal from the veins of a living animal of the same species. Now it is quite clear that this introduction of fresh blood could be of no avail in a case where the heart was unable to act upon the blood which had already reached it.

The phenomena which attend this mode of dying are paleness of the countenance and lips, cold sweats, dimness of vision, dilated pupils, vertigo, a slow, weak, irregular pulse, and speedy insensibility. With these symptoms are frequently conjoined nausea, and even vomiting, restlessness and tossing of the limbs, transient delirium; the breathing is irregular, sighing, and, at last, gasping; and convulsions generally occur, and are once or twice repeated, before the scene closes.

It is thus that women often die, in whom “flooding” happens after childbirth. Sometime the sudden bursting of an aneurism occasions this form of death. It is common on the field of battle, and in accidental injuries whereby large blood-vessels are wounded. Internal hæmorrhage, depending upon diseases to be hereafter described, may also prove fatal in the same manner.

This, then, is one form of death beginning at the heart. Another form, the converse of this, but spoken of also as death beginning at the heart, is that in which there

is no deficiency of the proper stimulus to the heart's action, but a total failure of contractile power in that organ. This is well denominated *death by asthenia*.

Death occurring in this way is not uncommon. The effects of some kinds of poison furnish a good illustration of it. There are certain substances which, applied to some part or other of the body, speedily extinguish life: and when, after their fatal operation, the thorax is opened, each chamber of the heart is found to be filled with its proper kind of blood, upon which it has been unable to contract.

This was distinctly made out by Sir Benjamin Brodie, in his able and scientific investigation of the effects of different poisons. You may read with advantage his papers on this subject in the *Philosophical Transactions* for 1811 and 1812. He ascertained, upon examining the chest after death occasioned by the *upas antiar*, that the heart was not empty, but full, there being purple blood in its right, and scarlet blood in its left cavities. These are the anatomical characters of this kind of death; and they prove that the action of the heart has not ceased from a defect of the necessary stimulus, but from a loss of its contractile power.

The state of suspended animation common to both these forms of dying—the ultimate external phenomena being nearly the same in each, and the result in each being the simple failure of the circulation—is often expressed by the term *syncope*.

Besides the essential distinctions between them already mentioned, there is this further point of difference. In death by *anæmia*, the suspension of the functions of the nervous system arises from a lack of the blood which should be sent to the brain from the heart. Hence the well-known effect of mere position. Syncope is sooner produced by venæsection when the person bled is sitting up than when he is recumbent: and the first remedy for the fainting state is to lay the patient flat upon the ground, or even to place his head a little lower than the trunk of his body. In the one posture the current of the blood towards the head is impeded by the force of gravity; in the other it is not. In sudden death by *asthenia* this order is reversed; the *nervous system* is the part first affected, and through it, consecutively, the heart. This appears from the fact that sudden death by *asthenia* is sometimes produced by causes which we know to act primarily upon and through the nervous system; by strong mental emotion—as intense grief, joy, terror. Instances of fatal concussion, where the brain is jarred by some bodily shock—and death occurring almost in a moment from blows on the epigastrium—are of this kind: though in the latter case the mortal influence is probably conveyed through the ganglia of the great sympathetic nerve. Lightning and electricity kill too, when they kill at all, in the same way. And we shall hereafter see that certain varieties of apoplexy, and several other diseased conditions, destroy life by suddenly arresting the contractile power of the heart.

When death by *asthenia* occurs more slowly, from disease, the phenomena are somewhat different. The pulse becomes very feeble and frequent, and the muscular debility extreme; but the senses are perfect, the hearing is sometimes even painfully acute, and the intellect remains clear to the last. The tendency to death of this form is remarkably manifest in acute inflammation of the peritoneum, in what is called malignant cholera, and in cases of extensive mortification.

Akin to this form of dying is that in which the living powers are slowly exhausted by lingering and wasting disorders, as in many cases of phthisis, in diabetes, and in dysentery; or by hæmorrhages moderate in amount, but frequently repeated; or by any other long-continued drain upon the system. The death is partly, however, to be ascribed to a deficient supply of the natural stimulus to the heart's action. The type of these mixed modes of dying is seen in *death by starvation*, which may be considered intermediate between death by *anæmia* and death by *asthenia*. Death from inanition can never be sudden. The blood, renewed no longer from without, and fed only by absorption from the system itself, diminishes gradually in quantity, while its quality deteriorates. Gradually also the contractile power of the heart, as well as of the muscles generally, is weakened; and from these combined causes its movements at length cease. Accordingly, after death by starvation the heart is not found to be so much contracted, nor so nearly empty, as after death by sudden and copious hæmorrhage.

Certain diseases of the throat or of the œsophagus, prohibiting the introduction of food; of the stomach, preventing its retention; of the digestive organs generally, hindering its assimilation, are fatal in this manner.

We have yet to consider how death is produced by the suspension of the respiratory function—in other words, by a want of the due arterialization of the blood.

There are two perfectly distinct modes in which this cause of death may proceed; distinct, I mean, in regard to the steps of the process, although identical in regard to the ultimate result.

1. When the access of air to the lungs is suddenly denied by some direct obstacle to its entrance;

2. When the muscular actions required for breathing cease in consequence of *insensibility*, caused by disease or injury of the brain.

The first of these two forms of dying is commonly called death by *asphyxia*. The second is conveniently termed death by *coma*. Bichat spoke of them respectively as death beginning at the lungs, and at the head.

It is of much importance to get rid, when we can, of improper names. They are very apt to warp our notions concerning the real nature of the things which they are intended to express. This term *asphyxia*, though in everybody's mouth, is very inappropriate, if we look to its etymology, to the kind of death which it has come to denote. It signifies, literally, you know, pulselessness, the want of pulse; and therefore it *might* express any kind of death whatever; or if applied to any particular *mode of dying*, it would seem to belong to that which we have just been considering, namely, death beginning at the heart. And you will presently see that it is *peculiarly* inapplicable to all those cases where death results from the nonarterialization of the venous blood. Its current signification has, I am afraid, been too long established by custom, to allow of its being restored to its proper meaning without much confusion. But, at any rate, I can and shall avoid its use, and adopt in preference the generic term *apnœa* (privation of breath) as justly expressive of the mode of death to which the word *asphyxia* is commonly given by authors. The generic English term is *suffocation*.

The entrance of air into the lungs may be prevented in various ways: by stoppage of the mouth and nostrils (*smothering*):—by submersion of the same inlets in some liquid (*drowning*); or in gases which, though not in themselves poisonous, contain no oxygen; such are hydrogen and azote:—by mechanical obstruction of the larynx or trachea from within, as by a morsel of food (*choking*), or from without, as by the bow-string (*strangulation*); both these varieties are included in the term *throttling*):—by forcible pressure made at once upon the chest and abdomen, preventing all movement of the ribs and of the diaphragm; this happens sometimes to workmen employed in excavating, who are buried, their heads excepted, by the falling of a mass of earth; it was near happening, Dr. Roget tells us, to an athletic black man, of whose body, as an academic model, a cast was attempted to be taken, by one operation, and in one entire piece; “as soon as the plaster began to set, he felt on a sudden deprived of the power of respiration, and to add to his misfortune, was cut off from the means of expressing his distress; his situation was just perceived in time to save his life;” in this way the victims of Burke and Hare were stifled; and the same immovable state of the lung-case is sometimes produced in tetanus, or by the poisonous influence of strychnine, all the respiratory muscles being fixed in rigid spasm:—by paralysis of the same muscles, from injury or disease of the spinal cord above the origin of the nerves that give off the phrenic nerve, and therefore above the origin of the intercostal nerves also; or from section of the phrenic and intercostal nerves:—lastly, by such breaches in the walls of the thorax as admit air freely to the surface of both lungs, and spoil the pneumatic machine, as a pair of bellows is spoiled when deprived of its valve. Of course the same consequences ensue when both pleuræ become filled with liquid of any kind.

Whenever the privation of air is sudden and complete, the following external phenomena present themselves.—Strong but vain contractions occur of all the muscles concerned in breathing, and struggling efforts to respire are made, prompted by that uneasy sensation which every one has experienced who has tried how long he can hold his breath, and which, when unappeased, soon rises to agony. This extreme distress is transient, being almost immediately succeeded by sensations, not unpleasant, of vertigo, and then by loss of consciousness, and convulsions: at length all effort ceases, a few irregular twitchings or tremors of the limbs alone perhaps remaining; the muscles relax, and the sphincters yield; but still the movements of the heart,

and even the pulse at the wrist, continue for a short time after all other signs of life are over; there is no *asphyxia* (properly so called) till the very last.

During this process, which does not occupy more than two or three minutes, the face at first becomes flushed and turgid, then livid and purplish, the veins of the head and neck swell, and the eyeballs seem to protrude from their sockets; at length the heart ceases to palpitate, and life is extinct.

The internal changes, which correspond with and cause these outward symptoms, have been carefully studied, and accurately, though slowly and lately ascertained. They all proceed from the prevention of the chemical alteration naturally produced in the blood, within the capillary vessels of the lungs. The blood, continuing venous, passes at first in considerable quantity through the pulmonary veins, into the left side of the heart, and thence through the arteries, to all parts of the body. This venous blood however, loaded with carbonic acid, is inadequate to sustain, or sufficiently to excite, the functions of the parts it thus reaches. In the brain the effect of the unnatural circulation is felt at once; and shown by the convulsions and insensibility that ensue. The motion of the blood in the pulmonary capillaries is also, from the first, impeded, and its current gradually retarded, until it stagnates altogether; the lungs remaining full, the right chambers of the heart distended, and therefore less capable of contracting, while venous congestion becomes general. The main cause of this impediment in the lungs appears to be the check given to the diffusion of carbonic acid out of the air cells. The blood, charged with this gas, cannot pass readily through the pulmonary capillaries. Meanwhile the dark and languid stream, flowing more and more tardily and scantily into the left chambers, leads by its unnatural quality, as well as by its deficient supply, to feeble contractions; and this side of the heart is comparatively empty.

In this state, even after the heart has ceased to beat, but not *long* after, if the cause which has excluded the air be withdrawn, and fresh air readmitted—in other words, if artificial respiration be instituted—the blood in the pulmonary capillaries undergoes the required change, becomes arterial, begins again to pass onwards, and by degrees the circulation is restored, and the patient saved.

In this mode of death, the circulation is first arrested, and death truly begins, *in the lungs*.

When the carcass of an animal that has thus perished of apnœa is immediately afterwards examined, (so speedy an inspection of the human body being, for obvious reasons, seldom practicable or proper,) the left side of the heart is found to contain a small quantity of dark blood, while its right cavities are greatly distended, and the lungs, the cavæ, and the whole venous system, are gorged with blood of the same character. These are, in few words, the anatomical characters of sudden death by apnœa.

The pathology of this mode of dying has, I say, been thoroughly understood only of late. It will not be uninteresting, and may, I think, be useful, to trace briefly the successive steps by which the true explanation has been attained. Haller was of opinion that the quiescence of the lungs, consequent upon the cessation of the alternate movements of the thorax, formed a mechanical impediment to the further transit of blood through them; and that death resulted from obstruction of the circulation *in the lungs*. He was partly right; but he erred in supposing that the stream of blood was arrested absolutely, and at once, and by a mechanical obstacle. Apnœa, with all its peculiar phenomena, occurs, when atmospheric air is excluded, although the lungs continue to play; as in persons who breathe azote or hydrogen gas. It was clearly shown by Dr. Goodwin, in his *Essay upon the Connexion of Life with Respiration*, that the unærated blood passed through the lungs, and entered the left auricle and ventricle of the heart; but he thought that it went no further. His notion was that arterial blood is the only stimulus which can excite the contraction of the left cavities of the heart, and that when venous blood arrives in them, the organ becomes motionless; and no blood being sent to the brain, the person dies. Had this theory been true, the left chambers would be found full of blood after death (which they are not), and the mode of dying would not have differed essentially from that which we have already considered as death by asthenia. The well-devised experiments of Bichat carried the investigation a step further, and proved that the unærated blood not only reached the heart, but was propelled by the contractions of that organ to every part

of the body, through the arteries. Having applied a ligature upon the trachea of a living animal, he made a small opening in one of its carotid arteries. Presently the slender stream of blood that issued began to lose its florid tint, and to assume the dark colour of venous blood; but *it continued to flow*, and the afflux of this dark blood upon the brain was marked by convulsions and insensibility. Bichat conceived, therefore, the erroneous belief that the blood underwent *no* obstruction in its passage through the lungs, but that, remaining unpurified and venous, it acted as a poison upon every part to which it was carried by the arteries—first upon the nervous system, and ultimately (passing through the coronary arteries) upon the muscular substance of the heart itself. There are, however, two well-known facts, which upon this theory would be inexplicable—the comparative emptiness of the left chambers of the heart, and the restoration of the suspended functions by the timely performance of artificial respiration. The air could never reach and revivify or deplete the venous blood, stagnating in the capillaries of the heart. It was reserved for Dr. Kay¹ to correct the unsound parts of Bichat's doctrine, and to show that the blood begins to stagnate in the capillaries of the lungs, in consequence of its failing to undergo the change from venous to arterial; and that the movements of the left heart are brought to an end, principally by the deficient supply of blood from the lungs. His experiments tend moreover to prove that venous blood circulating through the arteries has no directly poisonous operation, but is capable, though much less effectually than arterial blood, of supporting in some degree the irritability of the muscles. A muscle will continue to contract longer when supplied with venous blood by its arteries, than when supplied with no blood at all. Doubtless, in death by apnœa, the movements of the heart are weakened, partly in consequence of the imperfect stimulus afforded by the venous blood that penetrates its substance; but the primary and main cause of the failure of the circulation seems to be the difficulty with which the non-arterialized blood finds its way through the capillaries of the lungs. This theory is consistent with all the phenomena observed. For a detailed account of the experiments and reasonings upon which Dr. Kay's conclusions are founded, I must refer you to his work on Asphyxia. More recently Mr. Erichsen has published, in the *Edinburgh Medical and Surgical Journal*, some well-devised and convincing experiments, illustrative of the series of changes which I have been describing.

Sudden death by apnœa is not very often witnessed as the result of disease. It sometimes is caused by a spasmodic closure of the rima glottidis. It is no uncommon consequence of accidents, in which the upper cervical vertebræ are broken or displaced. I have seen several instances of death rapidly produced, with all the symptoms of sudden suffocation, generally in intoxicated persons, in whom the chink of the glottis has been found closely plugged by a fragment of meat, which "had gone the wrong way."

But there are numerous forms of more chronic disease, in which the tendency to death by apnœa is plainly discernible, sometimes for a long while before their fatal termination arrives. And the phenomena are similar in character to those which are noticed when the struggle is short. We hear the patients complain of the "want of breath." We see how they labour to satisfy this want, when it becomes urgent, by the elevated shoulders, the dilating nostrils, the energetic action of all the muscles that are auxiliary to the respiration. We perceive by the dusky and loaded countenance, the livid lips, and ears, and eyelids, that the blood is but imperfectly arterialized. The diminished capability of such blood to support the functions of the brain is made evident by the vertiginous sensations and the delirious thoughts of the gasping sufferers; and after death we find the same distension of the right chambers of the heart, while the left are nearly empty—the same gorged condition of the pulmonary arteries and venous system generally, which constitute the anatomical characters of this mode of dying. These appearances are even more constantly visible in the dead body, when apnœa has been gradually produced, than after sudden suffocation; simply, I believe, because they are more *permanent*. After sudden death, however caused, the blood seldom coagulates; and the venous turgescence consequent upon rapid apnœa, although great at first, has time to subside and disappear before the body is examined.

¹ Now, Sir James Kaye Shuttleworth.

In protracted cases, death does not take place purely in the way of apnœa; the heart is weakened, and the nervous influence impaired by the continued circulation of imperfectly arterialized blood; but the symptoms belonging to apnœa are plainly predominant.

When (as is most common) the privation of air is incomplete, and a scanty and insufficient supply is admitted, morbid changes take place in the lungs themselves; the air-tubes and cells become charged with serous fluid, which operates as an additional cause of suffocation. The same phenomenon is observed when the *par vagum* is divided on both sides.

Death by apnœa in disease is extremely common. It may be produced by anything which narrows the chink of the glottis; by warts that sometimes grow there, by œdema of the sub-mucous tissue of the larynx; by inflammatory tumefaction of its lining membrane: it may result from the presence of what are called false membranes in the windpipe and bronchi, such as are formed in the distemper named croup: it may be the consequence of disease situate in the substance of the lungs themselves, rendering them incapable of receiving the requisite quantity of air; of this we have examples in pneumonia, and in pulmonary apoplexy: or it may proceed from disorders of the pulmonary mucous membrane, the air passages becoming choked up with excessive and unnatural secretions, as in bronchitis. Phthisis is sometimes fatal in the way of apnœa; more commonly it tends to death by asthenia. Diseases of the pleuræ attended with effusion, and causing pressure upon the lungs; diseases of the heart and great thoracic blood-vessels, affecting the quantity of blood in the same organs; even certain abdominal maladies, accompanied by swelling, and thrusting the diaphragm upwards—terminate by the same mode of dissolution.

Death by *coma*, although common enough, and of much importance to be understood, need not detain us long. Certain morbid states of the brain (it is unnecessary at present to inquire into their nature and origin) produce stupor, more or less profound; the sensibility to outward impressions is destroyed, sometimes wholly and at once, much oftener gradually; the respiration becomes slow, irregular, stertorous; all voluntary attention to the act of breathing is lost, but the instinctive motions continue; the stimulus conveyed by the pulmonary branches of the eighth pair of nerves, and probably by certain branches too of the fifth, still excites, though perhaps imperfectly, the reflex power of the medulla oblongata, which sustains the involuntary movements of the thorax. At length this function fails also—the chest ceases to expand—the blood is no longer aerated—and thenceforward precisely the same inter-nal changes occur as in death by apnœa.

You will observe that the extinction of *organic* life takes place in exactly the same manner in both cases; the difference between the two forms of dying being this—that in death by apnœa, the chemical functions of the lungs cease first, and then the circulation of venous blood through the arteries suspends the sensibility; whereas, in death by coma, the sensibility ceases first, and in consequence of this the movements of the thorax, and the chemical functions of the lungs, cease also. So that the circulation of venous blood through the arteries is in the one case the cause, and in the other the effect, of the cessation of *animal* life.

The causes that destroy the sensibility leave no constant or necessary traces of their operation. The essential anatomical characters of death by coma, and of death by apnœa, are therefore the same.

Death occurring in the way of coma has this peculiar kind of interest belonging to it, that it may sometimes be effectually obviated by a mechanical expedient. The circulation ceases because the actions of respiration cease—and the failure of the acts of respiration arises from a suspension of the nervous power. If it be merely a suspension—if the nervous functions pause within the verge of recovery—organic life may be sustained by the performance of *artificial* respiration, until the insensibility has passed away; and thus the danger to life, which depended on that insensible state, may be escaped.

Many years ago, in the course of those researches to which I have already referred, Sir Benjamin, then Mr. Brodie, was led to think that by continuing the respiration artificially in animals labouring under the influence of narcotic poisons, the heart might be kept in action until the stupefying but transitory effect of the poison upon the brain had gone off. This idea he soon brought to the test of experiment,

and the result was such as to justify his ingenious reasoning. He inserted some woorara into a wound which he had made in a young cat. After a certain time the respiration had entirely ceased, and the animal *appeared to be dead*, but the heart could be *still felt beating*. The lungs were then artificially inflated about forty times in a minute. The heart continued to beat regularly. When the artificial breathing had been kept up for forty minutes, the pupils of the cat's eyes were observed to contract and dilate upon the increase or diminution of light, but the animal remained perfectly motionless and insensible. At the end of an hour and forty minutes there were slight involuntary contractions of the muscles, and every now and then there was an effort to breathe. At the end of another hour the animal, for the first time, showed some signs of sensibility when roused, and made spontaneous efforts to breathe twenty-two times in a minute. The artificial breathing was, therefore, now discontinued. She lay, as in a state of profound sleep, for forty minutes longer, when she suddenly awoke, and began to walk about.

Sir Benjamin Brodie had indeed been anticipated in this happy proposition for recovering persons apparently dead after taking narcotic poison, after submersion, and the like,—although he does not seem to have been aware of it. The experiment had once been tried before, and on the human subject, and with success, though not upon such scientific principles. The case is given by Mr. Whately, in the *Medical Observations and Inquiries*, vol. vi. A man who had swallowed an immense quantity of solid opium, and who seemed to be dead, was restored by the patient perseverance, on the part of his medical attendants, in a process of artificial breathing.

It is seldom that we can hope for success from this expedient in the treatment of disease; simply because, in most cases, the injury of the nervous system which has produced the insensibility, is irretrievable.

In most forms of apoplexy, and of hydrocephalus, death occurs in the way of coma. Sometimes, however, as I mentioned before, the damage to the nervous substance is so extensive and sudden, as to operate like a shock, and cause death by asthenia. The tendency to death by coma is also strongly pronounced in sundry affections of the brain, both acute and chronic. These will form subjects for our consideration hereafter.

The several modes of dying, then, in cases of sudden death, are clearly enough made out. Let me briefly sum up the conclusions at which we have arrived. Life cannot be maintained without the circulation of arterial blood: and whenever a person dies, he dies either because *no blood circulates through his arteries*, or because *venous blood circulates through them*.

When it comes to pass that no blood is circulated through the arteries, we say that death occurs in the way of syncope; and this is of two kinds. In the one there is not blood enough received by the left side of the heart to stimulate its chambers to contract, or to be sent onwards by their contraction; in the other there may be blood enough, but the heart has not sufficient power to contract upon it.

Also there are two ways in which death may be brought about in consequence of the circulation of venous blood through the arteries. In one of these, the first step is the sudden shutting out of air from the lungs; the blood which arrives in those organs is not aerated, or rendered arterial, but circulates again as venous blood, producing a failure of the animal functions, and weakening the muscles, till it finally stagnates in the capillaries of the lungs themselves. In the other, the animal functions are the first to suffer—insensibility occurs—the power which governs the movements of respiration is in abeyance—the breathing ceases—and organic life is extinguished as in the former case.

I trust you even already perceive that a right understanding of these matters is calculated to throw both light and interest upon our study of the symptoms, and of the tendencies of disease. It will enable us to aim with more precision at fulfilling the indication so often inculcated by Cullen, of “obviating the tendency to death.” In this sketch I have merely been able to hint at the important bearings of such views upon our practice. My attention was first called to them by the lectures of my respected instructor, Dr. Alison, who was accustomed to illustrate them by reference to the phenomena of one large class of disorders. All the modes of dying that I have described are apt to take place in *fevers*. Sometimes we have to combat the one, sometimes the other tendency. The disease often proves fatal in the way of coma;

this happens principally when the brain has been a good deal affected, when there has been much headache, delirium, and stupor: sometimes, when the lungs have been seriously implicated, life is extinguished in the way of suffocation or apnoea: and occasionally fever seems to terminate fatally in the way of syncope, especially when the stress of the disorder has fallen upon the bowels, when there has been continued diarrhoea, and ulceration of the intestinal glands. Or if death do not occur precisely in these ways, at least it resembles more in different cases, sometimes one form of dying, sometimes another.

It is notorious that very different remedies, and even different plans of treatment, have been strongly recommended, in fevers, by different practitioners. Of this diversity in practice and opinion it is a probable explanation that one plan has been found the most proper to avert the fatal event in one form of the disease, and one in another. The tendency to a particular mode of death will prevail in, and characterize, a whole epidemic. We shall resume these considerations hereafter: in the mean time the facts we have been reviewing may teach us the danger of applying, with too much confidence, the experience we may have gained of one epidemic to the treatment of another; and the risk we sometimes incur of misjudging, and criticising unjustly, the practice recommended by other physicians, because it does not appear to accord with the results of our own observation.

LECTURE VI.

Causes of Disease: distinction between predisposing and exciting causes. Enumeration of causes, as connected with the Atmosphere—Food and Drink—Poisons—Exercise—Sleep—Mental and Moral Conditions—Hereditary Tendencies—Malformations. Temperature. Effects of Heat and of Cold.

THE *causes of disease* are commonly arranged under three heads—as predisposing, exciting, or proximate.

Of these three, the last mentioned, or the *proximate* cause, is nothing else than the actual disease itself—the actual condition of that part of the body, from which the whole train of morbid phenomena essentially flows. When we know that part, and that condition, we name the disease accordingly. It may be *inflammation* of the *lungs*; or *softening* of the *brain*. When we do not, we call the complaint after the group of symptoms by which it is characterized; *intermittent fever*, perhaps; or *marasmus*. The term “proximate cause” is, therefore, an unnecessary term: it is moreover, to learners, a puzzling term, and tends to give to the study of disease a scholastic and repulsive aspect. I wish you to get into the habit of contemplating the whole science of medicine under its simplest and plainest form. I am sure we may very well banish the term “proximate cause” altogether; and having now given an explanation of its meaning, for your guidance when you meet with it in books, I shall never employ it, in these lectures, except perhaps in a quotation, again.

In strictness of language, one event is held to be the cause of another event which follows it, when the first being absent, the second never occurs; and the first being present, the second never fails to occur, unless some other event intervene to prevent it. But the causes of disease will not bear to be spoken of after so strict a fashion. We perceive that certain external circumstances (*quæ nos circumstant*) often precede such and such diseases; and that the diseases seldom happen when the same circumstances were not previously observable; and we begin to regard those circumstances as exciting causes of those diseases. We find that the diseases are much more frequent among persons known to have been exposed to the agency of the suspected causes, than among persons who are not known to have been so exposed. The evidence at first is presumptive only. But the more uniform their conjunction, and

the more rare their disjunction, the more confidently do we assign to the two consecutive events the relation of cause and effect. By this kind of observation a number of exciting causes of disease have been clearly established to be such.

But recollect, certain circumstances being present, such and such diseases do *often*, not *always*, follow. Some persons are more liable to be affected by the operation of many of these ascertained causes than others are; and the same person is more liable to be influenced by the same cause at one time than at another. And special circumstances, existing in particular cases, will be found to account for this variable operation of known exciting causes upon the bodily health. The special circumstances may properly be called *predisposing causes*. Thus, of twenty persons exposed to the same noxious influence — to the combined agency of wet and cold during a shipwreck for example — one shall have catarrh, another, rheumatism, a third, pleurisy, a fourth, ophthalmia, a fifth, inflammation of the bowels, and fifteen shall escape without any illness at all. A man does that with impunity to-day, which shall put his life in jeopardy when he repeats it next week. It is not, therefore, the exciting cause *alone* that in all cases determines the disease. Something — nay much, or all — will frequently depend upon the *condition of the body* at the time when the exciting cause is applied; and this condition of the body, which we call *predisposition*, results from circumstances then or previously in operation: and these circumstances are, in our language, *predisposing causes*.

Do not confound, as many seem to do, the predisposition with the circumstances creating it. The predisposition is a certain state of the body — the predisposing cause is what produces that state. The *cause* of the predisposition is the *predisposing cause* of the disease. A predisposing cause may therefore be defined to be anything whatever which has had such a previous influence upon the body as to have rendered it unusually susceptible to the exciting causes of the particular disease.

It is sometimes difficult, or impossible, to say of a given cause whether it ought to be ranked among the exciting or among the predisposing causes; whether it have prepared the system for being affected by some other agent, or whether it have itself produced the disease; but for the most part the distinction is real, and sufficiently well marked, and of great importance to be attended to.

Disease may sometimes be averted, even in despite of strong and fixed predisposition to it, if we know, and can guard against, the agencies by which it is capable of being excited. A man may inherit a proclivity to consumption, yet fortunately escape that fatal complaint by timely removal to a warm and equable climate, and by other suitable precautions; that is, by avoiding whatever tends to rouse the dormant tendency into action. On the other hand, disease may often be warded off, notwithstanding the presence and application of its exciting cause, when its predisposing causes are ascertained and can be prevented. In proportion as the body is weakened or exhausted, it yields more readily to the pernicious influence of contagion, or of malaria; but by obviating all causes of debility, and fortifying the system, we walk with comparative security amid surrounding pestilence.

Diseases sometimes occur when no exciting cause, when no cause at all, has been apparent. All that we can say of such cases (which are not, however, very frequent) is, that the causes have not hitherto been discovered.

Now the ascertained causes of disease are many and various. Whatever ministers to life, health, or enjoyment, may become, under changing circumstances, the medium of pain, disease, or death. The atmosphere, in which we are constantly immersed, is full of dangers. Both the organic and the inorganic world of matter around us abound in poisons; they lurk in our very food, which becomes pernicious when taken in excess, or when it consists of certain substances, or certain admixtures of substances; so that there really was much truth, as well as some humour, in the startling motto to Mr. Accum's book on adulterations — "*There is death in the pot.*" Our passions and emotions also, nay, even some of our better impulses, when strained or perverted, tend to our physical destruction. The seeds of our decay are within as well as around us.

Let us enumerate, however, a little more particularly, the various known sources of disease, with the view of making, afterwards, a few practical comments upon some of them.

I shall pass over, in this enumeration, nearly all *chemical and mechanical injuries*; inasmuch as these belong to surgery.

If we look to atmospherical causes, we shall find that those varieties in the state of the air which proceed from mere differences of degree in its natural qualities may be productive of disease. Such are, extremes of heat and of cold; sudden variations of temperature; excessive moisture or excessive dryness; different electric conditions; differences of pressure as measured by the barometer; a deficiency of light.

Again, the atmosphere may be a source of disease in consequence of its being loaded with impurities. Malaria, contagions of various kinds, and noxious gases in general, may be considered as so many poisons.

Under the head of *nutriment* we may place the use of food of which the quality is bad and hurtful; this cause also strictly belongs to the class of poisons. Again, an insufficient supply of healthy food; and still more common causes are *excess in eating and intemperance in drinking*.

The numerous *poisons* which are not comprehended under either of the foregoing heads are also prolific sources of disease.

Another great class among the causes of disease might be formed by considering together the influence of various *trades and avocations* which are directly injurious to the health of those who pursue them.

We know, by ample experience, that a certain amount of bodily *exercise* is essential to good health. We see the evil consequences of much overstepping that amount, in the deformities and disorders which result from labour too severe, or too long continued. But a much more numerous train of complaints follow the opposite state—that in which, from indolence, or from necessity, but *little* exercise is used.

Excessive indulgence in *sleep* on the one hand, and long continued want or interruption of repose on the other, are apt to give rise to serious maladies.

Very many diseases have a mental origin. Excessive intellectual toil—the domination of violent passions—the frequent recurrence of strong mental emotions—vicious and exhausting indulgences,—each and all will sap the strength, and grievously impair the health of the body: and perhaps there is no cause of corporal disease more clearly made out, or more certainly effective, than protracted anxiety and distress of mind.

When we add to this catalogue of the sources of disease all those morbid tendencies which are *hereditary*—and those which flow from original *malformation*, and are congenital—we shall have a tolerably complete list of the manifold dangers to which our mortal frames are continually liable.

There are several points of view under which the consideration of these causes of disease might be shown to be interesting. We might inquire, for example, which of them are commonly predisposing, which exciting causes; and what are the circumstances which are found to render the same agent at one time merely a predisposing, and at another time an exciting cause.

We might also separate, with some advantage, those causes of disease to which the human body is often and necessarily exposed, from those which consist in agencies that are of local or temporary existence only. But without multiplying these artificial distinctions, I shall take occasion to advert to them either when speaking more in detail of particular causes, or when speaking of the disorders they have produced.

In our investigations into the causes of disease, great caution is necessary in order to avoid being misled by individual cases. The circumstances capable of influencing the bodily health are so various—so many of them are apt to be in operation at the same time—and so little power have we of excluding them, one after the other, so as to ascertain the exact efficacy of each—that our observations respecting their relative or their actual effects are open to much fallacy. We endeavour to escape this source of mistake by repeating and multiplying our observations. But it is by tracing diseases as they affect considerable masses of men, placed as nearly as possible under the same external circumstances, that we gain the surest and most satisfactory evidence in respect to the causes of disease. And hence it is that the experience of the medical officers of our fleets and armies is so valuable. Dr. Alison has well remarked, that all the circumstances of the whole number of men whose diseases fall under the notice of military and naval practitioners are, in many respects, exactly alike: the men are generally healthy adults in the first instance—the circumstances in which

they are placed are thoroughly known to the observer—and indeed are often to a certain degree at his disposal; they are often suddenly changed also—and changed sometimes as to one portion of the whole mass of individuals, while they remain unchanged as to another portion; so that his opportunities of observation partake in some measure of the nature of experiments, and being made upon a large scale, they are especially interesting and conclusive. In point of fact a great deal has been learned, with absolute certainty, upon this subject.

Hitherto I have simply *enumerated* the principal causes of disease:—but conceiving a bare enumeration of this kind to be of but little use, I shall inquire somewhat more nearly into the nature and mode of operation of several of them *now*: of others I prefer to speak in connexion with the particular diseases to which they give rise.

You will not consider the inquiry superfluous. To know the cause of a disease is sometimes to be able to *cure*, often to be able to *prevent* it. In some cases the cause is beyond our power, but an acquaintance with its nature may teach us how to moderate or to remedy its consequences. There are many diseases also over which medicine has very little control, but the causes of which, when ascertained, may be avoided, counteracted, or extinguished. Such causes, when they do not happen to be removable by individual efforts, are often susceptible of extinction by the united measures of a community. For this reason it is very desirable that correct opinions respecting the causes of disease should be widely diffused among the public;—and there is no way in which information of this kind is so likely to be made generally known, as by communicating it to medical students who are about to scatter themselves in all directions over the face of the land.

I shall proceed, then, in the first place, to the consideration of *heat and cold*, as external agencies capable of producing disease.

The range of atmospheric temperature compatible with human life is very considerable. Its limits are probably just those extremes of heat and cold that belong to the lower strata of the air in the different parts of the planet on which man is destined to exist. Under the burning sunshine of the tropics, and amid the profound frost of the polar regions, we alike find human dwellers. These different degrees of external temperature impress indeed peculiar physical characters upon those who are subjected to them, but they do not, of necessity, extinguish life, or even cause disease. It requires more care, however, to preserve life under intense cold than under intense heat. In some parts of India the temperature ranges for a long time together from 80 to 100, and even 110° of Fahrenheit's thermometer: I believe it sometimes reaches 120°. We can form some estimate of this heat by remembering the oppressive effect of the lowest of these temperatures—that of 80°—to which the thermometer sometimes rises in this country in the hot weather of summer. But these tropical climates are very thickly peopled. In the arctic countries, on the other hand—in the northernmost parts of America, for example, where the sun appears above the horizon for a short part of the year only, and where the thermometer sinks to 40 or 50° below zero—we still find inhabitants indeed, but they are few, and thinly scattered. This mainly depends, no doubt, upon the scanty supply of human food in those parts of the world: but something also is to be ascribed to the depressing influence of extreme cold upon the vital powers. The deficiency of human food is itself owing to the restraining effect of a low temperature upon organic life. Under a degree of temperature a little greater than that at the equator—or a little less than the lowest around the poles—it seems probable that man would soon perish. And in this fact we have one striking instance of the adaptation of external nature to the physical constitution of the human race.

But *for a short time*—and *under certain circumstances*—man is capable of enduring a very much higher degree of heat than the open and general atmosphere ever attains even in the hottest portions of the earth. Whether he could continue to exist, even for a little while, under a much more intense cold than ever occurs naturally on the surface of the globe, is more questionable.

It was long believed that the human body could not be safely exposed, even for a short time, to a degree of heat much exceeding that which is met with in hot climates. This opinion, which we now know to have been erroneous, was strengthened

by the result of some experiments made by the celebrated Fahrenheit himself, and related by Boerhaave in his *Chemistry*. Some animals were shut up in a sugar-baker's stove, where the mercury stood at 146° . A sparrow died in less than seven minutes, a cat in rather more than a quarter of an hour, and a dog in about twenty-eight minutes. The *noxious air* of the stove had probably more to do with the speedy deaths of these animals, than the heat. The truth, upon this subject, may be said to have been discovered by accident. In the years 1760 and 1761, M.M. Duhamel and Tillet were appointed to devise some means of destroying an insect which consumed the grain in the province of Angoumois, in France. They found that this could be done by subjecting the corn, and the insects contained in it, in an oven, to a degree of heat great enough to kill the insect, but not so great as to hurt the grain. In order to ascertain the precise heat of the oven, they introduced into it a thermometer placed upon the end of a long shovel. The mercury, when the thermometer was withdrawn, was found to indicate a degree of heat considerably above that of boiling water. But M. Tillet was aware that the thermometer had sunk several degrees as it was drawn towards the mouth of the oven. While he was puzzled to invent some way of determining more exactly the actual degree of heat, a girl, who was one of the attendants on the oven, offered to go in, and to mark with a pencil the height at which the mercury stood. And she did enter the oven, and remained there two or three minutes, and then marked the thermometer at 100° of Reaumur, which nearly equal 260° of Fahrenheit. M. Tillet then began to express some anxiety for the safety of the girl, but she assured him that she felt no inconvenience, and stayed in the oven ten minutes longer, during which time the mercury reached the 288th degree of Fahrenheit's scale—denoting 76° of heat above that of water when it boils. When she came out her complexion was considerably heightened, but her respiration was by no means quick or laborious. This experiment was afterwards repeated. Another girl remained in the oven as long as the former had done, at the same temperature, and with the same impunity. Nay, she even breathed, for the space of five minutes, air heated to about 325° of Fahrenheit—or 113° above that of boiling water.

The publication of these facts naturally excited the curiosity of scientific men, and other experiments were soon instituted. Dr. Dobson, of Liverpool, and several other persons with him, shut themselves up in the sweating-room of the public hospital there, the air having been heated till the quicksilver stood at 224° of Fahrenheit. They did not experience any oppressive or painful sensation of heat. Dr. Fordyce and Dr. Blagden made some remarkable trials of the same kind. They entered rooms artificially heated to a very high degree, sometimes naked, and sometimes with their clothes on, and bore the extraordinary temperature of 240° , and even 260° , for a considerable time, with very little inconvenience. In all these experiments it was found that the animal heat as ascertained by thermometers placed under the tongue, or grasped in the hand, was scarcely increased at all, and the respiration but little affected: but the pulse was very much quickened. The frequency of Dr. Blagden's pulse in one instance was doubled. You may read a detailed account of these experiments in the *Philosophical Transactions*; but to give you a more lively notion of the degree of heat to which the bodies of these gentlemen were exposed, I may tell you that their watch-chains, and other pieces of metal about them, became so hot that they could scarcely be touched; when they breathed upon the thermometer, the mercury immediately *sank* several degrees; each act of expiration produced a pleasant feeling of coolness in the nostrils, and they cooled their fingers by breathing upon them. In and by the same heated air which they respired, eggs were roasted quite hard in twenty minutes, and beef-steaks were dressed in thirty-three minutes; and when the air was blown upon the meat by means of bellows, it was sufficiently cooked in thirteen minutes.

It is ascertained, then, beyond all doubt, that the human body is capable of sustaining these very high degrees of temperature, *for a short time*, without detriment.

Facts of this kind may, perhaps, appear to you rather curious than useful. Man is never submitted to any natural heat of the air even approaching towards that to which the authors of the experiments I have been describing voluntarily exposed themselves. But a knowledge of extreme cases always tends to throw light upon those that lie *between* the extremes; and the direct results arrived at in these philosophic inquiries are not barren of practical utility to members of our profession. It

is not long ago that a man was found almost dead in an oven: he expired a quarter of an hour after he was taken to one of the Borough hospitals; and an inquest was held upon his body. The newspaper report of the case (which is the only one I have seen) states the temperature of the oven to have been about 120° —a candle was melted by it in half a minute. Now, prior to the trials just mentioned, exposure to such a degree of heat would have been held a sufficient cause of death. We now know (and it would be discreditable if we could not support our opinion in a court of law, or before a coroner, by a reference to authentic facts) that something else must have concurred in extinguishing life: and, in fact, it turned out that the man was *drunk* when he went into the oven.

But what are the effects upon the human frame, of a high, yet less excessive, temperature of the air?

One very constant effect of heat is that of stimulating the *organic functions* of the body. We have seen that the temporary application of great heat accelerates remarkably the action of the heart: the pulse was uniformly found to be much increased in frequency in the persons who made trials of their powers of endurance in heated rooms. We have evidence to the same purpose in the annual changes that take place in the vegetable kingdom at a given place, the summer renewing its foliage, the winter checking and repressing it; and still more in the superior luxuriance of vegetation in warm climates as compared with cold. And the same observation applies to those functions which animals possess in common with plants. Towards the poles both man and the lower animals are smaller than at the equator. Linnæus remarks that the hares, partridges, and other animals which inhabit the northern climes, are considerably smaller in size than the same species in more southern countries. And Mr. Tooke, in his *View of Russia*, observes, "As we approach nearer to the north pole, both the animal and vegetable productions of nature become more and more stunted. The ordinary stature of the Samoyedes seldom exceeds four or five feet, and their whole exterior corresponds with their dwarfish size." The stature of the native inhabitants of hot climates does not, I believe, exceed that which is proper to the temperate zone; but if, as is generally supposed, the human body, like plants and fruits, grows faster, and ripens sooner, in proportion as we approach the equator, this must be attributed to the stimulus of heat acting upon the organic life.

On the other hand, and in some sort as a contrast with this, we may observe that considerable heat, when applied for some time together, has a sedative or depressing influence upon the *animal functions*, *i. e.*, upon the nervous system; causing languor and lassitude, want of energy, a disinclination to exertion both bodily and mental.

Under favourable circumstances, and where due precaution is exercised, it is probable that a very high degree of natural temperature of the atmosphere may be borne with impunity. Sir James M'Grigor informs us (in his account of the passage of the army in 1801 from India to Egypt), that during the march over the sandy desert of Thebes, where the heat was *uniform*, though the thermometer in the soldiers' tents was as high 118° , the health of the troops was equal to that which they had enjoyed in any former period in India.

But there are some forms of disease which are distinctly traceable to heat as their cause.

The effect of hot weather in promoting the cutaneous perspiration is notorious. By the same influence the hepatic function is rendered more active. Dr. James Johnson first, I think, distinctly pointed out the sympathy or consent that obtains between the liver and the skin, under varying conditions of external warmth. Whatever may be the explanation of the fact, experience has taught us that a high atmospheric temperature, when its operation is continued for some time, has a marked influence upon the liver, increasing the quantity of bile that is secreted, and altering its sensible qualities; and this disturbance of function is not unfrequently followed by inflammation of the gland itself. In this country we witness, almost annually, the effects of a succession of sultry weather, in those attacks of vomiting and diarrhœa which are so common towards the latter end of summer, and in the autumn, especially when the season happens to have been unusually hot; and which result, apparently, from the excessive quantity or a morbid state of the bile. The English cholera (a totally different disorder from that which has of late years been called, most improperly, *the cholera*) is, as you know, so frequent and general in some years, as to be fairly con-

sidered and termed an epidemic disease. In tropical climates the same morbid operation of external heat is still more conspicuous; leading not only to violent disorder of the stomach and intestines, with the evacuation of large quantities of vitiated and acrid bile, but also to acute inflammation of the liver going on to suppuration, and the formation of large abscesses. These last diseased conditions are extremely rare in this latitude. The yellow complexions of those who return to England after a long residence in India, are to be attributed to that disordered state of the liver, and of its functions, to which such persons are proverbially subject, and which has in them been brought on by the influence of a hot atmosphere, operating for a long space of time together. Hepatic affections, acute or chronic, are among the chief diseases to which Europeans, at least, are liable in that climate.

We have here an example of the distinction I wish you to notice between predisposing and exciting causes. The heated atmosphere stimulates unduly the secreting function of the liver. Now a secreting organ is never so apt to be affected by any exciting cause of inflammation as when the process of secretion is going on. This law, which I mention by anticipation, depends, no doubt, upon the increased afflux of blood that accompanies the act of secretion. The excessive activity of the hepatic function constitutes thus a predisposition to inflammation of the liver. The hot atmosphere, which creates this predisposition, holds the place of a predisposing cause in respect to the inflammation that ensues; but the exciting cause is exposure to cold: one of the most common and best ascertained exciting causes of inflammation in general. You are not to imagine that there can be no such thing as exposure to cold in a climate where the temperature of the air is habitually above 80°. Dr. James Johnson, in his book on *Tropical Climates*, observes that on the coast of Coromandel the temperature is steady by day, and the nights are hot; but yet, he says, nothing is more common than *exposure to cold* in this place. The European soldier or sailor, after the heat occasioned by his employments in the day, strips off his clothes, and lies opposite a window or port, his shirt wet with perspiration, to enjoy the sea breeze at night. And the same author tells us that the application of cold after or during perspiration, commonly produces an attack of hepatitis in some one of its various forms. Now the effect of that kind of exposure here described does not depend upon the *actual* temperature, but upon the *sensation* that is produced, and the sensation depends upon the *relative* temperature; and there can be no doubt that, under the circumstances mentioned by Dr. Johnson, a strong sensation of cold would be occasioned, even by a sea breeze as warm as 80°, or warmer. Changes of temperature seem to be as readily felt at one part of the thermometric scale, as at another, and in whichever direction they take place. Dr. Walsh states, that while sailing along the coast of Brazil, after having been long accustomed to a temperature of 72°, a strong breeze set in from the sea, and the thermometer fell to 61°; (*i. e.*, to a point which we should here call temperate;) “but,” he says, “the sense of cold from the sudden transition of temperature was quite painful. After bearing it for some time shivering on deck, it became intolerable, and we all went below, put on warm clothing, and dreadnoughts—and again appeared with thick woollen jackets and trowsers, as if we had been entering Baffin’s Bay, and not a harbour under one of the tropics.”

It is interesting to compare this statement with Capt. Parry’s account of a change of temperature at the opposite extreme of the scale, and in the other direction. Having previously said that the thermometer had fallen to 13° below zero in the night of the 21st of October, he goes on thus—“The wind veering to the south-east on the 24th and 25th, the thermometer gradually rose to 23°. I may possibly incur the charge of affectation in stating that this temperature was much too high to be agreeable to us; but it was nevertheless the fact, that everybody felt and complained of the change. We had often before remarked that considerable alterations of the temperature of the atmosphere are as sensibly felt by the human frame at a very low part of the scale as in the higher. The difference consists only in this, that a change from -40 upwards to about zero is usually a very welcome one; while from zero upwards to the freezing point, as in the instance just alluded to, it becomes, to persons in our situation, rather an inconvenience than otherwise.”

Besides the more gradual effects of great heat, direct or indirect, upon the human body, it sometimes operates distinctly as an *exciting* cause, and gives rise to more sudden attacks of illness. Persons who are exposed to the direct beams of a hot sun,

especially during any labour or active exercise, are apt to be affected by what is called the sun-stroke, the *coup de soleil*, insolation: they fall down insensible, and often die in a very short time. This disorder is common among troops in long marches in India. It is a complaint of which the cause has long been known by the inhabitants of hot climates. There is a case of it related in the Bible. "And Manasses was her husband, of her tribe and kindred, who died in the barley harvest. For as he stood overseeing them, and bound sheaves in the field, the heat came upon his head, and he fell on his bed, and died in the city of Bethulia."

Pathologists are not agreed respecting the intimate nature of this distemper; nor about the manner in which it destroys life. Some regard it as a sort of apoplexy; and hold that death takes place in the way of coma. But the most approved remedies of congestive apoplexy—bleeding and other evacuations—have not proved successful in relieving it. The natives of India prefer the pouring of cold water upon the head to every other curative measure. Our army serjeants also found that stimulants—rum and water, for instance—answered better than depletion. I have never seen this affection, but I should conjecture that it is more akin to the state we call concussion than to true apoplexy. It would appear that the sun's rays act upon the brain like a shock. The nervous system is suddenly and extensively influenced, and the heart's movements arrested, as in syncope. One of Sir Benjamin Brodie's experiments is in favour of this opinion. He placed a rabbit in a basket in an oven, the temperature of which was not more than 150°, and it died in a few minutes without any apparent suffering. The heart was afterwards found distended with blood, on both sides, as after death by asthenia.

[By some it has been supposed that *coup de soleil* is the result of a sudden change produced by the sun's rays in the blood; and the absence of any marked organic lesions, and the fluid condition of the blood, observed in the bodies of those who have died of supposed sun-stroke, have no doubt led to this belief.

Coup de soleil, as it is usually but improperly termed, in the majority of cases, occurs in persons engaged in some laborious occupation, when exposed, for the most part, to the direct rays of the sun; or, at least, to an atmosphere of very high temperature. By the combined effects of violent muscular exertion, and the heat of the atmosphere, the heart is stimulated to a morbid degree of activity; copious perspiration flows from every part of the surface of the body, extreme exhaustion speedily ensues, and the heart, fatigued by its morbid activity, allows a passive congestion to take place throughout the body. Violent convulsions often take place; the pulse becomes thready, irregular, and sometimes scarcely perceptible, and the pupils dilated. In the intervals of the convulsions the patient is often affected with muscular tremors like those of paralysis agitans; in other cases he becomes entirely quiescent and insensible. In four *post mortem* examinations of patients who had perished from stroke of the sun, Dr. Pepper, of Philadelphia (*Transactions of the Philadelphia College of Physicians*, vol. iii., *old series*, pp. 99, *et. seq.*), detected no indications of congestion of the brain; nothing, in fact, of an unusual appearance in any of the organs, save the heart, which, in all the subjects, was pallid, flaccid, and softened, while the other muscles of the body were florid and firm. The cavities of the heart contained but little blood, and no coagulum. The endocardic membrane, and that lining the large blood-vessels, were of a very dark, almost purple colour. The softening of the heart and discolouration of the lining membrane, were not, we are assured, the effects of commencing decomposition, but evidently the result of disease. The disease in these cases is evidently one of nervous exhaustion; and demands for its cure stimulants by the mouth, when the patient can swallow, and by injection into the rectum when such is not the case. In the cases which survived under the care of Dr. Pepper, recovery was very slow; the mind continued for some time very much confused; some became insane; one became affected with partial paralysis, and one with amaurosis.

But notwithstanding a state of nervous exhaustion, as evidenced in the cases just referred to, is that which usually results from severe muscular exercise in an intensely heated atmosphere, exposure to the direct rays of the sun in hot weather, occasionally gives rise to symptoms and lesions of a very different character. Thus, in some cases we have intense congestion of the brain, terminating, perhaps, in a true apoplectic seizure. Other individuals are attacked with great heat and dryness of the entire

surface, injection of the conjunctiva, contracted pupils, a small, quick, and corded pulse, red and dry tongue. They become delirious, morose, or restless, or in a constant state of jactitation. If this condition of things be not speedily subdued by appropriate and active treatment, a state of coma, more or less intense, ensues, and the patient dies, as in cases of acute meningeal inflammation; and upon a post mortem examination, the usual indications of that affection are detected.

In this form of sun-stroke, active depletion by the lancet, cups, or leeches, to the head, cold applications to the scalp, active purgatives, and all the other means which enter into a strictly antiphlogistic plan of treatment, will be demanded. If these be neglected, or but timidly employed, a fatal termination is almost inevitable. The administration internally of stimulants would, in such cases, but aggravate the meningeal inflammation, and accelerate the unfavourable result of the case.—C.]

Great heat tends also to the production of certain cutaneous diseases: it is said that few Europeans escape, on their first settling in tropical climates, an eruption of pimples, attended with almost intolerable itching and pricking, and lasting for some weeks. It is called in India *the prickly heat*.

Before considering that most prolific source of disease which is familiar to the commonest observation in sudden *transitions* of temperature, let us inquire what are the ascertained effects of extreme *cold* upon the human frame. Of course I use the term cold in its popular acceptation, as if it were something positive, instead of signifying the mere privation of heat. It is much more convenient to speak of it in this way, and there is no risk of your being misled by my doing so.

Now this inquiry is of more practical interest to us than that which is concerned with the immediate effects of extreme heat. Even in this climate medical men are not unfrequently called upon, in cases of injury or death produced by intense cold, either to remedy the morbid conditions it has caused, or to explain the mode and probability of its operation in extinguishing life.

The effects of cold, as might well be imagined, are in many respects the direct opposites of the effects of heat. When its application is continued, it acts as a sedative upon the organic functions both of animals and of plants. This appears from the shrinking of the external parts. The superficial arteries become unable to transmit the blood in the usual quantity through the integuments. Hence the skin becomes pale, and contracting round the sebaceous glands and the hair-bulbs, exhibits a roughness which is compared to that of the skin of a plucked goose, and is technically called *cutis anserina*. By the same contraction of the smaller vessels, and repressed circulation, the extreme and projecting parts are diminished in size. Thus, rings which are tight on the fingers while the body is warm, drop off in cold weather—and even the shoes fall from the feet during extreme exposure. The heart with the whole arterial system becomes weak. I have already, when speaking of the contrasted operation of heat, illustrated the withering influence of a continued low atmospheric temperature upon the organic functions, by referring to the dwarfish size of both men and the lower animals, as well as of plants, in cold regions. We have evidence of the same fact in the tardy development of the functions, and particularly, as many travellers affirm, of the sexual functions, in cold climates as compared with hot; and in the winter torpor of certain animals, which is very analogous with the state of trees and shrubs in that season.

I need not tell you that to judge of the effects of mere coldness of the atmosphere, we must take the case of the atmosphere *at rest*. The air is a bad conductor of caloric, and for that reason, a much lower, as well as a much higher degree of temperature, can be borne when it is in a state of quiescence, than when fresh portions of it are perpetually brought into contact with the surface of the body by currents of air. "With the thermometer," says Captain Parry, "at -55° ," (a most fearful degree of cold, you will observe, 55° below zero, *i. e.*, 87° below the freezing point), "with the thermometer at -55° , and no wind stirring, the hands may remain uncovered for ten minutes or a quarter of an hour without inconvenience; while with a fresh breeze, and the thermometer nearly as high as zero, few people can keep them exposed so long without considerable pain." And speaking in another place of the cold, when the thermometer was 49° below zero; 9° or 10° , that is, below the point at which

mercury freezes; he says, "The weather being quite calm, we walked on shore for an hour without inconvenience, the *sensation* of cold depending much more on the degree of wind at the time, than on the absolute temperature of the atmosphere as indicated by the thermometer. In several of the accounts given of those countries, in which an intense degree of natural cold is experienced, some effects are attributed to it which certainly did not come under *our* observation in the course of this winter. The first of these is the dreadful sensation *said to be produced in the lungs*, causing them to feel as if torn asunder when the air is inhaled at a very low temperature. No such sensation was ever experienced by us, though in going from the cabin into the open air, and *vice versâ*, we were constantly in the habit, for some months, of undergoing a change of from 80° to 100°, and in several instances 120° of temperature, in less than one minute: and what is still more extraordinary, not a single inflammatory complaint (beyond a slight cold, which was cured by common care in a day or two) occurred during this particular period."

But when the cold air is in motion, in other words, when there is wind, so that fresh portions of cold air are brought, successively, in contact with the surface; or when it is accompanied with moisture, or occurs under other circumstances favourable to its operation, and to be spoken of more particularly presently; then cold of a much inferior degree of intensity may very speedily occasion partial or total death. By partial death I mean the loss of vitality in certain parts of the body only—the ears, nose, fingers, toes, and feet. The parts thus affected are said to be frost-bitten: and the mode of managing such accidents falling within the province of surgery, I shall confine my remarks almost entirely to the case where general death—death in its full and ordinary meaning—is either brought about, or impending, in consequence of exposure to cold.

One of the earliest effects of extreme cold upon the system at large has been said to be a remarkable and overpowering drowsiness. But I believe you will find that most or all of the persons in whom this torpor has been noticed had not only been exposed to severe cold, but had been using also a great deal of exercise: and perhaps the drowsiness ought to be ascribed, in some measure, at least, to that exercise. They who attribute it to the cold alone, explain the comatose state in this way. They say that the chilling of the surface and extremities drives the blood inwards, causes it to accumulate internally, and increases the flow of blood towards the head. One thing, however, is certain, viz., that drowsiness is not a necessary consequence of exposure to severe cold, although it is a very common consequence. Dr. Currie, in his *Medical Reports*, gives a very interesting account of the shipwreck of an American vessel on the coast of Ireland. Most of the crew, fourteen in all, were immersed to a considerable depth, for twenty-three hours, in water of which the temperature was believed not to exceed 33° or 34° of Fahrenheit: and he states expressly that none of the men were drowsy, and that in no one of the three who perished was death preceded by sleep.

The overpowering tendency of cold when combined with fatigue (and perhaps under certain circumstances of intense cold alone), to induce sleep, was strikingly exemplified in what befel Dr. Solander among the hills of Terra del Fuego. The story, as given in Captain Cook's *Voyages*, is well known. Sir Joseph Banks and Dr. Solander had been out botanizing. On their return towards the ship, after various hardships, and after having travelled through swamps for a considerable way, the weather, which had been very fine, became gloomy and cold, with sudden blasts of piercing wind, accompanied by snow. Finding it impossible to get back to the ship before the morning, they resolved to push on through another swamp that lay in their way, into the shelter of a wood, where they might build a wigwam and kindle a fire. Mr. Banks (as he was then) undertook to bring up the rear. Dr. Solander, who had more than once crossed the mountains that divide Sweden from Norway, and who well knew that extreme cold, especially when joined with fatigue, produces a torpor and sleepiness which are almost irresistible, conjured the company to keep moving, whatever pain it might cost them, and whatever relief they might be promised by an inclination to rest. "Whoever *sits down*," said he, "will *sleep*, and whoever *sleeps*, will *wake no more*." Thus at once admonished and alarmed, they set forwards; but they had not gone far before the cold became suddenly so intense as to produce the effects that had been most dreaded. Dr. Solander was the *first* who found the

inclination, against which he had warned others, invincible, and he insisted on being suffered to lie down. Mr. Banks entreated and remonstrated with him in vain: down he lay upon the ground, although it was covered with snow, and it was with much difficulty that his friend kept him from sleeping. Richmond also, one of the black servants, began to linger in the same manner. When he was told that if he did not go on he would in a short time be frozen to death, his answer was that he desired nothing but to lie down and die. The Doctor said he was willing to go on, but that he must first take some sleep; although but a short time before he had told the company that to sleep was to perish. Mr. Banks and the rest found it impossible to carry them, and there being no remedy, they were both at length suffered to lie down, being partly supported by some bushes; and in a few minutes they fell into a profound sleep. Soon after, some of the people who had been sent forward returned with the welcome news, that a fire was kindled about a quarter of a mile on the way. Mr. Banks then endeavoured to wake Dr. Solander, and happily succeeded; but though he had not slept five minutes, he had almost lost the use of his limbs, and the flesh was so shrunk that his shoes fell from his feet. He consented to go forward with such assistance as could be given him; but no attempts to relieve poor Richmond were successful. He, together with another black left with him, died.

In many instances, before this complete torpor comes on, intense cold has a curious effect upon the nervous system, blunting the sensations, and confusing the intellect, and giving to the person exposed to it the appearance of one intoxicated. It is very necessary that you should be aware of this, for there is too much reason to believe that poor wretches who have been picked up by the constables in the streets at night, during periods of hard frost, have been supposed to be drunk, when, in truth, they were only stupefied by the cold. Such a mistake is most likely to be fatal to them. Instead of receiving the attention and treatment proper for persons in their condition, they are liable to be laid aside, by themselves, to sleep off their supposed debauch; and the morning finds them corpses. It is not at all improbable that some of you may be called upon to investigate such cases: and as actual instances are more readily impressed upon the memory than any general description, I will read you a short history illustrative of what I have just been saying, from Captain Parry's Journal.

"John Pearson*** had his hands severely frost-bitten, having unfortunately gone without mittens, and with a musket in his hand. A party of our people, most providentially, found him, although the night was very dark, just as he had fallen down a steep bank of snow, and was beginning to feel that degree of torpor and drowsiness which, if indulged, inevitably proves fatal. When he was brought on board his fingers were quite stiff, and bent into the shape of that part of the musket which he had been carrying: and the frost had so far destroyed the animation in his fingers on one hand, that it was necessary to amputate three of them a short time after."

It is what immediately follows this, that I was desirous of pointing out to your attention.

"The effect which exposure to severe frost has in benumbing the mental as well as the corporeal faculties, was very striking in this man, as well as in two of the young gentlemen who returned after dark, and of whom we were anxious to make inquiries respecting Pearson. When I sent for them into my cabin, they looked wild, and spoke thick and indistinctly, and it was impossible to draw from them an rational answer to any of our questions. After being on board for a short time, the mental faculties appeared gradually to return with the returning circulation; and it was not till then that a-looker on could easily persuade himself that they had not been drinking too freely. To those who have been much accustomed to cold countries, this will be no new remark, but I cannot help thinking (and it is with this view that I speak of it) that many a man may have been punished for intoxication, who was only suffering from the benumbing effects of frost: for I have more than once seen our people in a state so exactly resembling that of the most stupid intoxication, that I should certainly have charged them with that offence, had I not been quite sure that no possible means were afforded them on Melville Island to procure anything stronger than snow-water."

When persons in this state are suffered to sleep, and the operation of the cold continues, they become less and less sensible to external impressions, until death closes the scene.

[To the soporific effects of intense cold, and the peculiar influence it exerts upon the sensations and intellect of those who are exposed to it, Dr. Kane, in his "Arctic Explorations in the years 1853, '54, '55," makes frequent reference. In one part of his narrative, he observes: "I was of course familiar with the benumbed and almost lethargic sensation of extreme cold; and once when exposed for some hours in the midwinter of Baffin's Bay, I had experienced symptoms which I compared to the diffused paralysis of the electro-galvanic shock. But I treated the *sleepy comfort* of freezing as something like the embellishment of romance. I had evidence now to the contrary."

During a travel of between eighty and ninety miles, most of the way dragging a heavy sledge, the mean temperature of the whole time, including the warmest hours of three days, being at minus $41^{\circ}.2$, Dr. Kane writes — "Bonsall and Morton, two of our stoutest men, came to me, begging permission to sleep. They were not cold: the wind did not enter them now: a little sleep was all they wanted." Presently Hans was found nearly stiff under a drift; and Thomas, bolt upright, had his eyes closed, and could hardly articulate. At last Blake threw himself on the snow, and refused to rise. They did not complain of feeling cold; but it was in vain that I wrestled, boxed, ran, argued, jeered or reprimanded; an immediate halt could not be avoided." On another occasion, during the same journey, he says: "We kept ourselves awake by imposing on each other a continued articulation of words; they must have been incoherent enough." "We were neither of us in our right senses, and retained a very confused recollection of what preceded our arrival at the tent." "Our halts multiplied, and we fell half sleeping on the snow. I could not prevent it. Strange to say, it refreshed us. I ventured upon the experiment myself, making Riley wake me at the end of three minutes; and I felt so much benefited by it that I timed the men in the same way. They sat on the runners of the sledge, fell asleep instantly, and were forced to wakefulness when their three minutes were out." In the further prosecution of their journey, "We were," says Dr. Kane, "quite delirious, and had ceased to entertain a sane apprehension of the circumstances about us. We moved on like men in a dream. Our foot-marks, seen afterward, showed that we had steered a bee-line for the brig. It must have been by a sort of instinct, for it left no impress on the memory." "I thought myself the soundest of all, for I went through all the formula of sanity, and can recall the muttering delirium of my comrades when we got back to the cabin of our brig. Yet I have been told since of some speeches, and some orders too of mine, which I should have remembered for their absurdity, if my mind had retained its balance." — C.]

LECTURE VII.

Causes of Disease, continued. Laws by which the operation of Cold upon the Bodily Health is regulated. Circumstances that favour its injurious Effects, and respect, first, the Body itself; secondly, the manner in which the Cold is applied. Modifying influence of certain states of the Mind — of Sleep — of Habit. Means of protection. Influence of the different Seasons. Impurity of the Air. Hereditary tendencies to Disease.

In the last lecture I commenced the consideration of some of the *causes* of disease. We learned, by the evidence of authentic facts, that the human body is capable of bearing a very *high* degree of external temperature, for a short time, without detriment — and even without much inconvenience; and we learned — also by the testimony of facts — that the body is equally well calculated to endure, under favourable circumstances, a very *low* degree of atmospheric temperature — or, to speak in popular language, a very intense degree of *cold*.

It appears also that a high, but not extreme, atmospheric temperature, when long continued, has a stimulating effect upon the *organic* functions, and a depressing or sedative effect upon the *animal* functions of the body. Long-continued heat predisposes the body to be injuriously influenced by exposure to cold: the diseases apt to follow such exposure, under such circumstances, being derangement of the hepatic functions—violent disturbances of the stomach and bowels, with a copious discharge of vitiated and acrid bile—and acute inflammation of the liver itself. As more direct consequences of exposure to extreme heat—in other words, as examples of disorders of which extreme heat sometimes proves an *exciting* cause—I mentioned the *coup de soleil*, and the eruption called *the prickly heat*.

With respect to external cold, I pointed out to you its depressing effects upon the organic functions of the body—and, when it becomes very intense indeed, its directly sedative influence upon the animal functions also—producing a state resembling intoxication, overpowering drowsiness, and coma, especially when the cold has had an auxiliary in fatigue; and, ultimately, death itself.

I hardly need say that the effect of external cold upon the body within certain limits of intensity and duration, is totally different from all this. When it is not intense—or when, though intense, it is applied for a short time only—or when its refrigerating and sedative properties can be sufficiently counteracted by exercise and warm clothing—cold becomes a *tonic*; stimulating, refreshing, and invigorating both mind and body. Instead of benumbing, it heightens the sensibility; instead of stupefying, it clears and sharpens the faculties, and bestows alacrity and cheerfulness of spirit; and in this way, among others, cold becomes a very important curative agent.

Here, also, therefore, the contrast obtains; a high external temperature relaxes and depresses—a low one, under the circumstances just mentioned, braces and enlivens.

Nevertheless, exposure to cold is one of the most common causes of various complaints. Many or most of the internal inflammations acknowledge cold as their ordinary exciting cause. Acute rheumatism has, perhaps, no other external origin. Apoplexy, and palsy, and dropsy, are its frequent consequences. “With the exception,” says Dr. Bateman, in his *Observations on the Diseases of London*, “with the exception of a small number of diseases occasioned by unwholesome occupations, and by the contagions, the great mass of human malady in this metropolis is referable to the climate or state of the seasons, and to intemperance; but, of these two causes, the vicissitudes of the weather, especially its cold, are by far the most prolific sources of mischief.”

It must, therefore, to every one who is engaged, or likely to be engaged, in the practice of physic, be a matter of first-rate importance, and of great interest, to ascertain the circumstances under which the application of cold is the most prejudicial, or has the greatest influence upon the body—as well as the means by which the bad effects of exposure to cold may often, in a great degree, be prevented.

There are some short but valuable hints upon this subject in Cullen’s *First Lines*. The late Dr. Currie, of Liverpool, was, however, the first person who distinctly pointed out the laws that regulate the operation of cold as a cause of health and disease.

Of the circumstances which favour the morbid effects of cold, some relate to the condition of the body itself, some to the particular manner in which the cold is applied. The former are predisposing circumstances; the latter accessory. We will glance at these in succession.

It has long been a popular, as well as a professional axiom, that *sudden vicissitudes* of temperature are dangerous; that a *previous hot state of the body* augments the hurtful effect of cold, whether applied externally or internally. But the proposition thus broadly stated is not universally true. It is well known that the inhabitants of Russia are in the habit, while reeking from their vapour baths, of rolling immediately in the snow, or plunging into cold water, without suffering from the change. Sir Charles Blagden, describing some of the experiments which I mentioned in the last lecture, says, “During the whole day we passed out of the heated room (of which the temperature ranged from 240° to 260°) after every experiment, immediately into the cold air without any precaution. After exposing our naked bodies to the heat, and sweating most violently, we instantly went into a cold room, and staid there even some minutes before we began to dress, yet no one received the least injury.” And

Captain Scoresby, speaking of the arctic regions, tells us that he has often gone from the breakfast-room of the vessel, where the temperature was 50° or 60° , to the mast-head, where it was only 10° , without any additional clothing, except a cap—"yet," says he, "I never received any injury, and seldom much inconvenience, from the uncommon transition."

It is plain, therefore, that the proposition which assigns larger to sudden vicissitudes of temperature requires limitation. The effects of a sudden descent from one point to another in the scale of atmospheric temperature vary according to the state of the body at the time. Without going into any physiological discussion respecting the source of animal heat, I may just remind you of the faculty of evolving heat possessed by man and the warm-blooded animals; by which faculty very nearly the same degree of inward temperature is steadily maintained under very different degrees of outward temperature. If the external temperature be lower than that of the body, the caloric thereby carried off is speedily replaced, in a healthy adult, by this evolution of heat from within, aided by clothing, or by exercise. When the external temperature approaches the standard heat of the body, sweat soon breaks forth, and the superfluous heat is removed by evaporation: for so constant is the internal evolution of caloric, that an atmosphere which does not as constantly abstract any of it is excessively incommoding. An external temperature of 98° , which is about the average heat of the blood in man, is, as you know, extremely oppressive. The terms hot, warm, cool, cold, as applied to the surrounding air, are regulated by the sensations that it produces upon the average of persons. If the heat be carried off as fast as it is generated, and no faster, no particular sensation is felt, and the bodily powers are neither stimulated nor exhausted. This equilibrium is maintained (supposing that no extraordinary exertions are made) when the thermometer stands at 62° , or thereabouts. We call that point in the scale *temperate*. All degrees above that point, up to 70 , are reckoned *warm*; all above 70 , *hot*. Descending in the scale, we speak of the temperature denoted by any degree between the 60 th and the 50 th, as being *cool*; and every lower degree of temperature is *cold*. I am speaking of the average of healthy men: for remarkable diversities occur among individuals in respect to the epithets which they assign, under the guidance of their sensations, to particular degrees of the thermometric scale; their sensations differing according to the power which their constitutions respectively possess of evolving heat. Now if this power of evolving heat, thus inherent in the system, be entire, and active, and persistent—if it have not been weakened by any of those circumstances which are known to have the effect of weakening it—no peril need attend even violent alternations of external temperature. Unusual heat of the body at the time when the cold is applied, so far from implying danger, is really the condition of safety, provided the heat is steady and permanent. You may read, in Dr. Currie's book, numerous instances of the cold affusion being employed in the hot stage of fever, and particularly in cases of scarlet fever, not only with impunity, but with great benefit to the patient. The same holds true of the application of cold when the body has been heated by exercise—and, indeed, whatever may have been the cause of the increased heat—provided always that that cause remains steadily in action, that there is no local disease, and that the body is not fatigued, and fast losing its heat. But if a person be already exhausted and weakened by exercise—if he be sweating and rapidly parting with his heat—and especially if the exercise be over, and he remain at rest immediately after and during the application of the cold—then it becomes highly perilous, and likely to produce internal mischief.

The more correct statement, therefore, respecting the application of cold is, that it is dangerous—not when the body is *hot*—but when the body is *cooling after having been heated*.

This principle obtains alike, I say, whether the cold be applied externally or internally; to the surface of the body, or to the mucous membrane of the stomach. Very many instances are recorded of death taking place immediately after a copious draught of cold water. I believe it will be found that in all these cases, the body, after having been much heated and enfeebled by severe exertion, was losing its preternatural heat from profuse perspiration, and, in general, from the cessation also of the exertion by which this heat was accumulated. Celsus was aware of the danger: "*ex labore sudanti frigida potio perniciosissima est.*" The fatal influence of cold water thus

applied was experienced, on a large scale, among the troops of Alexander the Great, upon their reaching the banks of the River Oxus, thirsty, fatigued, and perspiring from their toilsome march of forty-six miles across the scorching sands of the desert. According to Quintus Curtius, Alexander lost more of his soldiers on that occasion than in any one of his battles. "*Sed qui intemperantius hauserant intercluso spiritu extincti sunt; multoque major horum numerus fuit, quam ullo amiserat proelio.*" Dr. Currie relates a striking example, which fell under his own observation, of sudden death thus produced. A young man had been playing a severe match at fives, and had violently heated himself. When it was over he sat down on the ground panting for breath, and covered with profuse perspiration. In this state he called to a servant to bring him a pitcher of cold water just drawn from a neighbouring pump. After holding it in his hand a little while, till he recovered his breath, he put it to his mouth, and drank a large quantity at once. He laid his head on his shoulder, and bent forwards; his countenance became pale, his breath laborious, and in a few minutes he expired.

I may take the opportunity of telling you that the remedies to be administered, when life is in jeopardy from this cause, are warmth to the epigastrium; and laudanum in free doses.

If death do not speedily follow the external or internal application of cold to the body under the untoward circumstances I have described, inflammation of some internal part is very apt to arise.

By attending to the principles now laid down, you will be enabled to furnish those whom it may be your business to advise with many useful suggestions, and to caution them against some common mistakes: mistakes which have had their origin in the unqualified credit given to the maxim, that sudden vicissitudes of external temperature, and exposure to cold while the body is hot, are dangerous; whereas, these things are dangerous under certain circumstances only. Thus, you may tell the sportsman that wet feet, or a wet skin, need cause him no apprehension, so that he continues in active exercise; and changes his clothes, and avoids all further application of cold, as soon as his exercise ends. You may admonish the bather that after walking on a hot day to the river's side, he had better *not* wait, to cool himself a little, before he plunges into the stream; and in like manner you may venture to counsel the young lady who has heated herself with dancing, not to linger in the entrance-hall till the glow has somewhat subsided, but to make the best of her way to her carriage, and thence to her bed; and you may tell your male friends who happen to be similarly circumstanced, that the best thing they can do is to walk briskly home in their great coats. The main points to be remembered are, that "the heat which is preternaturally accumulated by exercise is held with little tenacity, is dissipated by profuse perspiration, and is speedily lost when to this perspiration is added a state of rest after fatigue;" and that, in these circumstances, the application of cold is most apt to be prejudicial.

Among the circumstances which favour the morbid effects of cold, and relate to the condition of the body itself, is to be included,—for reasons that must now be obvious to you—whatever has the effect of weakening the system, and so diminishing its capability of evolving heat. The most common of these debilitating circumstances are enumerated by Cullen—"fasting, evacuations, fatigue, a last night's debauch, excess in venery, long watching, much study, rest immediately after great exercise, sleep, and preceding disease." All these, you will observe, tend to lessen the vigour of the circulation, and to depress the power of generating heat. Consistent with the same principle is the fact ascertained by Dr. M. Edwards, that the faculty of evolving heat is very feeble in old persons, and in the newly born; it being in these classes that we find the greatest number of victims to the power of cold.

The bad effects of cold upon the system depend partly upon the intensity of the sensation it produces—but still more upon the duration of that sensation. We are seldom the worse for a momentary sensation of cold, however lively it may have been; whereas even slight feelings of chilliness, if long protracted, are apt to terminate in some form of disease.

By the help of this principle we may explain most of the circumstances which, relating to the manner in which the cold is applied, have been found by experience to aggravate its hurtful influence.

Cold is more likely, *cæteris paribus*, to prove injurious when it is applied by a wind or a current of air. The sensation of cold is sustained by the continual accession of fresh particles of frigid air to the surface of the body. Some striking facts in illustration of the refrigerating and depressing effects of a stream of cold air were mentioned in the last lecture.

Again, the injurious operation of cold is augmented, when it is accompanied with moisture. Wetness is notoriously the worst way in which cold can be applied. The contact of wet or damp clothes with the skin both increases and prolongs the sensation of cold. For the same reason, a cold foggy atmosphere is more prejudicial than a clear, and therefore drier one, of the same temperature. The heat of the body is abstracted more rapidly than it is generated from within, and if it be not replaced by exercise, or cordials, the balance of the circulation is deranged, and internal mischief often follows.

The same principles serve to illustrate the effect of certain other circumstances, adverted to by Cullen, as being *counteractive* of the morbid tendency of exposure to cold: "passions engaging a close attention to one object;" "that state of the body in which sensibility is greatly diminished, as in maniacs;" and "the power of habit." These circumstances are worthy of a moment's notice.

Impressions which are unheeded are unfelt and inoperative. As it is scarcely possible, when the attention is engrossed by bodily pain, to carry on any connected train of thought, so, on the other hand, the senses become impassive in proportion as the mind is fixed upon some absorbing subject of reflection, or enchained by some powerful emotion: impressions made upon the organs of sense are no longer taken notice of; the corresponding sensations, if they be excited at all, are not remembered, and the effect of such impressions is as if they had never been; they are not followed by the usual consequences. Persons gasping for breath in spasmodic asthma will remain for hours at an open window, with scarcely any clothing, during severe frost, and without suffering from the cold; their attention is so anxiously and exclusively bestowed upon the distress in their breathing, that the coldness of the air is unnoticed and unperceived, and has no sensible effect.

"For where the greater malady is fixed,
The lesser is scarce felt."

The morbid effect of cold upon the system is certainly modified by the degree of attention that is paid to the sensation it excites.

Upon the very same principle may be explained the impunity with which some maniacs undergo exposure to cold—even when suffering no fever which might regenerate the lost heat. "I have seen," says Dr. Currie, "a young woman, once of the greatest delicacy of frame, struck with madness, lie all night on a cold floor, with hardly the covering that decency requires, when the water was frozen on the table by her, and the milk that she was to feed on was a mass of ice."

Sleep is enumerated by Dr. Cullen among those conditions of the body which diminish its power of resisting cold. And certainly cold is very readily caught (as the phrase is), when its causes are present, during sleep. But while we sleep sensation is in a great measure suspended. This would seem, therefore, to furnish a contradiction to the principle that the effect of cold upon the bodily health depends upon the strength and the duration of the sensation excited by it. Dr. Alison—I speak from recollection of his observations heard many years ago—disposes of this difficulty by affirming that the sleeper who thus suffers, does really feel, and is conscious of, the sensation of cold, and that it mingles with and probably suggests his dreams. Lord Brougham, in his *Discourse of Natural Theology*, gives a very lively picture of dreams so excited,—drawn, as I should guess, from his own experience. Probably something of the same kind has occurred to most of us. "Every one knows (he says) the effect of a bottle of hot water, applied during sleep to the soles of the feet; you instantly dream of walking over hot mould, or ashes, or a stream of lava, or having your feet burnt by coming too near the fire. But the effect of falling asleep in a stream of cold air, as in an open carriage, varies this experiment in a very interesting, and indeed instructive manner: you will, instantly that the wind begins to blow, dream of being upon some exposed point, and anxious for shelter, but unable to reach it: then you are on the deck of a ship, suffering from the gale—you

run behind a sail for shelter, and the wind changes, so that it still blows upon you; you are driven to the cabin, but the ladder is removed, or the door locked. Presently you are on shore in a house with all the windows open, and endeavour to shut them in vain; or, seeing a smith's forge, you are attracted by the fire, and suddenly a hundred bellows play upon it, and extinguish it in an instant, but fill the whole smithy with their blast, till you are as cold as on the road."

Certain it is, that though while sleeping we are not sensible of, or (what perhaps is the same thing) do not remember, ordinary impressions, we are nevertheless conscious of unusual sensations; so that the facility with which we take cold during sleep is no real exception to the general law, that the sensation produced by cold is concerned in its injurious effects.

The last of the accessory circumstances mentioned by Cullen is "the power of *habit*." No one can doubt the effect of custom in enabling the body to resist the operation of cold, who has had opportunities of observing how differently an inclement temperature is borne by persons whose employments oblige them to live much under the open sky, as shepherds, sailors, stage-coachmen; and by such as pursue in-door occupations — mechanics, tailors, shopmen and the like. Probably the sensibility of the surface is blunted by habitual exposure. We may believe too that the purer air breathed by the out-door labourer, and his more active life, confer a more vigorous state of health, and endow him with an ampler faculty of evolving animal heat. The fact is unquestionable; and we may sometimes turn our knowledge of it to good account, in gradually fortifying the system against the influence of cold that cannot be avoided. An ill-directed application of this principle has led, however, to grave errors, and cost many lives. You will now and then hear parents talk of *hardening* their children, by causing them to brave all sorts of weather, by teaching them to be indifferent about variations of temperature, to sit in winter-time without a fire in the room, and to despise great coats, flannel, and other additions to their usual dress. Fearing to render them effeminate by over care and cockering, they run into the opposite and more dangerous extreme.

This process is often attempted with children originally delicate, and to such it is doubly hazardous. During the early periods of life the inherent protective power of evolving heat is comparatively feeble; and in this climate it requires to be carefully cherished.

The experiment of hardening should never be tried on any child or person who is ailing or unsound; who shows any sign of present disease; or any marked disposition to future, and especially to scrofulous, disease. Whenever it is tried it must be conducted in conformity with the principles already laid down. The subject of the experiment must be sufficiently clothed, and he must not fail to use such exercise during the exposure as may be requisite to excite and sustain the adequate generation of animal heat. An *abiding* sense even of chilliness must never be aimed at nor permitted.

The most direct and certain mode of fortifying the body against injury from accidental exposure to cold, is afforded by the use of the cold bath, and especially of the shower bath. When this is regularly taken in the morning, the surface of the body becomes inured to a degree of cold greater than it is likely to encounter during the remainder of the day. It is fortunate that we have an easy criterion of the propriety of continuing this expedient. When the sense of cold does not remain long, and is followed by a glow of warmth, the cold shower bath is sure to do good. If, however, after the bath, the person suffer headache, and continue to be chilly, languid, and uncomfortable, it should at once be given up, as useless, and even hazardous.

By observing these simple rules, a healthy child may be made hardy also, without the risk which their neglect would impose, of damage to his bodily fabric, and of abbreviating, by what was meant to prolong, his mortal span.

Closely connected with the effects of temperature upon the health is the influence of the different *seasons* of the year. A few remarks upon that influence, as it is witnessed in our own climate and country, may properly conclude our present subject.

It is open to the commonest observation that the general health of the community fluctuates with the changing seasons. Catarrhs, and coughs, and pectoral complaints of all kinds, are most apt to commence, or to grow worse, in the winter and spring

months; while bowel complaints are more numerous and distressing in the summer and autumn. The mucous membranes of the air-passages sympathize with the skin under the agency of external cold; those of the stomach and intestines under that of continued atmospheric heat.

The thoracic disorders are more serious and fatal than the abdominal. Various other maladies are likewise aggravated by cold, or by vicissitudes of temperature. Hence the mortality of the winter is always larger than that of the summer: unless indeed this rule happens to be disturbed by the intervention of some widely-spread epidemic. I am not sure that the superior salubrity of the hotter over the colder portion of the year is generally acknowledged, even in this age of enlightenment.

[This observation is not true in reference to a large portion of the United States. Judging of the salubrity of Philadelphia by the number of deaths occurring at the different seasons, we find that in a series of ten years, during which no important epidemic prevailed throughout either year, the number of deaths which occurred in summer exceeded by 3529 those which occurred in winter. Comparing the warm months of the year, from June to November inclusive, with the cold, from December to May inclusive, the deaths for the same ten years, during the first period, exceeded those during the second period by 1690. — C.]

It is the cold that, more than any other element of the weather, occasions the difference.

There are two small publications by the second Dr. Heberden, to which I would direct your attention, as being singularly instructive upon these points. One you will find in the eighty-sixth volume of the Philosophical Transactions: *Of the influence of Cold upon the Health of the Inhabitants of London*. The other is a separate pamphlet: *Observations on the Increase and Decrease of different Diseases*.

From a number of tables, framed chiefly upon the weekly bills of mortality, Dr. Heberden (in the last named paper) draws the conclusion that the whole number of deaths is greatest in January, February, and March, and least in June, July, and August. This is contrary to the notions of the ancients, and perhaps of many of the moderns also. Celsus says, "Igitur saluberrimum ver est; proxime deinde ab hoc, hiems; periculosior æstas; autumnus longe periculosissimus."

[In Philadelphia, during the ten years referred to above, the deaths in June, July and August, exceeded those in January, February and March, by 3032. — C.]

The difference of place may perhaps account for this difference of opinion. Celsus lived in Rome. That city, and the surrounding district, abound in *malaria*; a cause of disease which, happily, is now scarcely known in London, but which, wherever it exists, operates most powerfully and most extensively during the autumn. The comparative healthiness of the several seasons may doubtless be disturbed, and even reversed, by endemic peculiarities of this kind.

In his paper in the Philosophical Transactions, Dr. Heberden compares the number of deaths that took place in London in January, 1795, which was an unusually severe month, with the number that occurred in January, 1796, which was an uncommonly mild month. Of those two successive winters one was the coldest, and the other the warmest, of which any regular account had been kept in this country. In the month of January, 1795, the thermometer, upon an average, stood at 23° in the morning, and at 29·4° in the afternoon; always, you will observe, below the freezing point. In the same month in 1796, it stood at 43·5° in the morning, and at 50° in the afternoon; always much above the freezing point. The average difference in the two months was more than 20°.

In the five weeks beginning upon January 1st, 1795, there were 2823 deaths: in the five weeks beginning upon January 1st, 1796, there were only 1471. The difference, 1352, is enormous. The mortality in the former year was nearly double that in the latter.

One object which Dr. Heberden had in view in making this comparison, was to disabuse his countrymen of the notion that, in winter, frosty weather is more favour-

able to health than mild weather; a notion which has been embodied in the proverb, that "a green Christmas makes a fat churchyard."

It is very instructive to remark in what class of persons the injurious effects of the severe weather of winter is most felt. The increased mortality was found to be chiefly among the very young, and the very old: in other words, among those in whom the recuperative power of generating heat is the feeblest.

In January, 1795, there were in London 717 deaths of persons above sixty years old, while in January, 1796, there were only 153 such deaths; or scarcely more than one-fifth of the former number.

I have often been struck by the unusual length of the newspaper obituaries during periods of hard frost; and by observing how many of the individuals whose deaths they record were far advanced in years. Dr. Heberden remarks that among persons older than sixty, the tide of mortality, as measured by the weekly bills, follows regularly the degree of coldness of the weather; so that any one accustomed to examine these lists may form a tolerably accurate judgment of the severity of any of our winter months, by noting the ratio of mortality in persons above sixty.

The deaths from asthma (under which vague term all kinds of pectoral disorder attended with shortness of breath appear to have been included) were 249 in January, 1795; only 29 in January, 1796. In the former of these months there were 825 deaths attributed to consumption; in the latter, 342.

All this accords with what I mentioned before of the effect of cold weather in producing or exasperating diseases of the respiratory organs.

One of the conclusions deduced by Dr. Heberden from his examination of the bills of mortality is, that "the number of deaths by palsies and apoplexies is in this country always greatest in winter." There are intelligible reasons for this. When the surface is chilled, and the blood driven out of the superficial vessels by the cold, it must accumulate in internal parts, and so press with increased force towards the head. And there is another reason for the frequency of these affections in the winter season: it is, as we have seen, the season of pectoral complaints, and of embarrassed respiration. Dyspnoea, and fits of coughing, greatly impede the return of the blood from the head through the veins: and cerebral congestion tends to the production of cerebral hæmorrhage, especially when the arteries of the brain are diseased; and they often are so. Accordingly we find that in January, 1795, there were fifty-two deaths from apoplexy and palsy; while in January, 1796, the number was only thirty-one.

Since these lectures were first delivered, several *Annual Reports of the Registrar-General of Births, Marriages, and Deaths in England*, have been printed, and liberally circulated, by the obliging attention of Mr. Lister and of Major Graham, among the members of our profession. Much of the practical information afforded by these interesting volumes is rendered accessible, even to a cursory reader, by Dr. Farr's able analysis of the registered facts; which amply illustrate and confirm most of the inferences drawn by Dr. Heberden from the old tables of mortality.

For example, under the head of "Influence of the Seasons," Dr. Farr shows, by numerical statements, especially in the *third Annual Report*, that in London the degree to which the mean monthly temperature descends in December, January, or February, determines, to a great extent, the mortality of the winter.

Again, "The causes of death which prove most fatal in the cold months belong principally to the pulmonary class, and the cerebral diseases of the aged: those which prove most fatal in summer belong to diseases of the bowels."

The mean temperature of the external atmosphere in London is $50\frac{1}{2}^{\circ}$. In proportion as the mean temperature of the day and night falls beneath that point, the mortality progressively increases.

"The rise in the mortality," says Dr. Farr, "is immediate; but the effects of the low temperature go on accumulating, and continue to be felt thirty or forty days after the extremities of the cold have passed away. The cold destroys a certain number of persons rapidly; and in others occasions diseases which prove fatal in a month or six weeks. The practical lesson taught by these facts is obvious. A great number of the aged, and of those afflicted with difficulty of breathing, cannot resist cold sunk so low as 32° . The temperature of the atmosphere in which they sleep can never safely descend lower than 40° ; for if the cold that freezes water in their chamber do

not freeze their blood, it impedes respiration, and life ceases when the blood heat has sunk a few degrees below the standard."

The immense body of authentic facts thus yearly accumulating in these Reports constitute most valuable contributions to the science of vital statistics; and cannot fail to throw light upon the sources, and to point towards the prevention, of many very dangerous and destructive disorders. To ascertain the causes of any disease, and to display them before the public mind, are, I repeat, large steps towards the ultimate removal of such as human endeavours are competent to remove.

You may trace the influence of the seasons, not only in the prevalence of particular diseases in certain portions of the year, but also in the character of other disorders that are liable to occur in all periods of the year alike: in the character, for example, of fevers. In the majority of cases of continued fever you will find that the pectoral symptoms are most troublesome in the spring, and the abdominal symptoms in the autumn. It is said also, but I do not know that this is so generally true, that affections of the head, in continued fever, are more frequent and severe in the winter than at other periods of the year.

Mere impurity of the air—by which I mean impurity that does not result from the admixture of any *specific* poison, such as the marsh poison, and the various contagions—is a powerful predisposing cause of disease. The prejudicial effect of impure air is seen, on a large scale, by comparing the inhabitants of great towns, in respect of health and longevity, with those who live in the country. If we again refer to Dr. Farr's calculations, founded upon the returns made to the Registrar-General, we find it stated, in the third Annual Report, that in cities, as contrasted with rural districts, the deaths from consumption are increased 24 per cent.; those from typhus 55 per cent.; those from childbirth 59 per cent.; and so of several other disorders. "The diseases chiefly incidental to childhood are twice as fatal in the town districts as they are in the country." The mean duration of life in the two classes of districts differs nearly 17 years; being in the proportion of 55 years (*country*) to 38 years (*towns*).

These differences we can explain only by attributing them to the weakening influence of impure air, and the want of sufficient exercise; for, as Dr. Alison has remarked, "it is hardly possible to observe separately the effect on the animal economy of deficiency of exercise, and deficiency of fresh air, these two causes being very generally applied together. But it is perfectly ascertained on an extensive scale, in regard to the inhabitants of large and crowded cities as compared with the rural population of the same climate, that their mortality is very much greater, especially in early life—and the probability of life very much less." There is one circumstance which shows that impure air is the more noxious agent of the two, namely, the great comparative mortality, in towns, of children under two years of age, even although they get as much exercise as their time of life would allow of anywhere.

The noxious and depressing influence of vitiated air is made strikingly manifest by the effect of removal to a purer atmosphere. We are continually obliged to recommend "change of air" to our patients. We advise them to go out of London,— "where houses thick and sewers annoy the air,"—that their recovery from acute disorders may be accelerated, and that they may regain the degree of general strength which is necessary to the cure of many chronic complaints; of all those especially that require the use of *tonic* medicines, among which class of remedies no one is so effectual, in constitutions that have been weakened by a town life, as migration to the clear and pure air of the country.

It is necessary, however, to remember that although impure air has most unquestionably a very hurtful effect upon the general health, there is no specific disease which can be distinctly traced to it as an *exciting* cause. It is as a predisposing influence that the impurity operates. For instance, it never *generates* (as I believe) continued fever, yet it will most certainly aggravate the symptoms, and favour the propagation, and augment the mortality, of that and of other diseases, in a great degree. If there be any diseased condition that is strictly the product of impure air, it is scrofula. Scrofula (as I shall presently show you) depends in part upon hereditary constitution; it partly arises also from exposure to cold and wet; but there is much reason for believing that impure air is a very powerful agent in calling scrofula into action, and in aggravating the strumous diathesis.

[This statement of Dr. W. is not strictly correct. That specific disease is produced by impure air is a fact established by the most abundant and conclusive testimony. Wherever we find individuals crowded together in localities where filth is accumulated, and a free ventilation is prevented, there, also, we find diseases to prevail, evidently dependent upon the impure and stagnant state of the atmosphere; though other causes, no doubt, conspire to their development, and to augment their malignancy. It cannot, certainly, be denied, that it is to the impurity of the air, produced by decomposition of the exhalations and excretions of individuals regardless of personal and domestic cleanliness, when crowded together in confined apartments, that we are to refer the production of the typhus fever, so apt to prevail under such circumstances, as well as the typhoid character of most of the diseases with which such individuals may become affected from other causes. Typhus fever, we know, is particularly liable to be produced in camps, barracks, hospitals, prisons, besieged fortresses, and on board of ships, whenever in such situations a number of persons are crowded together, and cleanliness and ventilation are neglected; and it is seldom, if ever, generated when the sources of impurity are carefully removed, and a free and constant ventilation secured.]

The dependence of yellow fever upon an impure state of the atmosphere is shown by the fact, that it is almost exclusively confined to the filthy and confined holds of vessels, or to towns or other situations where the population is dense, and many causes exist calculated to impair the purity of the atmosphere; and by its being very generally confined, when it occurs in commercial cities, to the vicinity of the wharves and docks, into which the common sewers empty, and various impurities are allowed to accumulate, and which, when acted upon by heat, cannot fail to give rise to a miasm, by which the purity of the surrounding atmosphere is very materially impaired.

In regard to the agency of an impure air "in calling scrofula into action, and in aggravating the strumous diathesis," the evidence is by no means so strong as the language of Dr. W. would imply. Dr. Phillips, in his late work on the Nature and Causes of Scrofula, has examined this question with a great deal of care: his conclusions are, that "if we regard scrofula in the widest signification of the term, so as to include all diseases in which a tubercle-like deposit seems to determine the loss of life, namely, phthisis, scrofula, and tabes mesenterica, the result may be thus stated: The mortality from those diseases is larger in towns than in country districts, but it is not found that the densest portion of a town is that in which the mortality is the largest. Thus, in the four districts in London in which the crowding is the greatest, the average deaths from these affections is under 0.46, whilst, in four other districts, in which there is less crowding, the average deaths amount to 0.51, and in Bethnal Green they are under 0.40. If we now narrow the question to the influence of a residence in towns and bad air in developing scrofula alone, the result may be thus stated: In the four years' mortality included in the fifth report of the Registrar-General, namely, from 1838 to 1841, in a district comprising towns having a population of 3,759,186, the deaths from scrofula amounted to 758; and in a district having a rural population of 3,440,501 souls, the deaths amounted to 1333; or, to state the question more simply, the proportion of deaths from scrofula to 1,000,000 living was, in the town districts, 50 per annum, and in the country districts, 97. And if, with scrofula, tabes mesenterica be included, the numbers would stand as 122 deaths in towns to 160 deaths in the country. Again, let us observe the south-eastern, the south-midland, and the south-western districts: there the mortality from scrofula amounted to 1 in 12,000, whilst, in the densely packed factory districts of Yorkshire, Lancashire, and Cheshire, the mortality from scrofula will be seen to be less than half the preceding mortality from the same cause, namely, about 1 in 25,000. And suppose we go one step further, and compare one portion of the metropolis with another, we find that the total deaths from scrofula in the metropolis, when compared with the population, are as 5.6 to 100,000 living, whilst in the most densely populated districts, comprising the East and West London Union, the Strand, Holborn, or St. Giles, Whitechapel, and Bethnal Green, the proportion is as 5.1 to 100,000; Bethnal Green being represented by 4.8, and Whitechapel by 4.7 to 100,000 living. So in the low, dense, and poor districts of Bethnal Green, Poplar, Stepney, Whitechapel, Shoreditch, Westminster, Bermondsey, Rotherhithe, and Lambeth, with a total mortality amounting to 21,522, the deaths from scrofula are 45, or 1 in 478; while in

the district embracing Kensington, St. George's, Hanover Square, and St. Marylebone, with a total mortality amounting to 14,734, the deaths from scrofula are 30, or 1 in 490. Thus the difference in these districts is scarcely appreciable; but if we compare the western districts of the metropolis with those of Bethnal Green, Shoreditch, and Whitechapel, we find that the proportion of deaths from scrofula to the general mortality in the former is as 1 to 490, and in the latter as 1 to 1000. Again, if from considering the influence of localities, we turn to the comparative mortality of the sexes, it will be found that 20 per cent. more boys die of scrofula than girls; and yet it must be admitted that girls are more exposed than boys to the effects, whatever they may be, of crowded rooms, and exclusion from out-door employments."—C.]

I have entered the more fully into the consideration of certain states of the atmosphere, its extremes and its variations of temperature, and its impurity, as causes of disease, because there is no part of the course in which I could more conveniently introduce them. Most of the other causes of disease, enumerated in my last lecture, will be discussed in connexion with the disorders to which they give birth: malaria, for example, when I speak of ague; contagions, when we come to the exanthemata and to continued fevers; epidemic influences, with epidemic distempers; improper or insufficient diet, and intemperance generally, with indigestion, and the disorders of the alimentary canal; and so on. There is, however, one remarkable predisposing cause of disease, a few observations upon which may serve to fill up the little that remains of the present hour. I mean, that disposition to certain diseases which is apt to descend from parents to children: *hereditary tendency* to disease.

We must distinguish between *susceptibility* of disease, and a *tendency* to disease. In one sense all persons are born with a predisposition to most forms of disorder. No one is protected by nature against inflammation when the causes of inflammation come into play. Poisons of various kinds, and specific contagions, which indeed are poisons, operate with tolerable uniformity upon *all men* alike.

But there are certain complaints which we may separate in this respect from the others; which complaints some persons have a tendency to, and some have not. The tendency is sometimes strong and evident, sometimes feeble and faintly marked; sometimes it displays itself in the midst of circumstances the most favourable to health, sometimes it requires for its development conditions the most adverse and trying. To mention some of these diseases: scrofula, which I soon shall describe more particularly, gout, mania, and (I believe I may add) spasmodic asthma.

Not only is a disposition to these complaints strikingly pronounced in some persons, but other persons appear wholly free from such a tendency—nay, even devoid of the susceptibility of them. Gout, in those *capable* of it, may be acquired by habits, as it may be prevented and repressed by the opposite habits. The habits that, in certain persons, bring it on, are the intemperate use of the luxuries of the table, and an indolent or sedentary manner of life; but they are many people in whom no amount of rich living or idleness will generate gout. So there are some in whom no exposure to impure air, cold, and wet, and no privations—in other words, no appliance of the influences calculated to bring the strumous diathesis into play—will ever produce any form of scrofula; will ever render them consumptive, for instance, consumption being one of the most common and fatal shapes of scrofulous disease. There are many who endure the utmost distress and excitement of mind, yet never become insane. There are many who never become afflicted with asthma, although surrounded by the most powerful exciting causes of that complaint.

Now with respect to these diseases, and perhaps a few others, it is a matter of fact that they occur much more frequently in persons, some one or more of whose ancestors have suffered from them, than in other persons: the tendency is transmitted, is hereditary.

That the circumstances of the parents *do* influence the physical characters of the children, no one can doubt: it is matter of daily observation; and one of the best possible illustrations of the fact is to be found in what are called family-likenesses. We see children resembling their father, or their mother; or both parents at once, as mulattoes.

It has been suggested that the similarity in features and expression, and even in

moral character, which cannot be denied to exist, may be ascribed to training and imitation. But allowing something to that cause, it cannot be all. It was, I remember, a common remark when I was at Cambridge, that the followers and admirers of a very good man, the late Mr. Simeon, appeared to come at last to resemble him. So man and wife are sometimes fancied to grow like each other. That is, the same prevailing cast of thought and feeling, the *idem sentile et idem velle*, may give such an habitual expression and character to the countenance, as shall constitute, to superficial observers, a likeness. But there are family-likenesses which will not admit of such an explanation as this: similarities in the shape or size, or disposition of peculiar features. Every one has heard of, or may remark in portraits, the hereditary thick lip of the Imperial House of Austria. Many persons now living have had the opportunity of tracing the lineaments of our own Royal Family through at least three generations. The sisters of one of our English dukes are remarkably handsome young women, and bear to this day a striking resemblance to the portraits of their beautiful ancestress, the celebrated Nell Gwyn. And independently of the general cast of features, we trace these family-likenesses in minute or unequivocal particulars, as the colour of the hair and eyes, the shape of the limbs, the stature of the body, and so on: nay, in more decided peculiarities than these, in points of unusual formation. You have heard, probably, of the American calculating boy, Zerah Colburn. A great number of individuals of his family, descended from a common ancestor, had six fingers and six toes instead of five. The peculiarity was transmitted through four successive generations; and probably, could his pedigree have been further traced, through many more. I am myself acquainted with a gentleman who had the misfortune some years ago to have a bastard child laid to his charge. At first he had some misgivings on the subject, and suspected he might have no title to the credit (or I should rather say to the discredit) of the imputed paternity; but all his scruples were satisfied when he found that the child had six fingers on each hand, for he had himself possessed two small supernumerary fingers, which had been amputated when he was an infant. Haller gives an account of a web-footed family, descended from a mother in whom that configuration existed. There is now living in London a musical composer of some celebrity, in whose person nature has played a similar freak; and whose father, grandfather, and great-grandfather, were all web-footed before him. Beyond this point his information does not reach. I am indebted for the knowledge of this instance to one of my former pupils, Mr. Cooper of Grafton-street.

Not only the complexion, the features, the stature of the parent, but the various successive phases of the parent's life, mental and corporal, of health and of decay, are often copied and repeated in the child. In the absence of disturbing agencies, the son attains maturity, becomes gray or bald, acquires a stoop or a round belly, loses his teeth and his memory, at about the same age, and after the very same manner, with his father. Particular forms of degeneration and disease unfold themselves at similar periods in both; and thus it is that certain maladies, the tendency to which is interwoven with the original texture of the body, are rightly deemed to be hereditary maladies.

Now there is one very curious circumstance observable in regard to these family-likenesses, namely, that they may fail to appear in the child, and yet appear in the grandchild: may skip over a generation or two; may, after lying dormant, break out, as it were, in some collateral branch of the family tree.

This not only proves that certain physical peculiarities may be transmitted, but it discloses this remarkable property, that peculiarities *not presented nor possessed* by the parent may nevertheless be *transmitted* by him. And this evidently opens a wide field for the operation of hereditary tendencies. A person is not to consider himself as necessarily free from a disposition to consumption or to gout, because his parents have never shown any symptoms of those disorders.

When one parent only bears the transmissible tendency, the disease appears to be most apt to break out in the children who most resemble that parent in their physical conformation and appearance. Yet this is not a universal law. I am acquainted with a gentleman who has lost several brothers or sisters by phthisis. The fatal disposition is known to exist on his mother's side, while his father's pedigree is believed to be quite free from it. All the children that have hitherto become consumptive have resembled the mother in bodily configuration and features, except this

gentleman, who is like his father's family, but who, nevertheless, labors under unequivocal consumption.¹

It becomes a very interesting, and a very important question, whether *acquired* peculiarities can be transmitted. I have been told, by a gentleman attending the class, that he knew a man who, having been accidentally deprived of sight, afterwards propagated blind children. I believe, however, such an event to be uncommon. Dr. Prichard is of opinion that all original or connate bodily peculiarities tend to become hereditary, while changes in the organic structure of the individual from external causes during life, end with him, and have no obvious influence on his progeny. Although this general law is probably true, I doubt whether it be yet sufficiently established by a reference to actual facts.

I need scarcely say a word respecting the importance to medical men, and indeed to all men, of a knowledge of these hereditary dispositions. Such knowledge ought to regulate, in some degree, the choice of persons wishing to marry. Where both parents have a decided tendency to any complaint, there will be a double probability of a diseased offspring. Lawful intermarriages between members of the same family are often highly objectionable on the same score. Any inherent defect or morbid propensity is aggravated by what cattle-dealers call "breeding in and in."

Again, if it be known that in any family a hereditary proclivity exists—to gout and gravel, for instance, or to consumption—this knowledge ought to warn every individual of that family sedulously to avoid the causes which foster and develop these diseases; and medical men, possessed of the requisite information, may give most valuable advice and instruction on these points.

LECTURE VIII.

Symptoms. Their Uses in Relation to the Diagnosis, the Prognosis, and the Treatment of Diseases. Signs, as distinguished from Symptoms. Pathognomonic, Commemorative, Direct, and Indirect Symptoms. Examples of Symptoms as they consist of uneasy Sensations, disordered Functions, or changes of Sensible Qualities.

WE are perpetually reading and talking about *symptoms*: and no wonder, for symptoms are the signals by which we learn that disease is present; the evidence upon which our whole craft proceeds. We are always, therefore, observing symptoms, analyzing them, striving to interpret their meaning, to ascertain what they denote. Without a knowledge of symptoms we can have no knowledge of the art of physic. Sagacity in penetrating the import of symptoms constitutes a great part of the skill of an able physician. We shall find it useful to take a cursory view of semeiology, and to familiarize our thoughts with some of the cardinal symptoms themselves, before we speak of them in connection with particular diseases.

What do we mean by a symptom? *Συμπτώμα*—"Something that happens concurrently with something else." Symptoms, they say, are *coincidences*, but this is merely translating the word *συμπτώματα* into English through the Latin. Symptoms are sometimes defined to be *morbid phenomena*—"anything observed in a patient out of the course of health." But in forming our estimate of disease, we must often take into account functions that are regular and undisturbed: these have been said to furnish *negative* symptoms. For my own part, if I were called upon to define a symptom, I should say, "Every thing or circumstance happening in the body of a sick person, and capable of being perceived by himself or by others, which can be made to assist our judgment concerning the seat or the nature of his disease, its probable course and termination, or its proper treatment: every such thing or circumstance is a *symptom*."

¹ This gentleman, an eminent London physician, has died since this lecture was given.

And I wish you to take notice at once, that is for the three purposes just adverted to, that we cultivate the study of symptoms, viz. : —

First, To ascertain the seat and the kind of the disease under which our patient is labouring: in technical language, to pronounce the *diagnosis*. I am no great friend to technical phrases when they can be avoided without inconvenience; but in some cases short terms of art save us a great deal of tiresome and needless circumlocution.

A *second* object of the study of symptoms is to enable us to foresee and foretell the probable course and issue of the disease; in other words, to frame the *prognosis*.

And a *third*, and paramount use of a knowledge of symptoms, is to direct our *treatment* of the disease.

I suspect that the immense importance of the first mentioned of these three objects — the diagnosis or *recognition* of disease, is not always clearly seen, either by students or practitioners of medicine. Sometimes we are obliged to prescribe for a malady, although we are in great uncertainty, perhaps in total ignorance, respecting its nature or its situation. But this is always unsatisfactory. On the other hand, when we have ascertained where and what the disease is, we apply with much more confidence, precision, and comfort, those rules for its relief which we have acquired by our own observation, or have been taught by others. This, however, is a very limited view of the importance of an exact and true diagnosis. Diagnosis forms the indispensable basis of all *advances* in physic as a practical art. There is a common saying, that the knowledge of what a disease is, is half its cure. In one sense this may sometimes be true, but in another sense it is not so. Almost all that we know concerning the proper treatment of the sick is *originally* derived from observation, *not* of the *nature of diseases*, but of the *effects of remedies*. That rhubarb will purge, and opium lull to sleep, and loss of blood occasion faintness, are truths which experience alone could suggest, and successive trials alone confirm. They are purely empirical truths. No one could guess them beforehand. No skill in the discrimination of disease has even a tendency to teach them. In some few cases, indeed, we *see* that certain *mechanical* derangements exist, which are manifestly capable of *mechanical* relief. When parts of the body are displaced, as in herniæ and dislocations; or when distension and pressure are evidently produced by accumulated fluids; the mechanical remedies are at once suggested by the physical and obvious faults. But with such exceptions, diagnosis does not, of itself, afford us *any* direct information as to the cure of diseases; but it does *this* — it defines and fixes the objects about which observation is to be exercised, and experience collected. When we can once identify a given diseased condition, we obtain the privilege of watching the behaviour of that diseased condition, again and again, under the operation of therapeutic measures; and from that time the increase of our knowledge concerning the appropriate management of that particular disease becomes progressive and sure. The term experience is obviously misapplied, and the results of all observation are vitiated, when any doubt exists about the sameness of the objects contemplated. It is mainly to this imperfection in the diagnostic part of medicine that we must attribute the uncertainty and variation, both of doctrine and practice, which have brought so much suspicion, and reproach, and ridicule, upon the science we profess. *False* experience, if I may use such a term, has greatly hindered the progress of the healing art: and *false experience* springs from *false diagnosis*. A man will tell you that he has cured a score of cases of advanced phthisis; but he has deceived himself: they were not instances of true phthisis, but simply cases of chronic inflammation, with puriform discharge, of the mucous membrane of the bronchi. He publishes an account of his success, and of his plan of treatment; and thus he deceives others also: and thus he retards the science which he fondly and conscientiously believes he is promoting. Accuracy of diagnosis, then, as the foundation of all true experience, and as the guide to rational treatment, cannot be too highly estimated, nor too diligently sought after. It has been wonderfully improved during the last thirty years.

The *prognosis*, or foreknowledge of the course and event of diseases, has but little connexion with the promotion of the art of healing; but it is not on that account unworthy of our attention. Both physician and patient find their advantage in the capability of the former to determine whether a disease be remediable — to foresee the changes that may be expected in its progress — to predict the manner in which it will terminate. Knowledge of this kind opens to us a fair and honourable source of

credit and reputation; and it begets a degree of confidence towards us, which is beneficial, not merely to ourselves, but to our clients. Our influence over a sick person, and the efficacy of many of our remedial measures, are remarkably increased by our evident acquaintance with the nature of his complaint, and by the reliance which he therefore places on our skill and judgment. It is often of material consequence, in another point of view, that the fatal character of a disease should be plainly perceived. A sick man, made aware of his danger, is furnished with a motive and an opportunity for arranging his worldly affairs, in the settlement of which the future comfort and happiness of his family may be very deeply concerned; for making his will; and for more solemn preparation for the awful change that awaits him. For these reasons physicians have, in all periods, endeavoured to read, in the phenomena presented to them by diseases, the event to which those diseases severally tend. To form an accurate opinion on this head is, however, one thing—to *divulge* it, another. There is always some hazard of losing, instead of gaining credit, by strong statements, and confident predictions of the death or the recovery of a patient. If you give an unfavourable prognosis, you incur the risk of losing your patient altogether. His friends argue, very naturally, that you are not infallible, that you may be wrong, that if *you* know of no means of safety for him, some other practitioner may; and they *will* grasp at whatever straw comes near them. Do not suppose that this is a merely selfish view of the matter. It is often of much moment to the patient himself, that he should not be tempted to put his life under the charge of impostors, who will feed his hopes, and promise largely, and torture him perhaps with their discipline, and have no mercy upon his pocket. Many an instance have I known of persons dying of consumption, who, when given over by their regular attendants, have been brought to London at considerable expense, exchanging the many comforts of home for the inconveniences of a hired lodging, that they might be *cured* by that ignorant, cruel, and rapacious quack, Mr. St. John Long. There are other reasons, too, why we must sometimes conceal the truth from our patients. It often happens that a person is extremely ill, and in great danger, but may yet recover if he be not informed of his peril. To agitate a person in this state by telling him that he is likely to die, is to lessen, perhaps to destroy, his *chance* of recovery. You kill him if you take away his hope of living. It must be confessed that the duty of the physician in these cases is very painful and embarrassing. The patient and the patient's friends are urgently inquisitive to know whether there is any danger: or whether he is not yet out of danger. The rule which I have always adopted in circumstances of this perplexing kind, when I see clearly that the case is hopeless of cure, is to fix as well as I can upon that person, among the family or friends of the patient, to whose prudence the real state of the matter may be the most safely confided. If I think there is a possible chance of recovery, and that the patient's knowledge of his danger would diminish that chance, of course I urge the necessity of speaking to *him* with assumed cheerfulness and confidence. If I see that the case is absolutely and inevitably mortal, either soon or at some little distance of time, I leave it to the discretion of the person with whom I communicate, to disclose or conceal my opinion as he or she may think best. There are, I believe, practitioners, who make it a point, on principles of worldly policy, *never* to speak despairingly of a patient; but I cannot regard such a rule of conduct as honest, or justifiable, or consistent with one's Christian duty.

Now I would have you observe that symptoms do not serve equally or indifferently the three several purposes of which I have been speaking. The same symptom or set of symptoms may indeed at once reveal the nature of the disease, and foreshow its result, and point to its treatment. When we have discovered what the disease is, we may want no further information to tell us how it will terminate, or how we are to prescribe for it. A man previously sound and well, shivers, then becomes hot, and afterwards sweats, and then reverts to his natural state of comfort and good health; and the same series of phenomena recurs every other day. We pronounce the disease to be *ague*; we predict that, in this climate at least, the patient will recover; and we give him *quina*; all upon the strength of the same set of symptoms. But this is not necessarily the case: certain symptoms may disclose to us what the malady is, and where it is situated; other symptoms teach us whether our patient is likely to survive or not; and a still different set instruct us what is the proper method of cure to be

attempted. We see a number of little pustules scattered over his skin, and we know that our patient is labouring under small-pox. His chance of recovery will be singularly different, according as the spots upon his face run together, or remain separate and distinct from each other: and we investigate the state of his pulse, and of his breathing, of his bowels and of his brain, before we can venture to prescribe for him. Those symptoms, or combinations of symptoms, which declare the place and nature of the disease, we call *signs of disease*; those which teach us what to do, we call *indications of treatment*. We speak also of prognostic *signs*. By keeping these distinct ends of the study of symptoms in mind, we shall be enabled to group them to advantage, and to avoid huddling confusedly together symptoms that speak, not indeed a different language, but upon a different topic. The ancients, who knew but little of the intimate nature of diseases, but who paid great attention to symptoms, have laid down most admirable rules in respect of prognosis: which shows not only that the prognostic signs are more easily made out, in many cases, than the diagnostic, but also that they may be independent of them.

I have just spoken of *symptoms* as being *signs*. These words are not, however, exactly synonymous, although they are frequently employed as if they were so. Even those medical writers, who admit a distinction between them, have not always succeeded in clearly pointing out the difference. Signs are deduced from symptoms, by arranging and comparing these, and noticing the circumstances under which they occur. Symptoms are obvious to all persons alike — to the nurse as well as to the physician: signs, for the most part, are such to medical eyes alone. Let me try to make this plainer by the help of an illustration. Symptoms may be considered as resembling so many *words*. When taken separately, or when put together at random, the words have no force or signification. Arrange them in due order, reduce them into a sentence, and they convey a meaning. The sentence is a *sign* or expression of something which is thus revealed. Symptoms become signs when their import can be interpreted.

A certain crackling sound, of which I shall have much to say hereafter, is heard (we will suppose) in some part of a patient's lung, by the ear applied outside his thorax. The sound is a symptom; any one who listens may perceive it. It is even so far a *sign*, that it denotes the unnatural presence of a liquid in the lung, and the passage of air through that liquid. But the liquid may be one of several — mucus, or serum, or pus, or blood; we cannot tell by the sound alone which of these it is. But if we learn that the person in whose lung the sound is audible has been ill for a day or two only, that he has pain in his chest, cough, embarrassed breathing, and fever, we conclude that he is labouring under that serious disease, *inflammation of the lung*. The crackling sound alone could not assure us of this; nor without the addition of this sign could the pain, the laboured breathing, the cough, or the fever. Taken collectively, the symptoms constitute a *diagnostic sign*, and bespeak the existence of pneumonia.

Sometimes a symptom, or set of symptoms, becomes a sign, by its relation to what has gone before, or to what follows it. To adhere to our illustration, the meaning becomes evident from the context. By comparing, at short intervals, in the supposed case of pneumonia, the extent and character of the sounds heard during respiration, we ascertain whether the disease be advancing or receding; and thus convert the sounds, or their variations from day to day, into a *prognostic sign*.

We always strive, then, to penetrate beyond the symptoms to the disease of which they are significant. But we do not always succeed in this, and when we do not (as in the case of ague), we are driven to the necessity of regarding the combination of symptoms as the disease.

You will often hear of *pathognomonic* symptoms. A pathognomonic symptom is one which, when it occurs, settles infallibly the nature of the malady; becomes a positive sign or token of a particular morbid condition. But there are very few symptoms, if there be any, which, taken singly, can ever be said to be strictly pathognomonic signs; yet a symptom which in itself possesses little or no value may become very significant when conjoined with others.

Much light is often thrown upon symptoms by what the French call *commemorative circumstances* — that is, by a knowledge of the previous history and condition of the patient. For example, a person may have palpitation and other marks of disor-

dered action of the heart, and doubts may exist whether these symptoms depend or not upon organic disease of that organ. The question is often determined in the affirmative, by our learning that the patient has had one or more attacks of acute rheumatism of the joints. In like manner, previous hæmoptysis may serve to identify suspected phthisis.

There are some other general divisions of symptoms, which it is useful to attend to. Thus some symptoms are said to be *direct* and others to be *indirect* symptoms. Direct symptoms relate to the very part which is affected; indirect symptoms are such as "declare themselves through the medium of some other parts, or through the medium of the constitution at large." There are some cases in which the direct symptoms are of much more value than the indirect; and there are other cases in which those which are indirect are the most important; and there are yet many more which require for their elucidation a knowledge of both the direct and the indirect symptoms.

Again, there are many symptoms of which we receive no information, except through the statements made by the patient himself; and there are many others of which we learn the existence by means of our own observation, by the exercise of our several senses. The relative importance of *these* varies too in different cases. Of course those symptoms which we are able to ascertain for ourselves are the most *trustworthy*; but both sorts of symptoms shed mutual light upon each other. We should constantly be making mistakes if we relied solely upon what our patients tell us. On the other hand, the value of the information we derive from their statements is made apparent by the difficulty we are apt to experience in investigating the diseases of children; of those who are dumb; or, what is much the same thing, who speak no language that we understand.

Now, setting aside that notice of the healthy functions which is sometimes necessary in order to determine the relative value and meaning of other symptoms, and regarding those symptoms only which consist of *morbid* changes, they may all be classed under three heads: 1. Uneasy, unnatural, or impaired *sensations*: 2. Disordered or impeded *functions*: and 3. Alterations of structure or of appearance; changes of *sensible qualities*. When these last come within the direct cognizance of our senses, they are called, usually, *physical signs*.

Uneasy or altered sensations we can only be aware of through the testimony of the patients themselves. The symptoms belonging to the other classes fall, generally, under our own notice.

Uneasy or altered sensations comprehend a large class of morbid symptoms. By their occurrence persons sometimes become conscious that they are unwell before any other symptoms are observable. Of all the uneasy sensations *pain* is the most common and the most important. It rarely happens that it is not felt, at one period or another, in inflammatory disorders; and it very often occurs, and is very acute too, when there is no inflammation at all. I shall have occasion, in a subsequent lecture, to lay before you the criteria between pains that accompany inflammation, and pains that are independent of it. Upon that point of distinction the whole question of treatment commonly depends; and it is often a most difficult point to determine.

There are many different kinds and degrees of pain. Different kinds of morbid action are accompanied by different kinds of pain; and the same kind of morbid action—inflammation, for example—produces different modifications of pain, according as it affects different parts. The pain that belongs to inflammation of the lungs differs from that which is felt in inflammation of the bowels. Bones, muscles, tendons, ligaments—the bladder, the kidney, the uterus—all modify, in a manner peculiar to themselves, the pain that is produced in them by injury or disease. Different epithets are given to the different varieties of pain—*i. e.*, persons endeavour to explain how they feel by likening their sensations to something which they have felt before, or fancy they have felt. Thus we hear of *sharp* pain—*shooting* pain—*dull* pain—*gnawing* pain—*burning* pain—*tearing* pain; and so on.

If pain be felt in a part, only when it is touched, *i. e.* when *pressure* is made upon it, the heightened sensibility is called *tenderness*:—the part is said to be tender. This is a very important kind of pain, as we shall see hereafter. A part may be both painful and tender: or painful without being tender: or tender without being otherwise painful.

Pain often takes place, not in the part really affected by disease, but in some distant part. Inflammation of the liver or diaphragm may cause pain in the right shoulder: the mechanical irritation of a stone in the bladder produces pain at the extremity of the urethra: inflammation of the hip-joint occasions pain in the knee: disease of the heart is often attended with pain running down the left arm: many headaches result from irritation of the stomach. We call these, instances of indirect or sympathetic pain. Some of them admit of no very obvious explanation:—others have been ascribed to connexions between the sentient nerves of the two parts; “especially when the part really injured is internal, and that to which the feeling is referred is external, and both derive their sentient nerves from the same larger branches.” You will perceive that a due estimation of these sympathetic pains is of no small importance.

I may observe of pain in general, that it is differently felt—or at any rate differently complained of—by persons of different constitutions and temperaments. There are even, I fancy, *national* differences in this respect. I have been present, as you may believe, at a great number of surgical operations, and I have been struck with the different degrees of patience with which the same operation has been borne by Irishmen and by Scotchmen. The Irishman, generally speaking, either feels more acutely, or gives more free vent to his feelings in cries and exclamations: the Scotchman, on the contrary, most commonly preserves a resolute silence. In complaints that are associated with low spirits and hypochondriacal feelings, there is reason to believe that the pain spoken of often depends, in a great degree, upon the eager attention that is paid to it. The accounts given by such patients of their sufferings are always to be received with a grain of allowance; and this is sometimes an embarrassing circumstance in practice. Patients take it ill if they do not seem to be implicitly credited; and yet if they are not convinced that much of what they suffer depends on their undue attention to it, they will never get well. You will often find that they cease to feel pain—*i. e.*, they forget to think of their complaints—when their attention is otherwise strongly arrested; as by conversation, or by music. I adverted to this principle in my last lecture.

The pain of various painful diseases admits of relief, in various degrees, from the resources of medicine. The pain, more dreadful and more dreaded, and so long exacted, in the capital operations of surgery, as the inevitable price of future ease, or as the instant ransom of life, has happily found, in our times, its specific antidote. By the mere breathing, for a few minutes of an invisible vapour, the corporal sensibility is laid asleep, and the knife, the gorget, or the cautery executes, at leisure, and unfelt, its terrible but salutary work. To “charm ache with air” is no longer the poet’s mock. Half a century ago was this blessed invention suggested by the sagacious mind of Sir Humphrey Davy; but his hint fell fruitless upon our negligent ears, and the glory and the triumph of the discovery (for in such things to proclaim and publicly to apply is practically to discover) was reserved for our brethren beyond the Atlantic. The safety, as well as the efficacy of this application of the vapour of æther, of chloroform, or (perhaps I may add) of amylene, has now been ascertained by abundant experience. And if we consider what it has done, and what it promises—the vast amount of torturing pain which already has been spared to thousands of our race, and which countless generations yet unborn may thus escape—and not the bodily anguish only, but the mental terrors of its prospect, and the agitating recollections of its endurance—and, still further, the improved chance of ultimate well-doing which the avoidance of so severe a shock to the nervous system is believed to confer—we shall scarcely deem the proposal extravagant, which has been made by one of our hospital physicians, that for so merciful a boon to suffering humanity, public thanksgiving should be humbly offered up to heaven in our churches.

Here, as in many other instances, it is curious to note how closely a great discovery may be approached, and yet be missed. Long before its power was used to prevent the inflicted pain of surgical operations, the vapour of æther had been successfully employed to suppress the inbred sufferings of natural disease. A former patient of mine told me this story of herself. She had been sorely tried, in her earlier years, with paroxysms of urgent dyspnoea, frequently recurring; and her life was thought to be in danger. After fruitless trials of various other remedies, the following method was

adopted, with the happiest result, under the advice of a physician of high promise, who died young, the late Dr. Woolcombe, of Plymouth.

About two tea-spoonfuls of sulphuric æther were poured into a saucer, which was placed on her lap, and over which she breathed, as she sat gasping in bed, with a shawl thrown over her head to prevent the escape of the vapour. Very soon a delightful sensation of tranquillity ensued; she felt (I quote her own words) "as if going to heaven in the most heavenly way;" and presently she sank back unconscious. As soon as this happened, her husband (the late distinguished Admiral of the fleet, Sir T. Byam Martin), by whom the process was managed, withdrew the shawl, and in a short time Lady Martin gradually recovered, breathing calmly.

This mode of quieting her attacks of asthma was begun in 1806, a few years after the publication of Sir Humphry Davy's hint: and it was repeated again and again, sometimes twice in the same day, for a very considerable period. Lady Martin survived the prediction of her speedy death for forty-three years.¹

Besides pain, in all its modifications, there are many other, and very interesting, uneasy sensations. *Itching* is an uneasy sensation nearly allied to pain. As severe mechanical irritation will cause pain, so a slighter degree of it will cause itching. Itching occurs in many cutaneous diseases, and it gives a name to one of them, which is called emphatically the *itch*. And the Latin word signifying the same sensation, *prurigo*, is made use of to denote other forms of disease of which itching is the most prominent symptom. It often affects some one of the natural outlets of the body. It occurs about the rectum, from the motions of little worms that nestle in the lower part of that gut. This *prurigo podicis*, which does not always depend on the cause just mentioned—and the *prurigo pudendi* in the female—are sometimes most distressing complaints; harassing the patients continually, preventing sleep, excluding them from society, and requiring medical treatment. Acrid matters in the intestines will sometimes produce a kind of itching there; and the call to void the fæces is perhaps more akin to itching than to any other sensation: sometimes, indeed, it amounts to pain. The tickling often felt in the windpipe and provocative of cough, appears to be of the same nature. Tingling and pricking are sensations which have also some analogy with itching.

Nausea is another uneasy sensation. It is sometimes a *direct* symptom of disease

¹ "Is there anything whereof it may be said, See, this is new?"

In turning over the pages of an Italian work, presented to the British Museum in 1853, one of the trustees lit upon the following curious passage:—

"Fra i Toscani scrittori di qualche pregio in chirurgia, mi si presenta in primo luogo Ugone da Lucca, nato poco dopo la metà del secolo XII. della nobile famiglia Borgognoni. Esse Ugone fu capo della setta dei cerusici, che medicavano le ferite con vino, stoppa e conveniente legatura, e gli riusciva felicemente. Aveva anche della pratica nella chimica; e di lui si ha il processo per la sublimazione dell' arsenico, descrittoci da Teodorico suo figlio. Il quale ci descrive anche certo olio *de lateribus*, preparato chimicamente da suo padre, ed un suo caustico potentissimo, ed un *soporifero* che per mezzo del solo odorato assopiva i malati, in occasione di operazioni dolorose, che dovessero soffrire.

"Notizie sulla storia delle scienze fisiche in Toscana cavate da un manoscritto inedito."—di Giovanni Targioni-Tozzetti. Firenze, 1852.

So that the practice of benumbing the sensibility of a patient about to undergo a surgical operation, by causing him to inhale an anæsthetic vapour, is as old as the 12th century.

During the earlier part of his life, the Theodoric mentioned in the foregoing extract studied surgery, and followed his father Hugo's calling. He afterwards entered the Church, and became bishop, first of Bitonto, and, finally, of Cervia. In his *Chirurgie*, of which the library in the Museum contains more than one copy, he made public the chemical preparations and the rules of his art, which Hugo had jealously kept secret from all, except his son. I owe to Mr. Panizzi's kindness the opportunity of transcribing the formula for preparing and using Hugo da Lucca's *soporificæ*.

"Confectio saporis a chirurgia facienda secundum Dominum Hugonem sic fit.

"R. Opii, et succi mori immaturi, hyoscyami, succi conidij, succi florum mandragoræ, succi ederæ arboreæ, succi mori sylvestris, semi-lactucæ, seminis lapathij quod habet poma dura et rotunda, et cicutæ, ana unciam.

"Hæc omnia in unum commisce in vase æneo: ac deinde in istud mitte spongiam novam, quod totum ebulliat: et tamdiu ad solem canalicularibus diebus donec omnia consumat: et decoquatur in ea: quoties autem opus erit, mittas ipsam spongiam in aquam calidam per unam horam: et naribus apponatur: quousque somnum capiat: qui incidendus est: et sic fiat chirurgia: qua peracta ut excitetur: aliam spongiam in aceto infusam, frequenter ad nares ponas."—THEODORICI DE LUCA EPISCOPI CERVENSIS *Chirurgiæ*, lib. 4, ch. viii.

or disorder of the stomach, to which the sensation is referred. Sometimes it is a very important *indirect* symptom, taken in conjunction with others, of disease in some part at a distance from the stomach—in the kidney, for example, or in the brain. The nausea which is so troublesome to pregnant women is another instance of a morbid sensation sympathetic of some change in a distant organ.

Another example of an uneasy sensation we have in *giddiness* or *dizziness*—technically *vertigo*. It sometimes results from disease within the head; sometimes it is an indirect consequence of disorder of the stomach; or of mere debility and an approach to syncope.

Patients will also complain of an undefinable sensation which they usually call *sinking*—a sensation which is referred to the epigastric region. This is frequently a source of much distress to hysterical women; and it is occasionally the forerunner of death at the close of severe diseases which have a tendency to end fatally in the way of syncope.

Many other symptoms might be mentioned which belong to this class of uneasy sensations, and for our knowledge of the existence of which we must depend upon the accounts given us by the patients themselves. Sensations of weight; of tightness and fullness; drowsiness, tenesmus, strangury, heartburn; and various depraved conditions of the special senses. In the majority of diseases the appetite is lost or impaired; but sometimes excessive hunger accompanies and denotes disease. We occasionally derive the first suspicion of the existence of diabetes from the preternatural keenness of the appetite. Thirst is a very constant and striking symptom in all febrile and inflammatory disorders: and in the disease just now mentioned, diabetes, it frequently constitutes the whole distress of which the patient is sensible. The appetite may be perverted, as well as deficient or excessive. Chlorotic girls will eat cinders, egg-shells, sealing-wax, slate-pencil, and such trash. So women who are pregnant either have or pretend to have inordinate longings for particular kinds of food—longings which are evidently fostered by encouragement. They are not, I believe, common at present in this country; and they are less frequently heard of among the poor, who have not the means of gratifying them, than in the higher ranks of society.

The class of uneasy sensations, you see, then, is a very large one, and some of the morbid feelings are of very great moment. However, there are not many diseases which consist altogether of uneasy sensations; and when we find that pain or uneasiness is complained of in any part or organ, we next proceed to inquire whether the *functions* of that part or organ are disturbed or suspended. If we discover any interruption or derangement of function, we have additional reason for concluding that the part so affected in its sensations and in its functions is actually the seat of disease. This is an inquiry which we can prosecute with much less assistance from the patient himself; and mostly with no assistance at all; and even in spite of any erroneous opinions which he may have formed, and is anxious to state upon the subject. The study of disordered functions is of great practical value.

The functions of the brain and nerves—of the heart and blood-vessels—of the respiratory apparatus—and of the digestive organs—are all of vital consequence.

Some of the impeded or disordered functions which relate to the brain and nerves are, in fact, identical with the last class of symptoms, and consist of altered or morbid *sensations*; sensation being one of the natural functions of those parts. Depravations, for instance, of the sense of touch; numbness; the total absence of sensation, which we call *anæsthesia*. Symptoms of this kind do not constitute primary diseases, but they often portend or accompany very serious alterations in the brain, or in some part of the nervous system: and it is from that circumstance that they derive the great interest and importance which belong to them. The same may be said of perverted conditions of the other senses. The sense of vision is often impaired, and in various ways and degrees, from mere dimness or imperfection of sight, to total blindness. And this total blindness may occur without any other *apparent* disease, the humours and fabric of the eye itself being in all evident respects healthy and right: it may come on, too, so gradually, and increase so slowly, as not to be perceived for a long time, even by the patient himself. Mr. Day, the great blacking man, of the firm of Day and Martin, who died not long since, was almost entirely blind. He told me he first discovered that the sight of one eye was gone, one day when he attempted to look at a distant object through a telescope. He could see nothing, and he imagined that the

little brass plate which slides over the eye-glass had not been withdrawn. There was, however, no such obstacle; and he too soon found that when the other eye alone was closed, he was in total darkness. This state of blindness is called amaurosis, and it may result from pressure made upon the retina, or upon the optic nerves, or upon the brain at the origin of those nerves. There are other causes also, to be mentioned hereafter, of amaurosis. Its approach is sometimes marked by the fallacious appearance of black spots upon the objects the patient is looking at, or floating before him like flies or cobwebs in the air — *muscæ volitantes*. Some of the other depravations of sight are still more extraordinary, and except that they are not uncommon, might almost be considered fabulous. Thus persons sometimes see things around them apparently in motion, when in truth they are not so. This is, in fact, a symptom I have mentioned before — vertigo. When the patient shuts his eyes, and consequently can see nothing, he feels as if he were himself turning round, while in reality he is at rest. Persons in this state fancy sometimes that the bed on which they lie is sinking rapidly down with them into some abyss. A still stranger depravation of the sense of vision is that in which a person sees only one-half of an object at which he is steadfastly looking. One man, in passing along the street, imagined that everybody he met had only one eye. The late Dr. Wollaston was subject to this optical defect: he frequently found that only one-half of the object he looked at was visible: and he wrote an ingenious paper in the Philosophical Transactions to explain this. After his death a tumour was found in his brain, interfering with the optic nerves. The celebrated Mr. Abernethy had once a temporary affection of the same kind, dependent, no doubt, upon some slight and transient injury of the brain. He was thrown, I believe, from his horse — at any rate, he received a violent blow on his head, which stunned him; and when he had recovered a little, he was taken home in a hackney-coach. On his way he amused himself with reading the names of the trades-people placed in front of the shops, and he was greatly surprised to find that one-half of each name — the last half — seemed blotted out. He described this in his lectures, after his whimsical manner, by taking his own name as an example: “I could see as far as the *ne* (said he), but I could not see a bit of the *thy*.”

Those very wonderful cases of spectral illusion which sometimes occur, come within the class of symptoms we are now considering; they throw a strong light upon many of the well-authenticated ghost-stories — which were in fact merely instances of disease or derangement in the brains of the ghost-seers. It would be out of place to go into any detail upon this interesting subject here. You will find some excellent examples of these spectral illusions in Dr. Hibbert's book on *Apparitions*, in Sir David Brewster's *Natural Magic*, and in Sir Walter Scott's *Letters on Demonology*.

The sense of hearing is liable to analogous disorders. Sometimes it becomes preternaturally acute; and this is a bad symptom when it does occur. I was called a year or two ago to see a gentleman in the Temple; he had been taken ill only a few hours before, but I found him dying; the pulse was gone from his wrist; and his skin was cold. His intellect, however, was entire, and he complained of nothing but the distress he felt from the loud noises that were made by those around him, in moving about and in speaking, although, in fact, all noise was as much as possible suppressed, and conversation was carried on in whispers: but his hearing was painfully acute. He died the same evening; I believe of an irregular form of cholera. It is always right that patients should be protected from the irritation which might arise from this source; for that degree of noise which would not interfere with the sleep of a healthy person will often not only prevent it in a sick man, but bring on delirium, and aggravate greatly the disease under which he labours. The custom of strewing the streets with straw before the houses of those who are seriously ill, is, in many cases, a very proper precautionary measure.

The opposite fault, *obtuseness* of hearing, is much more common. Deafness is frequently attributable to some physical imperfection in the organ of hearing. But it is with cases in which it has a deeper origin that the physician is chiefly concerned. It often occurs in fever, and is not then thought a bad symptom: it certainly is a much less unfavourable circumstance than morbid acuteness of hearing; and it probably depends upon a disordered state of the brain, which is not in itself very dangerous.

What is called *tinnitus aurium* is an instance of the depravation of the sense of hearing. It seems sometimes to result from the too strong throbbing of the arteries.

It occurs in many disorders, and is not unfrequently a symptom of diseased cerebral vessels, and a precursor of apoplexy or palsy. It is sometimes in itself extremely annoying. Curious and undefinable sounds are heard by some patients—sounds like a rushing wind, like the falling of a cataract, the ringing of a bell, or the beat of a drum. A female patient of mine in the Middlesex Hospital last year, who had disease of the bones of the ear, with symptoms that threatened some implication of the brain, affirmed that she heard a perpetual noise in her ear like the singing of a tea-kettle. I have lately been consulted by a gentleman from the country, who had no other complaint than a constant hissing, which worried him greatly, in one ear. Another had watched with curious anxiety, and described to me very graphically, the successive variations which this troublesome symptom underwent in his own person. It began suddenly, with some headache. At first it was a loud roaring, like that of the sea; in a few days it came to resemble exactly the whistling of the wind among the trees in winter; afterwards he could have believed that the room was filled with humming gnats; and finally the noise settled down into the gentle sound of a distant waterfall. It haunted him incessantly for seven years. Then came an attack of shingles on the right side of his head, face, and neck, and the noise at once ceased. It left him free for a year and a half, and then returned as before. Sir David Brewster relates the case of a lady, subject to spectral illusions, whose ear was mocked by unreal sounds, as her eye by unreal visions. Being in her right mind, and perfectly aware of the infidelity of her senses, she repeatedly heard, not vague noises merely, but voices and sentences, when none were uttered.

Affections of the intellect—of what are sometimes called the internal senses—are very common, and very important symptoms of disease. Incoherence of the trains of thought—palpably false belief—extravagant perversions of the judgment. These affections are sometimes considered as primary diseases themselves; they very frequently accompany certain febrile diseases; and they are not uncommon in diseases that are unattended with fever. There is more or less derangement of the internal senses from the very beginning of continued fevers. The power of attention is impaired. That kind or degree of mental exertion which would afford gratification and amusement when we are well, becomes laborious and irksome when we are ill; and to compel, or to urge the attention, under such circumstances, is injurious. This state is probably only the first degree of delirium, and therefore these slight approaches to derangement of the internal senses are by no means to be disregarded. It is curious that the delirium of fever is always most marked during the night; this seems to be owing to the circumstance that the erroneous notions and wandering thoughts of the patient are not corrected by impressions made upon his external senses. You will find, conformably with the same principle, that your patient sometimes ceases to be delirious upon your visiting him: the sight of a new face rouses him for a time, but he soon relapses.

Strange infirmities of the memory there are, associated with cerebral disease, and justly to be regarded among its symptoms: large blanks in the backward gaze; fitful suspensions of the remembering power; partial glimpses of the past; resurrections of thoughts long buried in oblivion. I speak not of that natural decay of the memory which is noticeable in most persons as age creeps on, and which is one of the most affecting of the many warnings then vouchsafed to us, that the bodily frame is suffering dilapidation. Even of this natural decay there are some curious things to be noted. Recent events are retained with difficulty, and soon forgotten; while those of older date are easily and accurately recalled. This has been referred, and rightly, I believe, to the differing degree of interest and therefore of attention which the same objects excite in the young and in the old. It would seem as if the effort of attention stamped characters upon the material fabric, which are deep and lasting in the youthful brain, faint and sooner effaced in the aged. But disease may revive things long forgotten; a language long unspoken and unthought in: or blot out entirely all traces of definite portions of time gone by.

Voluntary motion is another function connected with the nervous system, and one which affords a great variety of important symptoms. Like the power of the senses, it may be excessive, or deficient, or perverted. Excess of voluntary motion is not common, nor very important. Maniacal patients sometimes exhibit an extraordinary

degree of muscular strength; indeed, in the delirium of fever something of the same kind may be observed.

But the opposite state, that in which the power of voluntary motion is deficient, muscular debility, is exceedingly common. Debility is an original and essential part of fevers. It appears before there has been time for it to be produced by the mere exhaustion of disease. It is not always proportional to the other symptoms, and does not necessarily imply any great degree of danger. This sudden and early weakness has been a very striking symptom in our two or three recent visitations of influenza. Persons previously in apparent good health would be seized as they walked along the street, and be glad to sit down in a shop, or a carriage, and to get home and go to bed. Young and strong persons would be thus rapidly prostrated.

In some instances debility does not appear till late in the disease, of which it then forms an important prognostic symptom, and an important guide for our treatment. It shows us that there is a tendency to death by asthenia, and we have to endeavour to keep the patient alive by supporting his strength as well as we can, this being the chief, or perhaps the only indication.

Debility is occasionally the principal symptom of the whole disease—as in hemiplegia, paraplegia, or in more partial palsy, palsy of one limb, even of a finger, or of a single muscle, as of the levator palpebrarum. This, though it may seem trivial in itself, is far from being so in reality; it often forms a fragment only of a most serious disease. From such partial manifestations of palsy we presage a more general and alarming attack; as the loosening of a few stones in the wall announces the commencing earthquake. A slight degree of paralysis affecting some of the muscles of the eye will produce a squint, and consequent double vision; and this occurs not only in hydrocephalus, when it is a most significant phenomenon, but also as a prelude to more extensive palsy. General palsy is sometimes prefaced by a similar affection of the tongue, producing a faltering and indistinctness of speech.

Spasm is an instance of disturbance and perversion of the power of voluntary motion. It consists in an irregular and violent contraction of muscular parts—involuntary, even when the voluntary muscles are concerned. Cramp is a familiar example of it; and we have been taught, since the cholera came among us, to regard cramp as sometimes a very formidable symptom: not formidable so much in itself, as formidable in respect to the condition that gives rise to it. Tonic spasm is the principal symptom also of that frightful disease—frightful in its phenomena and in its frequent fatality—tetanus. The convulsions of epilepsy and of hysteria, and the jactitation of chorea, are ordinary examples of the perversion of the function of voluntary motion. Sometimes convulsions bode great danger, sometimes none at all.

So also *tremor*, which is near akin to spasm, is a sign, frequently, of a morbid state of the greatest peril; while it is sometimes violent without being attended with the smallest hazard.

If we turn now to the great function of respiration, we shall find that it affords a very large number of morbid symptoms, and those of the highest importance.

Dyspnœa, difficulty of respiration, is one of the most prominent of these symptoms. It may depend upon various causes. In inflammation of the lungs or pleuræ there are several circumstances in operation to impede the breathing; for example, pain, which would be enough of itself; the effusion of lymph into the texture of the lung, or of serum into the cavity of the pleura, mechanically resisting the entrance of air. In dyspnœa the breathing is almost always most difficult when the patient is lying flat on his back. One reason for this is plain. In the supine horizontal posture the action of the diaphragm is obstructed by the weight and pressure of the adjacent abdominal viscera; and the erect position obviates this. Upright breathing, *orthopnœa*, has come to be considered as a distinct modification of dyspnœa. The patient cannot lie down.

Sometimes, as in asthma, the difficulty of breathing comes on in separate paroxysms; the respiration becomes all at once noisy, wheezing, and laborious. A person who had never seen any cases of this kind would imagine that the patient was at the point of death—that it was all over with him; but the most frightful of these attacks are seldom attended with any *immediate* danger. They depend frequently upon organic disease of the lungs, heart, or aorta: sometimes they seem to be purely spasmodic; sometimes to result from transient congestion of blood in the lungs.

Cough is a violent spasmodic action. A full inspiration is taken; then the glottis is closed pretty firmly; and in expiration the air is forced suddenly out, and with it, frequently, mucus, or other matters which had irritated the air-passages. It is for the most part an involuntary effort of nature to expel from the lungs things which ought not to be there. There are several varieties of cough. It is a symptom belonging to so many dangerous complaints — pneumonia, pulmonary consumption, and diseases of the heart—that it always demands strict attention. No one who has once heard it can ever mistake the *hooping* cough. There is also a startling obstreperous sort of cough, shattering one's ears almost, like the noise of a person coughing through a brass trumpet—which depends upon some peculiar state of the nervous system, implies no danger, and is more distressing to the bystanders than to the person who utters it. I believe you may often distinguish the cough of inflammation of the lungs from that of phthisis, and each from the cough of hysteria, by their respective sounds: but we have much better methods of distinguishing them—viz., by the concurrence or the absence of certain other sounds belonging to the breathing, and ascertained by auscultation.

Sneezing is another morbid symptom, which, though it may appear trifling, is not to be overlooked. It is a very common symptom in catarrhal affections. When sneezing occurs in combination with cough, it affords a *presumption* that the cough is not phthisical. Sneezing may even happen as a primary disorder, occurring in long-continued paroxysms. I have at present under my care a young lady of a hysterical disposition, whose main distress consists in violent and protracted attacks of sternutation, which have harassed her almost daily for many months. One of our bishops is subject to very inconvenient fits of this kind. He will begin to sneeze, and go on sneezing incessantly for a long time together. I believe that he finds an effectual remedy for these attacks in plunging his head into cold water.

Sneezings, coughs, paroxysms of asthma, are all of them instances of *reflex* action. What is meant by that epithet I shall tell you in a future lecture.

I say nothing here of those direct symptoms of pulmonary disease which are ascertained by the sense of hearing—by auscultation and percussion. I shall enter fully into that subject hereafter. A systematic account of symptoms, if this were the fitting place for it, which it is not, would require a dozen or twenty lectures. In order to perceive the relation of symptoms, taken one by one, or in diverse combinations, to the various known forms of disease, you must have some prior knowledge of diseases. But I am obliged to suppose (however incorrect the supposition may be in respect to some among you) that you are mere beginners, and have still to learn even the rudiments of such knowledge. Different diseases may have many symptoms in common. The same symptom may bear a very different import according as it is combined with other symptoms; or connected with this or that disorder. The proper place for a comprehensive and complete review of symptoms would, therefore, be at the *end* of a course of lectures on the practice of physic. When the various forms of disease had been gone through, in reference to the symptoms belonging to them, then would be the time to take the converse aspect of the case, and to consider the long list of symptoms in reference to the diseases they denote or accompany. All that I am at present attempting, is to give you some general notion of what symptoms are; to put before you, as samples, a few of the most prominent; and to show you, even by this cursory and imperfect view of them, of how great importance it is that we should make their relations to each other and to different diseases, and their signification, diagnostic, prognostic, and therapeutic, the objects of our most diligent attention.

I might find matter for two or three lectures, if my present purpose would admit of them, in the symptoms that are drawn from the functions belonging to the *circulation*. Everybody knows how much importance is attributed to the state of the arterial pulse. It is expected of us, as a matter of course, that before we think of prescribing for a patient we should at any rate feel his pulse. And really the information obtained by that little touch of the wrist is often of the most interesting and instructive kind. But it requires practice and intelligence to appreciate this information.

The qualities that we most attend to in the pulse are its frequency, its regularity, its fulness, and its force. It is necessary that we should know the number of beats which the heart habitually makes in health; for it varies much in different persons. Its average number of pulsations in a healthy adult is from 70 to 75 in a minute.

but there are persons who, when they are quite well, have always a pulse of 80 or 90; and there are others in whom the pulse seldom rises above 60. In early life the pulse is more frequent, in old age it is more slow, than the standard I have given. *Cæteris paribus*, its beats are more numerous in the standing than in the sitting posture; in the sitting than in the recumbent. If we do not inform ourselves of these peculiarities, we may fall into great mistakes. In disease the pulse may acquire a degree of frequency which is scarcely calculable by the touch; and the less so because, when it is extremely frequent, it is also extremely feeble; it will reach 150, 160, or even 200 beats. I have myself reckoned, by aid of the stethoscope, 216 pulsations of the heart in a minute. On the other hand, in apoplexy sometimes, or when syncope is impending, or in certain organic affections of the heart, the pulse may become extremely slow. I do not remember to have counted a pulse slower than 22; but the gentleman whose pulse it was—a most exact observer of its changes—had numbered it when no more than 16. In another patient of mine, the surgeon who was also in attendance reported the pulse to have been, upon one occasion, 12 only; and I have been informed that in an old gentleman, whose spinal column had sustained some injury, and who was Dr. Chambers's patient, the beats of the pulse were for some time as few as 9 in the minute. I suspect that in some of these instances of very slow pulse, the stethoscope might detect, at the heart, intermediate weaker beats, too weak to stir the radial artery. But I satisfied myself that it was not so in the case in which I found the pulsations to be 22. We learn a good deal in certain disorders from the *variations* and *fluctuations* of the pulse in respect to frequency—in hydrocephalus, for example, and in continued fevers.

Irregularity of the pulse is another condition which is often full of meaning, and of interest. I hope, as the lectures proceed, to be able to point out the bearings of these several qualities of the pulse upon our views of disease, and especially upon its treatment. At present I must repeat that I pretend to do no more than furnish you with a few samples of the phenomena that characterize disease. Irregularity of the pulse is natural to some persons. I have a brother who enjoys very good health, and whose pulse is habitually irregular: I have been told that when he was ill with a fever at school, it became regular. I have heard of several precisely similar cases. There are two varieties of irregular pulse—in one the motions of the artery are unequal in number and force, a few beats being from time to time more rapid and feeble than the rest: in the other variety a pulsation is from time to time entirely left out—the pulse is said to intermit. These two varieties may coincide in the same person, or they may exist independently of each other.

Irregularity of the pulse may be caused by disease within the head; by organic disease of the heart; by simple disorder of the stomach; or it may be merely the result of debility, and the prelude to the complete stoppage of the heart's action from asthenia. How important must it be to ascertain and construe each of these meanings of the same symptom. It may indicate mortal disease—it may imply no danger at all: it may afford no clue to any available treatment; or it may teach us how to ward off impending dissolution.

Another most important quality of the pulse is what is called its hardness, or incompressibility. You find that you can scarcely abolish the pulsation by any degree of pressure; the blood still forces its way through the artery beneath your finger. Sometimes it is felt to strike a large portion also of the finger, and then we say that the pulse is full, or large, as well as hard. When it strikes a very narrow portion of the surface of the finger, it is compared to a thread; it is a small pulse: and if at the same time it be hard, such a pulse is often described as a wiry pulse. It requires some education of the finger to appreciate with exactness the several varieties of the pulse, even those which are practically important; for many have been mentioned by authors which are purely fanciful, or useless and unnecessary refinements.

Now this hard pulse I shall soon have to speak of again, in connexion with the treatment pursued in inflammation. It is one of the best warrants we have, in many cases, of the propriety of bleeding our patient. It does not occur, however, in *all* inflammations, and it may occur when there is *no* inflammation. It may depend upon hypertrophy of the left ventricle of the heart, and then it is beyond the reach of blood-letting as a remedy. It often seems to be connected with a morbid condition of the artery itself, brought on, as Dr. Latham has suggested, by the pernicious habit

of dram-drinking. It is, however, at all times considered so much a guide to our practice, that whenever it occurs, it is very necessary to make careful inquiry into its real cause.

Before I conclude this rough review of symptoms, I must point out one or two that belong to the third class mentioned; viz., changes of sensible qualities. These include variations in the temperature of the body: in the colour of the surface, and especially of the face: the diminution or increase of bulk; the latter, when general, we call corpulence; when partial, swelling: and various other symptoms, especially those which are detected by auscultation.

Wasting, or emaciation, is sometimes the first observable symptom of disease. This is early seen in the countenance, partly because it is uncovered, partly because a slight diminution of the subcutaneous fat of the face produces a striking alteration in its features. It occurs in complaints that are not commonly dangerous—as in dyspepsia, and in hypochondriasis, which is often connected with dyspepsia: and when it does appear it marks the reality of the disease. This wasting happens also in many fatal maladies—in phthisis pulmonalis, for example—and in dropsy, although the dropsical enlargement sometimes masks it. It accompanies many acute diseases, and is reckoned an unfavourable symptom; for it shows that the body is not properly nourished. Sometimes the emaciation is so extreme that the integuments give way—the bones of the patient are said to come through his skin.

We have examples of symptoms that consist in changes of colour, in the flushed face of fever; in the pallor belonging to many diseases; in the contrast exhibited by the white cheek with its central red spot, so characteristic of hectic fever; in the yellowness of the skin and conjunctiva in jaundice; in the dusky hue of the countenance and the lividity of the lips noticeable whenever the due arterialization of the blood in the lungs is interfered with; and in a long catalogue of cutaneous disorders.

Various and full of meaning are the conditions and appearances presented by the tongue. A patient would think you careless, or ignorant of your craft, if you did not, at every visit, look at his tongue, as well as feel his pulse. Again, symptoms most significant and instructive may often be gathered from the attitude of a patient, from his gestures and gait, from the cast and play and expression of his features.

Let me once more remind you of the peculiar importance of accustoming yourselves to take notice of the symptoms comprised in the last two classes, and especially in the last class, that you may attain to a quick perception of them. Changes of sensible qualities speak for themselves, and speak the truth. They cannot deceive us, as the verbal statements of even conscientious patients respecting their uneasy feelings might. They direct us in the choice and order of our inquiries: nay, they frequently spare us the necessity of putting many questions; questions that might be irksome or fatiguing to our patients, or offensive to their natural delicacy, or even hurtful by letting them know our thoughts respecting their disorders. Of the changes in sensible qualities we judge by our own eyes, and ears, and fingers, and often by our noses also; and the change is sometimes, of itself, perfectly characteristic of the complaint.

Many more morbid phenomena, or symptoms, or tokens of disease, might have been mentioned; but I have said enough, I hope, to rouse your attention to the extent and the fertility of this field of study. When we next meet I shall begin to consider one of the special forms of disease to which all parts of the body are liable—a disease that meets us at every turn—I mean *inflammation*.

LECTURE IX.

Inflammation. Its Morbid and its Salutary Effects. Sketch of the Local and Constitutional Phenomena of Inflammation as it occurs in External Parts. Examination of the Symptoms of Inflammation: Pain; Heat; Redness; Swelling. State of the Capillary Blood-vessels and of the Blood in a part inflamed.

INFLAMMATION is a subject which must needs engage a large share of the attention of both the surgeon and the physician. In nine cases out of ten the first question which either of them asks himself upon being summoned to a patient is, "Have I to deal with inflammation here?" It is continually the object of his treatment and watchful care. All parts that are furnished with blood-vessels are liable to be affected by it, and it affects different parts very variously. Being more easily excited by many external causes, it is, therefore, more *common* also than any other special disease. A great majority of all the disorders to which the human frame is liable begin with inflammation, or end in inflammation, or are accompanied by inflammation during some part of their course, or resemble inflammation in their symptoms. Most of the organic changes of different parts of the body recognise inflammation as their cause, or lead to it as their effect. The premature extinction of human life is more often owing to the agency, immediate or remote, of that process which we call inflammation, than to any other morbid condition whatsoever.

Again, inflammation is highly interesting, not only in its morbid phenomena and destructive consequences, but in its healing relations also. Wounds may, no doubt, be closed, and fractured bones may reunite without (and even sooner without than with) the intervention of any inflammatory process or product; but it is only under rare and very favourable circumstances that such injuries are actually so cured. In almost every instance, whether of a cut finger, of a deep sabre wound, or of a broken limb, it is the resulting inflammation which requires our care, and which furnishes the materials and the conditions of repair. It is through inflammation that parts adhere together when their adhesion is essential to the preservation of the individual—and that foreign or hurtful matters are conveyed safely out of the body. Does ulceration occur in the stomach or intestines, and threaten to penetrate through them? Inflammation will often forerun and provide against the evil—glue the endangered membrane to whatever surface may be next it—and so prevent that worse and universal inflammation of the peritoneum, and the almost certain death, which the escape of the contents of the alimentary canal into that serous bag would infallibly occasion.

[We doubt very much the correctness of this statement. Is it true, that wounds are closed and fractures united by inflammation, in the proper acceptance of the term? Union of divided surfaces, it is true, may be a result of inflammation; but, that which has been termed by surgeons "union by the first intention," takes place altogether independent of inflammation, and that the occurrence of the latter is calculated to impede or prevent it, must be evident, we think, to any one who will carefully watch its phenomena. Neither is it, we conceive, correct to describe the adhesions of the peritoneum which often precede the perforation, by an ulcer, of the coats of the stomach or intestines, and prevent the escape of the contents of the latter organs into the cavity of the abdomen, as a *provision* to guard against the universal and fatal peritonitis that would otherwise occur. The inflammation by which the adhesions alluded to are produced, is as strictly a morbid process as that which caused the ulceration of the alimentary canal, and cannot, with any propriety, be viewed as a process set up, in anticipation of the escape of the contents of the alimentary canal into the peritoneal sac, and to guard against the danger that would hence result. The danger is, we admit, occasionally lessened or even entirely prevented by the adhesions, but we have no right to infer from this circumstance, that Nature, foreseeing this danger, caused the adhesions, expressly to provide against its occurrence. — C.]

The foot *mortifies*; is killed by injury or by exposure to cold:—inflammation, if it be not anticipated by the knife of the surgeon, will cut off the dead and useless part. An abscess forms in the liver—or a large calculus concretes in the gall-bladder: how is the pus or the stone to be got rid of? If they make their way to the external surface of the organ, as they always tend to do, they might enter the cavity of the abdomen, and excite fatal peritonitis. But a natural safeguard arises; partial inflammation precedes and prepares for the expulsion; the liver or the gall-bladder, as the case may be, becomes adherent to the walls of the abdomen on the one hand, or to the intestinal canal on the other; and then the surgeon may plunge his lancet into the collection of pus—or the abscess or the calculus may eat its own way safely out of the body—through the skin, or into the bowel. Inflammation, limited in extent and moderate in degree, becomes conservative by preventing inflammation more severe and more widely spread, which would be fatal. This is what I mean when I speak of the curative properties of inflammation; and surely this process, which may save life or destroy it, deserves and demands our most careful study.

But inflammation has a still further and peculiar claim upon our attention. The salutary acts of restoration and prevention just adverted to, are such as *nature* conducts and originates. But we are ourselves able, in many instances, to direct and control the effects of inflammation—nay, we can produce it at our pleasure; and having produced it, we are able, in a great degree, to regulate its course. And for this reason it becomes in skilful hands an *instrument of cure*. This instrument the surgeon employs when, after letting out the water of a hydrocele, he wilfully excites inflammation of the tunica vaginalis, whereby its cavity is obliterated, and the reaccumulation of the fluid rendered impossible. A fractured femur fails to reunite, or its fragments become connected by pliant and unserviceable callus. The surgeon, using a happy violence, provokes inflammation in the fractured part, and a firm and bony union gives back to the limb its former strength and usefulness. It is by availing himself of the same agent that he is enabled to remedy many afflicting deformities:—to unite the cleft lip; to knit together the fissured palate; to restore the dilapidated nose. There is no other special disease which is thus at our command; we cannot, if we would, create a tubercle or a cancer. For all these reasons inflammation possesses a very high degree of interest for us—and for every one who would inquire, with any prospect of success, into either the pathology or the treatment of diseases.

Of the amount of our knowledge respecting the *intimate nature* of inflammation, I shall have occasion to say a few words by and by. We first become acquainted with inflammation in its symptoms, and as it displays itself externally. After we know what they are, it may be right, and it cannot but be interesting, to inquire how they come about. Now the symptoms which, when they exist together in an external or visible part, betoken inflammation of that part, are four in number: pain—redness—heat—swelling; *preternatural* redness and *preternatural* heat. These, from the earliest ages, have been recognised as the signals of outward inflammation. “*Notæ inflammationis* (says Celsus) *sunt quatuor—rubor et tumor cum calore et dolore.*”

No definition, however, or general description, can be made to embrace all the forms in which inflammation presents itself. No useful account of it can be given in the abstract; and therefore I shall first sketch the phenomena of inflammation under one of its most common and simplest external forms; and taking this as a type of the disease, proceed afterwards to trace its modifications and varieties, and to fill up the picture.

Let us suppose, then, that a healthy man receives some local mechanical injury—that he falls, for instance, against a window, and gets a fragment of glass embedded in the flesh of his arm. In a short time he begins to have pain in that part of the arm, and this is soon succeeded by redness, and increased heat, and swelling. The skin becomes of a bright red colour, the swelling augments. In the immediate place of the injury the swelling is firm and hard, and exquisitely tender: at some distance from that centre, although there is still swelling, the parts are softer and more yielding. In the seat of the redness and swelling the patient experiences a sense of heat,

a burning pain; the part is sensibly hotter than natural to the touch of a bystander; and if its actual temperature be measured by means of a thermometer, it will be found to exceed the temperature of the neighbouring surface. *The part is inflamed.* This is what is called phlegmonous inflammation. *Φλεγμονή* is a Greek word, and *inflammatio* is a Latin word, and they both mean the same thing, viz., a burning, or a flame. Phlegmonous inflammation is therefore, in truth, a tautological phrase. But custom has assigned a particular signification to the epithet phlegmonous;—it denotes that kind of violent inflammation in which the affected part seems all on fire; and chemistry teaches that, philosophically speaking, there is actual and excessive *combustion* going on in that part.

If the inflammation reach a certain degree of extent and of intensity, other signs of disorder present themselves at a distance from the injured spot. The patient usually at first feels chilly and feeble; but soon the temperature of the whole of the surface rises, the skin becomes hot and dry, the pulse more frequent and fuller and harder than is natural; lassitude comes on, with headache, and wandering pains in the limbs. The sufferer is unable or unwilling to exert himself, and finds that he is unapt for any mental effort; he cannot command his attention, gets confused and restless, and sleeps ill; he loses his appetite, his tongue becomes white, his mouth is parched, he is unusually thirsty, and the various secretions of the body are deranged and diminished.

This is *inflammatory fever*. This is an *indirect symptom* of inflammation, manifesting itself *through the medium of the system at large*. Various names have been given to this general derangement of the vascular and nervous systems: constitutional disturbance—sympathetic fever—symptomatic fever. It matters little what term is used, provided that we affix always the same meaning to it: but inasmuch as the word *fever*, in this and in other languages, is taken to express a specific disease, it would perhaps be better to employ the term *pyrexia*, as Cullen and others have done, to denote that secondary febrile state which is associated with, or grows out of, primary local inflammation.

Now, what is the issue of this remarkable state of things? Why, it may end in one of two or three different ways. Supposing the piece of glass to have been extracted, and proper measures to have been taken for subduing the inflammation, or even supposing that no other measure has been adopted except removing the bit of glass, then it will often happen that the phenomena just described will gradually recede and disappear; the pain will abate, the redness fade, the swelling diminish, the heat decline, the pyrexia cease; until the part has at length regained its usual sensations and its natural appearance. When inflammation subsides in this way it is said to be *resolved*, to terminate by *resolution*: and this is its most favourable and desirable mode of terminating, whenever inflammation occurs as a morbid process.

But in many instances the inflammation does not thus subside. The irritant cause still remains in action—or the original intensity of the inflammation has been too great to admit of resolution—or the means proper to abate it have not been used—or have not succeeded. The symptoms already described continue, and are aggravated in degree; at length the swelling begins to assume a more projecting and pointed form, and the skin in its centre to look white: the central part of the swelling, formerly so hard, becomes softer: the pain is of a throbbing kind; a pulsative sensation, keeping time with the beats of the heart, is experienced in the part, and often a feeling occurs as if something had given way within it: at last (if art do not interpose) the cuticle breaks, and a yellow cream-like fluid is poured out, which we call pus; and upon its escape there generally ensues a considerable and speedy abatement of all the local symptoms of inflammation—of the pain, the heat, the redness, the tumour.

This is *suppuration*.

Meanwhile, especially if the suppuration be long continued, and the discharge of pus profuse, the character of the general febrile derangement undergoes a change. Slight but frequent shiverings, or feelings of chilliness, take place, and are followed by flushes of heat, which end in perspiration.

This is *hectic fever*.

If the injury have been still more serious, and the inflammation more intense, the part which it has invaded perishes by the violence of the disease: there is partial death. In that case the vivid red colour alters to a livid or purplish, or even a black,

or greenish-black hue, the tension of the part exists no longer, the cuticle is elevated by a sanious fluid, the pain ceases, the part is devoid of all sensation — is dead and putrid — and exhales a peculiar and offensive odour.

This is *mortification*.

When the injury has been extensive, a corresponding and characteristic change is again observable in the constitutional febrile disturbance. The patient grows more and more feeble; is apt to be delirious; has involuntary startings of the tendons of the voluntary muscles; his pulse is weak and very frequent; his tongue becomes dry, brown, tremulous; his lips are black with accumulated sordes; his countenance is shrunk, haggard, damp, and ghastly; his stools and his urine escape from him without his appearing to be conscious that they do so.

These are features which belong to an advanced stage of *typhus fever*.

Under more favourable circumstances the dead or mortified part, which is called a *slough*, separates from the living parts, and leaves a breach of surface. The separation is effected by a vital process which is denominated *ulceration*; but which I need not now describe. The cavity thus formed gradually fills up, and heals in a particular way.

There is one other circumstance, not to be omitted in this rough outline of the local and general phenomena and effects of inflammation. If during its progress blood be drawn from a vein, the blood exhibits, after standing and coagulating, the peculiar appearance known by the name of the *buffy coat*; i. e., on the surface of the coagulum, and to a certain depth in its substance, the coloured particles of the blood leave the fibrin, which is therefore seen of a yellowish hue, or buff colour.

Taking the preceding statement as a groundwork, let us look back upon it, and trace its particulars a little more in full. The four characteristic signs of inflammation being pain, heat, redness, and swelling, it will be useful to examine more closely each of these symptoms in its turn.

The *pain* varies much in different cases of inflammation, both in degree and in kind. It is differently felt, *cæteris paribus*, by different persons, according to their natural susceptibilities. It varies from very slight uneasiness to the utmost agony and torment. Parts which, when sound, are endowed with little or no capacity of sensation (as tendons, ligaments, cartilage, bone), become often exquisitely sensible under inflammation. The organs of sense are variously affected in this respect. Thus the specific sensibilities of the mouth and nose are blunted by inflammation — those of the eye and ear are often rendered painfully acute. There are great diversities also in the kinds of pain. Sometimes it is of a dull aching character, as in tooth-ache; sometimes it is a prickling, tingling, smarting sensation — this is the case in some forms of inflammation of the skin, as in erysipelas for example, and in herpes; sometimes it is sharp and piercing, as if the part were stabbed or cut with a knife — such is frequently the feeling in inflammation of the serous membranes, in pleurisy for instance; sometimes the pain is tensive or stretching; and sometimes there is scarcely any pain at all. This last chiefly happens in the mucous membranes and in the parenchymatous texture of organs. Very often the pain is a “bulking” or throbbing pain — every beat of the heart makes itself felt in the tender part. The pain of inflammation results, no doubt, from the implication of the nerves in the diseased process. The stretching of the vessels and textures adds to the pain. Everybody who has been plagued by boils (and few escape them) has had proof of this: the pain is most harassing a short time before the ripening little tumour gives way, or is laid open by means of a scalpel; but as soon as the distension is thus relieved, perfect ease and comfort ensue. It is the same in common earache. It is upon this principle, I believe, that the differences in regard to pain, which occur in different structures under inflammation, are partly to be explained. Speaking generally, there is more pain felt in external inflammations, and in the inflammation of serous membranes, than in inflammation of the substance of the viscera, or of the mucous membranes: and it has been conjectured that this may be because, in the latter cases, the parts affected have fewer nerves of common sensation. But I do not think this explanation satisfactory. If it were well founded we should not have such exquisite pain in some of the textures already mentioned, which appear to be furnished with very few nerves of common sensation, and scarcely feel at all in their healthy state: tendons, ligaments, and cartilages, I mean. I think it will be found

that most pain is felt in those parts which are least capable of yielding—in which the tension produced by the swelling, or the tendency to swell, is the greatest. The substance of the liver, spleen, and viscera generally, is soft and extensible—the mucous membranes are spongy in their texture, and often attached to the subjacent parts in loose folds, and they allow of an accumulation of blood within them without becoming much stretched, or very tense. The investing serous and fibrous membranes are more tightly applied, and much less capable of yielding: and their inflammation is usually attended with severe pain.

The pain that belongs to inflammation sometimes precedes any other apparent change. This is especially observable in respect to internal parts. Sometimes the pain is continued and uniform. Sometimes it is continued, but irregular in severity, having periods of great exasperation; sometimes again it is intermittent, and even periodic.

It is an unsettled question that has often been mooted, whether in inflammation, the state of the blood-vessels is determined by that of the nerves, or the reverse. Mere nervous pains are known sometimes to be followed by congestion of the part in which they are felt. Whatever may be the true state of this question of priority, it is certain that the disordered condition of the blood-vessels, when produced, greatly augments the sensibility of the part. We may suppose that this depends, partly on over distension and stretching of the vessels and fibres, partly on pressure made upon the nerves by the swelling.

It is important to remark of the pain belonging to inflammation, that it is usually *aggravated by pressure*: frequently it is not felt at all, except when pressure is somehow made upon the affected part—intentionally by the physician—or accidentally, from the movements or position of the patient. This is *tenderness*.

And this is a point which requires a little further notice. I say the aggravation of the pain by pressure is an important circumstance, for it continually helps us to distinguish pain that is inflammatory from pain that is not inflammatory. Thus pain of the abdomen may result from colic, or spasm—from a distension of the intestines by air, and a stretching of the textures and nerves belonging to them: and this sort of pain is mostly relievable by pressure; you will find patients lying upon their bellies across the back of a chair for the sake of obtaining ease: but if the pain proceed, as it may, from inflammation of the peritoneum—oh! then the gentlest pressure, even that of the superincumbent bed-clothes, causes intolerable torture. The *suddenness* with which the pressure is made—and its being made on a *part only* of the suffering organ—these circumstances have much to do with the augmentation of the pain; and it is curious, and instructive too, to know that *gradual* pressure, applied *uniformly* to the *whole* organ or part under inflammation, is sometimes so far from enhancing the pain, that it lessens or removes it. Dr. Elliotson puts a very good case in illustration of this. “If (he says) you have a blister upon the sole of the foot, or at the ball of the great toe, and you rest gradually upon the part, the pain becomes mitigated, till at last it seems to be almost entirely removed; but the moment you take off the pressure, and raise the foot from the ground, you feel the part begin to throb—to throb with violent pain.”

Now all this exemplifies what I said just now—that though a deranged condition of the nerves, marked by pain, may, for aught I know, first lead to the vascular fulness—yet that same fulness, and the distension which it implies, will greatly increase the pain. In fact, the expulsion of the superfluous blood by means of well-regulated pressure is made the foundation of certain proposed methods of cure. This has been lately recommended in hernia humoralis, or swelled testicle—what is now more scientifically called orchitis. It gives one a sort of horror even to think of pressure being made on the healthy testicle—much more when it is rendered preternaturally sensible by inflammation: yet, when properly managed, pressure is said (by Dr. Fricke, of Hamburgh, and others) not to increase the pain, but entirely to remove it, so that the patient can at once walk about the room; and the disease is thus ultimately cured. In the same way it has been proposed to cure erysipelas, and gout, and rheumatism. Without inquiring here into the general merits of this remedial expedient, I may remark that pressure, so employed as to benefit an inflamed part by supporting its strained and oppressed blood-vessels, must be steady, gentle, continued, and (above all) uniform pressure. All these conditions are strictly supplied in an apparatus

recently devised by Dr. Arnott; whose *air-press* promises to be scarcely less useful to suffering humanity than his earlier contribution to the comfort of the sick—the *water-bed*.

It is sometimes necessary to recollect, especially when the existence of internal inflammation is suspected, that all expression of the sense of pain, and probably all sensation of pain, may be prevented or abolished by the presence of stupor or *coma*. So also, if the nervous connexion between the inflamed part and the sensorium be cut off, no pain is felt. Limbs in a state of palsy are often (though not always) destitute of sensibility also; and inflammation readily occurs in them, but is accompanied with no pain.

That *mere* pain does not constitute inflammation, must, I think, be plain to you. Spasmodic contractions of the muscles, stretching and tension of the tissues, a particular state of the nerves, and other conditions which do not imply inflammation, may, nevertheless, be attended with severe pain.

Let us next consider the *heat*.

Of course, as I hinted before, this means *preternatural* heat: the temperature of the part exceeds that which belongs to it in health; but in truth, the heat is not in general so much increased as the sensations of the patient, or his heightened sensibility, would persuade him to believe; nor even so much as a bystander might suppose. The heat of inflammation does not rise above the maximum heat of the blood in the central parts of the body. The natural heat of the blood is about 98° or 100° , but in fevers and inflammatory diseases it has been known to reach 107° , and the maximum heat of the blood in fever is probably the limit of the temperature as it exists in inflamed parts. The surface of the body, in its natural state, is not quite so warm as the internal parts, and the extremities are generally less warm than the trunk; so that the contrast between an inflamed and a healthy part, in respect to heat, is greater in the extremities than on the trunk. Thus if a blister be placed upon the chest, the heat of the part inflamed by its application will not exceed that of the neighbouring healthy surface by more than a degree or two; while a blister applied upon the leg may occasion a difference of five or six degrees. John Hunter took great pains to ascertain the degree of heat produced in inflammation. He excited inflammation in the cavity of the thorax of a dog, and in the vagina and rectum of an ass, and he could not find that the temperature of the parts thus inflamed ever exceeded that of the blood at the centre of the circulation. He did not neglect the opportunities that came before him of making similar observations on the human body. He had occasion to tap a patient in St. George's Hospital for hydrocele. As soon as he had let the fluid out, he introduced a thermometer through the puncture made by the trocar, and placed it in contact with the testicle. He found the temperature to be 92° . He repeated this experiment the next day, when inflammation had set in, and then the thermometer rose to $98\frac{1}{2}^{\circ}$. So that here an increase of $6\frac{1}{2}^{\circ}$ had taken place in consequence of the inflammation; but even this, you see, did not go beyond the natural warmth of the blood.

The increase of heat depends upon the increased influx of arterial blood, and therefore, of oxygen, into the part. Animal heat appears to be derived, in all cases, from the mutual action that takes place between oxygen and the elements of the tissues, their carbon and hydrogen; the tissues themselves undergoing meanwhile perpetual changes, which, in the natural condition of the body, belong and are necessary to health. In a part that is inflamed this kind of combustion is, I say, excessive in amount; while unnatural-metamorphoses occur in the affected tissues. It is, however, a curious fact, a fact worth remembering, that the heat of inflammation does not transgress that of the blood in the central parts of the body.

Heat alone neither constitutes nor implies inflammation: for parts of the body may be made preternaturally hot by holding them before the fire, by friction, by exercise, while there is no inflammation.

I apprehend that increased heat is essential to inflammation, in some stage or other of its progress, although there are cases in which the augmented temperature is not perceived or appreciated. Sometimes the increase of heat is very slight, and may be easily overlooked, there being, nevertheless, unequivocal inflammation; redness and swelling, which go slowly into suppuration. The heat is often concealed from the observation of the physician or the surgeon, by the situation of the part affected, and

it escapes the notice of the sufferer, because the sensibility to heat is less generally diffused through the body than the susceptibility of common sensation. The heat of inflammation is usually less felt and less complained of by the patient than the pain. A vivid sensation of heat is pain.

The *redness* of inflammation must also be *preternatural* in degree, for many parts of the body are, by nature, and in health, more or less red. This phenomenon depends upon the greater quantity of blood contained in the vessels of the part, and sometimes also upon the extravasation of a portion of the colouring matter of the blood into the affected texture. There is more blood than usual in those vessels which naturally carry red blood, and that blood is fuller of red particles. Red blood enters too into vessels which in the healthy state are destined to receive and convey colourless fluids only, or which naturally admit *so few* of the red particles, that from their paucity, and the quickness of their motion, they cannot be seen. We are sure of this from what takes place in ophthalmia. At a later stage, when the process of inflammation has subsided and that of repair has begun, the redness may be kept up or increased by the formation of *new* vessels which admit the colouring particles of the blood in visible numbers.

That the vessels which naturally circulate red blood are actually distended and enlarged in inflammation, there can be no doubt. John Hunter (whose treatise on Inflammation is a mine in which all succeeding writers have dug) excited inflammation in one of the ears of a rabbit, and then killed the animal. He next injected the head and ears from the aorta, so that the fluid used, passing through both the carotids, was driven with equal force towards each ear. The arteries of the inflamed ear were enlarged one-third beyond their natural size, and arteries in it were injected which had no visible counterparts in the sound ear. That the apparent increase in the number of blood-vessels is often owing to the circumstance that red blood enters tubes which already existed, but which did not previously admit the colouring matter, or did not admit it in sufficient quantity to be visible, is evident from the *rapidity* with which the redness may be produced in many textures: in the eye, for example, it may be effected in a few seconds; and many of the vessels which become suddenly apparent are evidently *continuations* of the trunks that could be seen before.

There is much variety in the tint of the redness of inflammation, depending on the kind and degree of the inflammation, and on the nature of the part affected. Sometimes the redness is bright and vivid, as if the part were full of arterial blood: this generally happens in the acuter forms and the earlier stages of inflammation. Sometimes the redness is dark, or livid, or purplish; more as if the part were gorged with venous blood: this occurs in some of the chronic and sluggish forms of inflammation, and it is often the case when there is a tendency to gangrene. Sometimes the redness is distinctly circumscribed, or presents itself in patches; and sometimes it is diffused in a general blush over a large space.

The redness may, and often does, remain for some time after the inflammation has ceased.

Now, seeing that redness accompanies inflammation of the external parts, we presume that it exists also in internal inflammation: indeed, we may convince ourselves that it is so. If a portion of intestine be drawn out through a slit in the parietes of the belly of a dog, and suffered to remain exposed to the air, it will soon inflame; and inflaming, it grows red. We see also that internal parts are *left* red after death, which parts we have other reasons for knowing had been inflamed during life; and we infer that redness may have been present before death, although we should find none remaining when the corpse is examined. That when it has been owing to mere fullness of the natural blood-vessels, it may disappear with parting life we know, because the same thing happens externally, as in erysipelas and scarlet fever; but in such cases the inflammation has not gone to any great height.

It is proper to remark that as the absence of redness is no proof that there has not been inflammation, so its presence is no proof of the contrary. There are many kinds of redness, both within the body and on its surface, that have nothing to do with inflammation; yet some of these are very apt to be mistaken for traces of inflammation. I shall endeavour to instruct you how to avoid such a mistake, when we come to examine the morbid anatomy of particular forms of disease.

While inflammation actually exists, redness, of some shade or degree, is seldom absent, even though the other symptoms may be scarcely apparent.

Lastly, let us take a glance at the *swelling*. This also depends, in some degree, upon the distension of the blood-vessels; but no great amount of swelling can be attributed to this cause; and as much as does proceed from it occurs *early* in the disease. Some also, and usually almost the whole, of the swelling, results from the presence of matters poured out into the interstices of the affected part. These effused matters are of very different kinds, although they are all modifications of the same liquid, the blood. I mentioned, in describing the condition of the part inflamed, that the central portion of the swelling is, at first, hard and resisting, while at a greater distance from the centre the swelling is softer, and yields more readily when pressed by the point of the finger, and, sometimes, even pits a little under that pressure. Now the central hardness is to be ascribed to the exudation into the areolar texture of the part, of a fluid, which, transparent at first, soon becomes partially opaque and more consistent, and at last assumes a solid form. This is what is commonly called, in this country, coagulable lymph. The softer swelling at the circumference of the tumid part proceeds from the passage of a thinner fluid, of serum, into the areolar tissue. Under very violent inflammation, blood in substance may be poured out into the same parts. When the central portion of the swelling softens and becomes pointed, this part of the whole enlargement is owing to the presence of a quantity of pus. The different liquids that I have now been mentioning are of great importance, and play a conspicuous but diversified part in altering textures. Blood; serum; albuminous fluid or coagulable lymph; pus. They are called the *products* of inflammation. We are sure that inflammation has been at work if we meet with certain of these products. We are not sure there has been inflammation if we perceive mere redness:—we are not always sure if we find serum only:—we are not sure if we find blood alone:—we are tolerably certain if we discover pus; we are certain at least that there has been inflammation *somewhere*, though doubts have been started whether the pus is not sometimes conveyed from an inflamed part to other parts of the body. We are *quite* sure that there has been inflammation in a part if we find coagulable lymph in that part. This often remains, as a monument of the inflammation, during life; it frequently becomes organized, furnished with blood-vessels; and a great number of changes, some reparative, some morbid, depend upon its presence. I shall have to recur to these *products* of inflammation hereafter.

The degree of swelling in different cases depends partly on the intensity of the inflammation, partly on the nature and texture of the structures affected.

I need scarcely observe that swelling may exist without any inflammation. Hernia, simple anasarous enlargements, dislocations, will occur to you as every-day examples of swellings that have no necessary connexion with inflammation.

On the other hand, inflammation may exist without any *appreciable* swelling. Inflammation of the sclerotic coat of the eye, for instance, may be present, without any swelling cognizable by our senses.

We have seen, in this review of the symptoms of inflammation, how much they severally depend, the pain, the swelling, the redness, and the heat, upon the increased influx of blood into the part.

It may be not uninteresting to pause here for a moment to inquire what has been ascertained in respect to the actual condition of the capillaries of an inflamed part, and of the blood they contain. Much has been learned on these points by patient and minute observation with the microscope, and by reasoning upon the facts thus brought to light. Kaltenbrunner, Gendrin, Müller, Mr. Wharton Jones, and others, have corrected many erroneous notions which formerly prevailed upon this subject.

In order to comprehend the minute phenomena of inflammation, you must have a clear conception of the constituent elements of the blood, and of the main changes it is liable to undergo. The rough anatomy, rather than the chemistry of the blood, is what I allude to.

Recollect, then, that the blood consists of red particles, or globules, and of a transparent colourless fluid called liquor sanguinis. By some modern writers the liquor sanguinis is denominated the *plasma* of the blood. Müller succeeded in separating these two constituents of the blood by filtering through paper that of a frog, which contains very large red globules. The liquor sanguinis thus obtained separates spontaneously, by coagulation, into two parts, into serum and fibrin, the last having previously existed in solution in the liquor sanguinis.

When the coagulation is suffered to take place without any attempt to remove the red particles, these are entangled and enclosed in the fibrin as it becomes solid; and the common well-known appearance of clot and serum results. You may even then wash out the red particles from the clot, and leave the fibrin.

I must now recur to the experiments and observations of Kaltenbrunner. I should have told you, on a previous occasion, that various stimulant substances, mechanical or chemical, when applied to the web of a frog's foot, will produce irregular disturbances in the circulation, which irregular disturbances you are not to confound with true congestion: in like manner you must avoid confounding them with the phenomena of *inflammation*, which are always preceded by those of true congestion. Kaltenbrunner found likewise, that (just as in congestion) a certain interval of time generally happened between the application of the exciting cause and the apparent development of the inflammation. Probably the blood-vessels are *contracted* during this interval. So I judge from experiments more lately made and reported by Mr. Paget. This accords with what we observe to be the case in respect to local injuries, and to those local internal inflammations that are apt to be produced by exposure to cold. There is a pause before the mischief lights up: or (to take the metaphor from the eggs of birds) there is a period during which the inflammation seems to be hatching, and it is called accordingly the period of *incubation*. Kaltenbrunner describes inflammation to be a regular process—as he had also described congestion to be.

On looking then at the web, to which some violence had been done, he observed, after the first irregular disturbances were over, and when the period of incubation had elapsed—he found (I say) that an afflux of blood took place to the part about to be inflamed; the velocity of the blood in the vessels was greatly accelerated; the vessels themselves were distended and tense, and therefore disposed to tighten upon the blood they contained—the functions of the part, that is to say, the secretion and absorption of lymph, were interrupted; the blood underwent an evident change—or it failed to undergo the proper changes: its red globules were crowded together, and the parenchyma of the web became tumefied. Now all this is just what I represented to you in a former lecture as constituting the state of the blood-vessels under *active congestion*; and I also told you, at the same time, that such congestion was just one step short of inflammation. The congestion now described increases, until, at length, this remarkable alteration happens: the capillary tubes lose their apparent tension, dilate, grow larger; the circulation, at first so rapid through the dilated vessels, begins to be *delayed* in some of them; the direction of its motion becomes uncertain; it oscillates, as it were, irregularly in those vessels, and at last stops altogether, the globules cohering in irregular masses, and thus *points of stagnation* are formed; and these points of stagnation, if the affection go on increasing, augment in size, and multiply in number. Around them, beyond their circumference, the circulation remains still very rapid, and the congestion persists. This is *inflammation*. Here it is, I apprehend, that the line must be drawn; viz., when the blood, ceasing to move with an accelerated stream through the dilated vessels, begins to pass slowly and more slowly till it halts, oscillates, and finally becomes stagnant in some of the capillaries. At the same time the red corpuscles are very numerous and crowded. This, I say, is inflammation, of which the characteristic or pathognomonic feature is the formation of these *points of stagnation*, as a sequel of active congestion.

Mr. Paget thus describes the general state of matters in and around a part that is inflamed:—"In a few words, there is in the focus of severe inflammation, more or less of *stagnation* of blood; in and close around it there is *congestion*, i. e., fulness and slow movement of the blood; more distantly around there is *determination*, i. e., fulness and rapid movement of blood." You will observe that he applies the phrase determination of blood to that condition which I have called in these lectures, active congestion.

He also bespeaks attention to the curious and important fact, that the stagnant or retarded blood of a part which is inflamed is not apt to coagulate. He had found it fluid after at least three days of complete stagnation.

Now one early consequence of the stagnation of the blood, or of even its slow movement, is, that a portion of it transudes through the sides of the vessels containing it: the serum; or the liquor sanguinis; or the colouring matter; or even sometimes the blood itself breaks through, red corpuscles and all. The effused serum remains,

or is absorbed, as serum. The fibrin, when it has so transuded, concretes, and thus the interstices of tissues are filled up, and layers of coagulable or coagulated lymph are formed upon the surfaces of inflamed parts, constituting false membranes. Under certain circumstances, already adverted to, other or further changes take place. The yellow cream-like fluid called *pus* is formed; sometimes rapidly and in vast abundance, like a secretion. Pus streams, almost, from certain of the mucous membranes, under inflammation. It has this analogy with the blood, that it consists of corpuscles diffused through a clear liquid (*liquor puris*) which both in its sensible and its chemical qualities appears to be identical with serum. It was Gendrin's opinion, as I have heretofore been accustomed to state, that the yellow globules of pus were in reality transmuted blood globules. But the rapid advance of pathological science has disproved this notion. The microscope, perfected by modern skill, is daily adding to, and rectifying, our previous knowledge, respecting the rudimental processes which occur in the living body, both in health and in disease. By its help we have learned that all, or most, of the varied animal tissues are formed through the intervention of minute closed sacs, or cells, having delicate membranous walls. These cells are themselves developed from pre-existing granules, germs, nuclei, or cytoblasts; for such and so variously have they been named. Upon the completion of the cell, the nucleus sometimes disappears; but commonly it remains, a mere spot, upon the inner surface of the wall of its cell. Mr. Paget has shown the probability that this

nucleus—which is not always developed into a cell—is “the chief seat or source of formative, reproductive, and secretive power:” and that the cell, when it exists, “is not a transitional but a terminal form, not giving origin by further development to any other structure.” Now the red corpuscles of the blood are nucleated cells. Among them are also to be seen, floating in the *liquor sanguinis*, a small number of colourless corpuscles. Again, with the fibrinous matter poured out in inflammation, there are intermixed corpuscles, revealed by the microscope, called exudation cells. It seems probable that the nuclei of these last exude from the blood together with the fibrinous coagulable lymph. Whether they are identical, as some suppose, with the colourless globules of the blood; or, as others, with the nuclei of its red globules; or again, as others, with the corpuscles of the chyle;—these are questions which must be regarded as *adhuc sub judice*. The

pus globule is also a nucleated cell; and it seems to be one of the forms of which the exudation cell is precursory.

Fig. 6.



a. Natural appearance of pus-corpuscles. *b.* Appearance after application of acetic acid.

Fig. 7.



Pus-corpuscles, magnified 400 diameters.

Fig. 8.



Healthy pus-cells.

Certainly much which used to be thought mysterious in the process of inflammation has been rendered more simple and intelligible by modern research. Most of the events or consequences of that process are traceable to the stagnation of the blood in the capillaries, and to the changes which the stagnant blood subsequently undergoes, or originates.

I must not omit to tell you what Kaltenbrunner says about the direct absorption that takes place in the inflamed part. He found that the *colouring* matter, and the *adipous* matter, were thus taken away. The web of a frog's foot is speckled over with little stars of five rays, caused by a black pigment. The extremities of these rays gradually disappear until mere black points are left in the places of the stars. He says that he has been lucky enough to catch the exact moment when the blood,

circulating rapidly in the canals, has detached a particle from one of the rays, and carried it into the torrent of the circulation. In the sound state, the mesenteric vessels of the rabbit are surrounded with much fat. When the mesentery is inflamed, the adipous cells soon empty themselves: a number of capillary canals are developed upon the walls of those cells, and it is probable that the fat is carried off by the blood circulating in these canals.

FIG. 9.



An exact copy of a portion of the web in the foot of a young frog, after a drop of strong alcohol had been placed upon it. The view exhibits a deep-seated artery and vein, somewhat out of focus; the intermediate or capillary plexus running over them, and pigment-cells of various sizes scattered over the whole. On the left of the figure, the circulation is still active and natural. About the middle it is more slow, the column of blood is oscillating, and the corpuscles crowded together. On the right, congestion, followed by exudation, has taken place, constituting inflammatory action in the part.

a. A deep-seated vein, partially out of focus. The current of blood is of a deeper colour, and not so rapid as that in the artery. It is running in the opposite direction. The lymph-space on each side, filled with slightly yellowish blood-plasma, is very apparent, containing a number of colorless corpuscles, clinging to or slowly moving along the sides of the vessel.

b. A deep-seated artery, out of focus, the rapid current of blood allowing nothing to be perceived but a reddish-yellow broad streak, with lighter spaces at the sides.

Opposite *c*, laceration of a capillary vessel has produced an extravasation of blood, which resembles a brownish-red spot.

At *d*, congestion has occurred, and the blood-corpuscles are apparently merged into one semi-transparent, reddish mass, entirely filling the vessels. The spaces of the web, between the capillaries, are rendered thicker and less transparent, partly by the action of the alcohol, partly by the exudation. This latter entirely fills up the spaces, or only coats the vessel. — *Bennett.*

I should be making a very wasteful use of your time and of my own, if I entered into the undecided and unprofitable disputes that have been raised respecting the vital conditions of the vessels engaged in inflammation. While some have pretended that the action of the small vessels is increased, others assert that it is diminished; that the vessels are in a state of atony. For my own part, I have never yet seen any conclusive evidence that the capillaries possess any vital contractile power distinct from their elasticity. And granting them such a power, it is extremely difficult to conceive how any increase in their vital contraction should produce the changes that are observed in inflammation. Certainly we have no warrant that any such contraction takes place, in the results of microscopical examination of the vessels of an inflamed part. The inquiry might be more properly directed, I think, towards the vital conditions of the *nerves* of the part: but here we are wholly in the dark.

I do not think it so evident as some have supposed it to be, that a greater quantity of blood than is natural passes through an inflamed part in a given time. It is quite true—and it is proper that you should be aware of it—that the arterial trunks leading to an inflamed part often pulsate with more than ordinary force, and, if opened, project a jet of blood further than they would naturally project it. It is true, also, that a venous trunk leading from an inflamed part will discharge blood faster and

more copiously than a corresponding vein leading from a sound part. Mr. Lawrence declares that he has frequently tried this experiment, and always with similar results. Finding it necessary to bleed a patient whose hand and forearm were inflamed, he has directed a vein to be opened in both arms at the same moment; and he has ascertained that about three times more blood flowed, in a given time, from the vein of the inflamed limb than from that of the sound. But it scarcely follows from this that more blood circulates through the whole of the part actually inflamed: the activity of the circulation in the vessels that remain pervious, and are merely congested, around the focus of inflammation, is greatly increased, and more blood circulates through the *limb*: and yet the blood may be stagnant, or scarcely circulate at all, in the very part that is strictly and truly inflamed. However, the fact of this increased afflux of blood towards the parts concerned in the inflammatory process is an important one.

LECTURE X.

Inflammation continued. Buffy coat of the blood. Terminations or Events of Inflammation. Resolution; Delitescence; Metastasis. Effusion of Serum. Exudation of Coagulable Lymph, or Fibrin. Organization of this Lymph. Suppuration. Ulceration.

IN the last lecture, after giving a very general sketch of the phenomena of inflammation, I particularly considered its four characteristic symptoms—pain, heat, redness, and swelling: and endeavoured to describe the changes that take place in an inflamed part, as they are seen through a microscope.

There is one very remarkable and important circumstance which is not often absent in cases of inflammation, but which hitherto I have barely mentioned: I mean a peculiar appearance of the blood itself after it has been drawn from a vein. A portion of the fibrin at the upper surface of the coagulum parts with its colouring matter; so that upon the deep red clot there is to be seen a layer of a yellowish, or sometimes of a bluish white colour, varying in thickness from a line or two to perhaps three-fourths of an inch. This uppermost whitish layer of the coagulum is called in this country the *buffy coat* of the blood. Sometimes the surface of the buffy coat is flat and wide; but often it is contracted and concave; i. e., the diameter of the buffy surface is less than the diameter of the lower portion of the clot, and it is hollowed out into a cup-like form. Accordingly the blood is said, in these circumstances, to be both buffed and cupped. The formation of this buffy coat appears to be favoured by many circumstances which have nothing to do with the disease under which the person may be labouring; such as the size of the aperture in the vein, the manner in which the blood flows, the form and size of the vessel that receives it: but it does not occur at all except in certain conditions of the system; and it belongs so especially to the

FIG. 10.

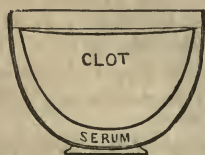


FIG. 11.

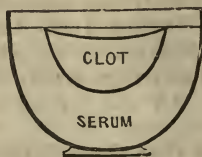


FIG. 12.



state of *inflammation*, that blood having the buffy coat upon it is often spoken of as *inflammatory blood*, or, with less propriety, as *inflamed blood*. Both these expressions indeed are incorrect, for inflammation sometimes exists without buffy blood; and buffy blood sometimes occurs without inflammation. The phenomenon is, how-

ever, upon the whole, a very valuable index of the nature of many cases of disease, and an important guide in their treatment.

Now this crust, or upper layer, or buffy coat, consists of pure fibrin, mixed with a certain quantity of serum, which M. Gendrin says is fuller of albumen than the rest of the serum. You will not fail to notice the great analogy that subsists between the buffy coat, and the coagulable lymph poured out in inflammation, either into the texture of the part, or (as I shall show you more particularly by and by) upon its surface, forming what are called false membranes. Both in appearance, and in chemical composition, the two seem to be identical: there is in fact no doubt of their being actually the same substance; the separation in the one case taking place from the blood while contained in its proper vessels; in the other case from the blood after it has been removed from the body.

There has been a great deal of speculation among pathologists as to the cause of this buffy coat. From its situation it is plain that gravity has something to do with its formation: that the red particles, leaving the colourless fibrin before it coagulates, sink downwards by their own weight. But though the *subsidence* of the red particles is occasioned by their greater specific gravity, their *separation from the fibrin* is not to be explained upon that principle alone. If it were, then it would follow that the slower the coagulation of the blood, the more time would there be for the sinking of the red particles, and the thicker and more decided would be the buffy crust. And it used to be supposed that this was the true explanation of the phenomenon. Careful observations, however, have shown that the formation of the buffy coat often takes place when the coagulation of the blood is unusually rapid. Dr. Davy and M. Gendrin both state, as the result of much attention to the subject, that the coagulation of blood drawn from a vein during inflammation begins sooner, and is more quickly completed than that of healthy blood. But certain observations made and published by Dr. Stokes have settled this question. He noted the appearance of the blood in twenty-seven cases. In fifteen of these the buffy coat presented itself; in twelve it did not. Now in three of these twelve, the coagulation of the blood did not begin till from twenty to forty minutes after it was drawn; and in four others there was no coagulation for eight minutes. So that there was plenty of time for the red particles to have left the fibrin, and subsided; but they did not do so. On the other hand, in twelve out of the fifteen cases in which the blood *was* buffed, the coagulation took place in five minutes; and in the remaining three it was delayed only fourteen minutes.

The slowness of the coagulation, therefore, although it may and doubtless does *favour* the subsidence of the red particles when they have a tendency to subside, cannot be regarded as the sole cause of the buffy coat. The red particles very soon begin to subside when they subside at all. You may tell, immediately after it has been drawn, and prior to any coagulation, that blood is about to buff, by a peculiar bluish hue on its surface. A German writer, Schroeder Van der Kolk, has stated observations to the same purpose, showing that in the blood abstracted by venesection during inflammation there is an unusual disposition to a separation of the fibrin from the red particles: a sort of *repulsion* between them. This separation takes place in mere films of blood, so thin as not to permit a buffy stratum to lie above a red stratum. The fibrin and the red particles then separate from each other laterally by horizontal movements, and the films acquire a speckled or mottled appearance, quite as characteristic of the state of the blood as the buffy coat itself.

One cause of the rapid subsidence of the red corpuscles in blood drawn during the presence of inflammation, is the tendency then especially manifested by these disk-like bodies, as noted by Hewson, Mr. Wharton Jones, and others, to cohere permanently together, by their flat surfaces, in little cylindrical columns, like rolls of coin. Thus aggregated into masses they would sink more readily through the liquid plasma of the blood than as separate corpuscles. Of this curious tendency no satisfactory explanation has yet been given.

While the clustered corpuscles thus descend, the thickness of the buffy coat is increased by the white corpuscles, which remain separate, and having a less specific gravity, float upwards.

That the formation of the buffy coat depends upon some *vital* change in the blood appears probable from this—that it will sometimes vary greatly in different portions of blood abstracted at the same bleeding. Thus, if the blood be received into four

different cups in succession, it will, perhaps, be buffy in the first, and in none of the others; or it will be buffy in the last only; or in the second and third only, the first and fourth cups being free from buff. Attempts have been made to explain these rapid variations. Some have fancied that the inflammatory state having been partly redressed by the removal of a certain quantity of blood, the blood that flows subsequently is *therefore* without the usual index of the presence of inflammation. But this explanation will not apply at all to those cases in which the portions last drawn are the only portions that exhibit the buffy crust. Others have suggested that the state of the nervous system is principally concerned in these sudden changes; that the depression caused in the outset of the bleeding by fear, and the faintishness produced towards its termination by the loss of blood, may prevent the appearance of the buffy coat on the first and last cups, when it shows itself only in those that are intermediate between the first and last. We cannot rely much on these hypothetical explanations: I mention them to impress upon your memories the facts which they are intended to explain.

[In nearly all the strongly developed acute inflammations, there is an excess of fibrin and of the colourless or lymph globules of the blood. From three parts in a thousand, which, according to Andral, is the average proportion in health, the fibrin has been found to rise to six or eight parts. In some cases, MM. Andral and Gavarret found it as high as ten parts in the thousand; namely, in pneumonia and acute articular rheumatism. The excess of fibrin was noticed by Andral in cellular inflammation, or simple phlegmon, in acute inflammations of the skin, as in burns and erysipelas, in mercurial stomatitis, in phlegmasiæ of the mucous membrane of the respiratory and digestive organs, in acute cystitis, either simple or combined with nephritis, in all of the phlegmasiæ of the serous membranes, in inflammation of the lymphatic glands, and in softening of the brain. But it does not follow the pustules of small pox, the exantheme of measles or scarlatina, or the patches in dothineritis.

The increase of fibrin in the blood is manifested so soon as the inflammation begins. M. Andral is inclined to believe, from what he has remarked in cases of burns, that the disease commences in the solids before the change occurs in the blood. The sympathetic fever in the phlegmasiæ is, he thinks, due to the alteration in the blood from the excess of its fibrin. With the increase of fibrin, the proportion of red particles remains unaffected. Even in anemic cases, and in low fevers, in which there is a diminution in the proportion of fibrin, the latter becomes abnormally increased whenever inflammation supervenes.

To this it may be objected, remarks Williams, that this fever frequently rises high before the blood has begun to exhibit the buffy coat, often subsides when the buffy coat is most abundant, and is sometimes wholly absent when the blood is both buffy and cupped, as in sub-acute rheumatism. But he admits that it is very probable the excess of fibrin may contribute to the excitement, and it certainly materially affects the duration and products of inflammation.

It would appear very certain that the formation of the buffy coat in inflammatory diseases is in a great degree dependent upon this excess of fibrin. It is found only in cases where the proportion of fibrin is abnormally augmented. In anæmia, the only disease excepting inflammation, in which the buffy coat is observed, notwithstanding the quantity of fibrin may be abnormally small, still it is very generally in excess in relation to the red globules. The buffy coat forms, also, upon the blood drawn during the latter period of pregnancy; when the blood also presents an excess of fibrin. In inflammation the buffy coat is frequently not present in the blood drawn in the early period of the disease, the fibrin not being as yet sufficiently abundant, while it becomes less and less marked as the intensity of the disease and the excess of fibrin diminish.

In cases of indirect prostration, incident upon great intensity of inflammation, the blood first drawn may be without the buffy coat; it becoming apparent only as the pulse and heat of the surface rise, as they often do in such cases, under the use of the lancet.

According to Andral, the newly-formed fibrin in inflammation coagulates more slowly than the old.—See Andral, *Patholog. Hæmatol.*; and Williams, *Principles of Medicine*.—C.]

There are two or three different forms presented by buffy blood; and with these you ought to be familiar.

In one form the buffy coat is thick, tough, contracted, puckered at its circumference, and its surface is cupped. There is a complete separation of the red corpuscles, and a strong aggregation of the particles of the fibrin among themselves. The red portion of the coagulum is also, in these cases, round and contracted, of a globular shape, firm, detached from the sides of the vessel, and floating generally in transparent serum.

This is usually seen when the inflammation is violent; when it occurs in strong and vigorous constitutions; and more I believe when it has its seat in certain tissues, in fibrous and serous parts especially.

In another form, the whole coagulum is large like a cake, or of the figure of the vessel containing it, not so much collected into a spherical shape; and the buffy coat is thin and flat, and easily broken. Here there is an imperfect separation of the colouring matter from the fibrin, and no great aggregation of the particles of the latter. This kind of buffy blood is apt to accompany slight or partial inflammation.

In a third form, the buffy coat is thick and abundant, but it is flat and soft, loose and flabby, like paste; and the coagulum often adheres by its edges to the vessel in which the blood has been received: there has been considerable separation, and but slight aggregation. The serum is apt to contain a few red particles distinct from the clot. Such blood is commonly said to be *szy*. Dr. Alison states that when it is observed, some other cause of general disorder of the system (continued fever, for example) usually co-exists with the local inflammation. Probably the qualities of the blood are altered, independently of the inflammation; the buffy coat taking place, in consequence of the inflammation, upon blood which was previously in a diseased or unnatural condition.

It is a curious fact that blood drawn by leeches never exhibits the buffy coat. It seldom appears (yet I have seen it) upon blood that has been removed by means of cupping glasses.

Arterial blood is liable to the buffy coat. I have myself twice at least noticed it upon blood drawn from the temporal artery. One of the patients was violently maniacal when the blood was taken. The other was labouring under acute inflammation of the membranes of the spinal cord, of which inflammation he died.

Blood is more likely to buff, *cæteris paribus*, when it is made to flow in a full stream, and when it is received in a deep and narrow vessel.

On the other hand, the formation of the buffy coat appears to be hindered, when the blood trickles from a small opening in the vein, and when it is caught in a large flat vessel. It is said, also, that the buffy coat may be prevented by adding to the blood a solution of caustic potass; by keeping it for some time in a state of agitation; by receiving it in a very cold vessel; or by suffering it to fall from a height of three or four feet. In this last case M. Gendrin supposes that the prevention is owing partly to the agitation which the descent of the stream produces in the blood already in the vessel, and partly to the circumstance that the blood is cooled as it passes through the air.

It is seldom, I believe, that the buffy coat appears on blood drawn at the very outset of inflammation; generally a day or two elapses before it shows itself. This is just what we might expect if it be true, as it has been conjectured, that this unnatural property of the blood is acquired only in the course of its transit through the vessels of, or adjacent to, the inflamed part.

I have dwelt the longer upon this peculiar appearance of the blood, because it really is of very great importance in determining the nature of various complaints, and in directing our treatment of them. Speaking generally, when a given organ is inflamed, the buffy coat is more marked in proportion to the intensity of the inflammation: when the organ is not known, it is more likely to be of a fibrous or a serous texture, in proportion as the blood is more decidedly buffed. The appearance of the buffy coat is especially valuable as an indication of treatment in cases concerning which we are in doubt whether they are inflammatory or not. On the other hand, if we have good evidence, in other symptoms, of the existence of inflammation, we are not to be shaken in our opinion by the absence of the buffy coat. Inflammation may certainly exist without it. I am not speaking now of slight cases of inflammation,

which do not disturb the general system. You would not look for buffy blood in the inflammation that supervenes on a cut finger, or in a small boil; but in serious inflammation, attended with pyrexia, the buffy coat may be wanting. It is not unfrequently absent in inflammation of the mucous membranes, especially in inflammation of the mucous lining of the bronchi.

I stated before — what it is quite necessary to remember — that buffy blood is not confined to cases of inflammation. The blood of persons affected with general plethora is often found to present a buffy coat: and the same thing is true in respect to pregnant women.

Buffy blood is no necessary measure of the *danger* of the disease. The blood drawn in acute rheumatism is always very much buffed and cupped: yet so long as the disease is confined to the joints, it is quite free from danger.

Neither is the appearance of buff on the blood, taken by itself, a sufficient warrant for abstracting more blood: for the blood will sometimes, in common inflammation, continue to be buffy, long after it has ceased to be useful, or safe, to bleed the patient. Nay, it is even affirmed, by trustworthy writers, that in pure anæmia the crassamentum, although small from the paucity of red particles, presents not unfrequently the same appearance. This statement I can neither confirm nor confute by my own experience; for we do not bleed anæmic patients unless they are believed to labour under some local inflammation.

Another less obvious, but not less important change, which has been established by Andral to be an invariable accompaniment of acute inflammation, is a remarkable increase in the *fibrin* of the blood. The augmentation begins with the inflammatory process, increases with its increasing intensity, and diminishes as it abates. That it depends upon the inflammation, and not upon the general febrile condition consequent upon the inflammation, is apparent from the interesting fact, that in idiopathic fevers the proportion of fibrin in the blood decreases.

Some pathologists enumerate several *terminations* of inflammation. Others quarrel with that word, as inappropriate: alleging, with great truth, that the inflammation does not necessarily cease or terminate whenever these so-called “terminations” happen. Some of them are in fact “co-existent states, or successive stages in the progress of the same inflammatory disease.” It has been proposed to speak rather of the *local effects* of inflammation: but even this phrase is not free from objection, for sometimes (though rarely) there are no local effects produced, beyond the four symptoms which characterize the inflammation itself. I think the *events of inflammation* is an expression not open to similar cavils. I have no ambition to introduce new modes of speech, unless when those already in use are inexact or inapplicable. It is enough if you clearly comprehend the meaning of the terms I employ. Among the events of inflammation I include only the *local changes* observed in its course. To those which are constitutional I must afterwards revert.

You will recollect that I did allude, in the last lecture, though in a very brief manner, to these local events of inflammation. Their frequency and importance render it necessary that we should consider them somewhat more particularly.

One of these events is the simple subsidence or *resolution* of the inflammation: this may strictly be called a *termination* also. The congestion of the blood-vessels increases till the blood stagnates in some of the capillary canals towards the centre of the affected part, which is then said to be *inflamed*; but the disease goes no further; there is no escape of the blood, nor of any part of the blood, nor of any of the constituents of the blood, beyond its natural channels; or, at any rate, there is no *sensible* evacuation into the inflamed tissue, or next to none. The inflammation begins to recede; the stagnant but still fluid blood is again set in motion; if there have been some slight effusion, it is re-absorbed; the rapidity of the circulation in the surrounding vessels diminishes; and the part returns, in all respects, to its former condition and integrity. This may be considered the spontaneous cure of inflammation; and to this event there seems to be always a natural tendency. It may be promoted, sometimes, by art.

When the process of resolution is unusually *sudden* and *rapid* (as it occasionally is, the well-marked phenomena of inflammation completely disappearing in a few hours), it is called by our neighbours the French, *delitescence*. And when the symp-

toms of inflammation thus suddenly desert one part, and show themselves immediately afterwards in another (as not unfrequently happens in respect to the joints in acute rheumatism, and between the parotid gland and the testicle or mamma in the mumps), *metastasis* is said to take place.

This transference, as it were, of morbid action, from one part to another, is a very curious circumstance. It is one which we sometimes endeavour to imitate. We excite inflammation upon the surface, where we know its effects will be of comparatively little consequence, in the hope of *diverting* it from some internal organ in which it threatens to work serious or even fatal changes. We follow the same principle perhaps when we apply purgative medicines to the mucous membrane of the alimentary canal. To denote this mode of cure, by stimulating distant parts, the terms *counter-irritation*, *derivation*, and *revulsion*, are employed.

Most commonly, even under moderate inflammation, *some* amount of exudation or effusion takes place into the texture or from the surface of the part.

The first effect or event of that kind which we notice, is the pouring out or *effusion* of *serous* liquid. The liquid is so like the serum of the blood, that it is called serosity or serous liquid. In most cases, however, it is something more than this; it is in reality the liquor sanguinis, serum holding more or less fibrin in solution. Mr. Paget teaches that an effusion of serum alone is an uncommon effect of inflammation, and that generally it is characteristic of only the lowest degrees of that process.

This effusion of serum, or of liquor sanguinis, as it is one of the earliest events of inflammation, so it is often its most important event; producing, mechanically, new symptoms, and giving rise to conditions of the most perilous kind. The quantity of serous fluid poured out in a short time is often immense. One of the pleuræ may be thus filled in a few hours, and the whole of one lung strongly compressed, and the respiratory apparatus reduced to one-half of its customary efficiency. And if inflammation and effusion should take place on both sides of the chest at once—if *double pleurisy* should occur, as it sometimes does—the patient must presently perish by apnoea, unless his condition is recognised, and free vent is given to the fluid. More than once or twice have I seen persons snatched from the brink of suffocation by what is called *tapping* the chest. Fatal coma is no uncommon result of the effusion of serosity, as an event of inflammation into the ventricles of the brain.

Even in the areolar tissue, where it is properly enough called oedema, a very trifling amount of this serous effusion may be sufficient to destroy life: when, for instance, it takes place into the submucous areolar tissue of the glottis, closing up by its pressure that little chink, the rima glottidis, and suffocating the patient after another fashion. Here also art may come to the rescue: an *artificial* chink or hole is made for the entrance and exit of air, below that part of the larynx in which the disease is situated, and the patient is delivered from imminent death. I have had two cases, under my own care, and have seen several others, in which life was so preserved. I allude to such cases now, merely to convince you of the importance of attending to this event of inflammation, and of studying the indications of its existence.

Sometimes some of the small vessels give way, and *hæmorrhage* into the part becomes an event of inflammation. Some slight degree of this occurs probably in most cases; and we frequently find that the colouring matter of the blood is mixed with the other effusions, giving to the serous liquid a deep tinge of red.

I hinted before, that we must not infer inflammation from the presence of serous effusion *alone*. Serum will exude, I believe, from loaded veins, even after death; but this never can be much in amount. It is certain that dropsical effusions may be, and very often are, the result of congestion of a purely mechanical kind.

A third *event* of inflammation is the effusion of what is called *coagulable lymph*, which, as I explained to you before, appears to be nothing else than the fibrin separated from the other constituents of the blood, and concreted. It is poured forth, at first, in a state of solution, or in a soft semi-fluid condition, and mixed with, or dissolved in, more or less serosity; but the fluid parts of the effusion are either soon re-absorbed, or soon separate themselves from the fibrin, which becomes firmer, and at length solid. The hard central portion of a phlegmon, in its earlier stages, owes its hardness to the presence of coagulable lymph in the natural interstices of the inflamed part; and a similar interstitial deposit of the same substance is common in various parts of the body, as a result or concomitant of inflammation. What is

called hepatization of the lung is one instance: the spongy texture of the lung is blocked up and solidified by this lymph. In certain cases of erysipelas, as well as in phlegmonous inflammation, the subcutaneous areolar tissue is rendered dense and hard in the same way. The white opaque spots which are often seen upon the cornea are produced by lymph interposed between the layers of that naturally transparent structure. But the most striking examples of effused coagulable lymph are to be seen upon the surfaces of inflamed membranes. The lymph forms a web or layer which by degrees assumes, itself, a membranous appearance; and is accordingly called by morbid anatomists a false or an adventitious membrane. Sometimes several layers of this kind are spread over each other, forming adventitious membranes of great thickness. When coagulable lymph is thus poured out between membranes that are habitually in contact with each other, it often causes them to cohere; just as two leaves of a book may be made to stick together by a layer of paste put between them. This result is very common indeed with serous membranes, especially the pleuræ, the pericardium, and the peritoneum. Lymph is sometimes also poured forth, under violent inflammation, from mucous surfaces. In croup, the interior of the trachea is inflamed, and a substance exudes which assumes a membranous form, and adheres more or less firmly to the sides of that tube, or is coughed up in ragged fragments. A similar effusion takes place occasionally from the mucous lining of the alimentary canal, and is expelled, with the other contents of the bowels, in shreds, or in tubular portions, which are, in fact, casts of the interior of the gut. It has been made a question whether the false membranes in these cases are really composed of fibrin. I may discuss that question hereafter. Similar films form within the uterus, and are moulded to the exact shape of its cavity, and marked with indentations that correspond to its rugæ; and these membrane-like casts are at length separated and extruded. These last are not very common; but I show you one which came from the uterus of a young woman who was a patient of mine in the Middlesex Hospital not very long ago. You may see lymph deposited like beads, upon the anterior surface of the iris under inflammation; or glueing its posterior surface to the crystalline lens behind it, and rendering the pupil irregular, and sometimes immoveable, or even sealing up that aperture. The internal surface of the heart, and especially its valves, are often studded with portions of lymph much resembling warts. When the opposite sides of an artery are brought together by a ligature, they inflame, and become united by the same medium. Coagulable lymph is effused, in the course of a few hours, upon the edges of a cut wound; and they adhere, under favourable circumstances, when placed in mutual apposition. This surgeons call union *by the first intention*; and the inflammation which is accompanied by this kind of exudation of lymph, or fibrin, is called *adhesive inflammation*; or the *adhesive stage* of inflammation.

It is seldom, if ever, that coagulable lymph *alone* is thus poured out. Sometimes it is tinged with the colouring matter of the blood. Oftener it is mingled with, or rather is at first dissolved in, a large quantity of serous fluid. In other words, the plasma of the blood exudes, and afterwards separates into fibrin and serum. When this happens in serous bags—as in the pericardium or pleura—the thinner fluid may keep the opposite membranes apart; and for some time, or entirely, prevent their agglutination. Sometimes the agglutination is partial, and the uniting portions of lymph are stretched out, by the distending effect of the fluid effusion, or by the natural movements of the parts, into strips or bridles of adhesion.

I must call upon you to notice, in passing, that although this event of inflammation may sometimes perhaps have a detrimental or destructive consequence, yet that in a vast majority of instances it is distinctly a salutary and conservative event. Vision may, no doubt, be destroyed by a plug of lymph which shuts up the pupil of the eye. A portion of intestine may become strangulated by a band of adhesion. Of this, which is a mere accident of the adhesion, I have seen several fatal examples. The closure of the trachea by the membrane of croup is scarcely, perhaps, a fair case in point. There are, at any rate, but few exceptions to the rule, that the effusion of coagulable lymph proves beneficial by preventing some worse event of the inflammation. It is better that suppurative inflammation of the areolar tissue should be limited and hemmed in by a barrier of lymph, than that it should extensively diffuse itself. It is better that the bag surrounding the heart, when it happens to be inflamed,

should become adherent to that organ, than that the inflammation should run on into suppuration, and fill the pericardium, and oppress the heart, with pus. In the one case, life may continue for several years; in the other, it seldom lasts many days. It is clearly more desirable, and more consistent with the safety and comfort of the patient, that his lungs should be fastened to his ribs, than that they should be compressed and flattened against his vertebral column. I shall have occasion so frequently to speak of this protecting and reparative tendency of adhesive inflammation, that I do no more than point it out to you at present.

When lymph has been effused upon an inflamed surface, it very readily becomes vascular and *organized*. Red streaks begin to be visible in it. These are incipient blood-vessels, which may soon be seen to be continuous with the blood-vessels of the inflamed part. The plastic lymph is fashioned, by outgrowth from adjacent vessels, into a definite structure, and made a living constituent portion of the body. It is, in truth, this remarkable *plastic* property belonging to the effused lymph, this aptitude for being organized, which invests the adhesive inflammation with its guardian and reparative character. None of the other fluids poured out under inflammation are capable of this change. It is in this way that the lips of recent wounds, and the surfaces of inflamed membranes in contact with each other, are permanently stitched together (if I may use so homely a metaphor), by living vascular threads. By this needlework of nature, parts recently severed from the body may sometimes be replaced, or even transferred and affixed to other situations, as in the Talicottian operation, whereby a new nose is engrafted in the place of that which had been lost. It is thus that ulcers fill up: successive layers of lymph exude, and are in succession attached to the ulcerated surface, and incorporated by this organizing process, until the breach of texture is repaired. Lymph thus organized comes at last to resemble areolar tissue, more or less condensed; or it tends "to assume, sooner or later, the characters of the tissue in or near which it is seated, or in place of which it is formed."

The length of time required for the exudation of coagulable lymph in inflammation, and for its subsequent organization, is variable under different circumstances. It is often effused very early. Dr. John Thompson found a distinct layer of it covering wounds he had made in an animal, in less than four hours after they were inflicted. It seems probable that vascular organization of the effused lymph may be effected within the space of a few days. Sometimes, on the other hand, two or three weeks may elapse before any such organization is observable. Mr. Paget affirms that its occurrence implies a cessation of the *inflammatory* process.

Several conditions seem necessary to ensure this adhesive form, or adhesive stage, of inflammation. The inflammation must reach a certain degree of intensity, or no lymph will be effused; it must not go beyond a certain degree of intensity, or the next *event* I have to mention, the formation of pus, will interfere with the adhesive process. We learn also from what takes place in recent wounds, that seclusion from the air, and the absence of all other causes of irritation, are necessary for adhesion, or union by the first intention.

It is supposed, by Vogel, —and the distinction is probably well founded, —that the permeation of mere serous fluid takes place through the walls of the veins; while the fibrinous portion of the blood exudes from the capillary vessels.

The formation of *pus* — *suppuration* — is a fourth *event* of *inflammation*, to which brief allusion has already been made.

Pus is an opaque, smooth, yellowish fluid, of the consistence of cream, and having little or no smell. I speak now of well-formed, or what is called good, or healthy pus; what the old writers spoke of as *pus laudabile*. This has been thought an absurd epithet; but it serves as well as any other to express what was meant, viz., that kind of pus which accompanies benign forms of inflammation, and indicates that all is going on regularly, and promises a fortunate ending; pus, in short, the appearance of which was *to be commended*. It is certainly not more absurd than the term *healthy* pus. This *pus laudabile* was described as being *album, leve, et æquale* — light-coloured, smooth, and homogeneous. This description of good pus has descended from the time of Hippocrates, who says, Το δε πύον, ἀριστον λευκον τε εἶναι, καὶ ὁμαλον, καὶ λειον, καὶ ὡς χρίστα δισώδες. It consists, as I told you before, of yellowish globules, diffused through a serous fluid. The globules are shown, by the microscope, to be

nucleated cells: and they are believed to be developed from germs which pre-exist in the effused plasma of the blood.

In order to explain the modern doctrine on this subject, it is necessary to go back a little.

The so-called serous effusions which take place under inflammation are some of them quite clear, some of them turbid. The clear effusion, while it remains shut up within the body, may remain clear for a long time; but being withdrawn, its fibrin presently coagulates, or lymph is deposited. I have seen several pints of transparent liquid removed by tapping the cavity of the chest, after having been confined there for many days; and immediately upon cooling, it separated into two distinct portions—the one limpid and thin, the other of the consistence of jelly, and distinctly fibrous when dried by pressure. Commonly, however, in pleurisy with liquid effusion, lymph is smeared over the surfaces of the pulmonary and costal pleuræ, which are only prevented from cohering by the intervening serum.

Now, of this inflammatory lymph there are described by Mr. Paget two varieties—the *fibrinous*, and the *corpuscular*.

The fibrinous variety is that which I have already spoken of as coagulable lymph, as constituting adhesions and false membranes, and as being capable of organization.

The corpuscular variety does not thus concrete or coagulate; but corpuscles (exudation corpuscles or cells they are called) form, and float free in the liquid: neither is this variety susceptible of being organized, but the corpuscles undergo degeneration; and one common mode of their degeneration or descent is into pus-corpuscles.

Sometimes they are broken down and disintegrated, and thus render the liquid with which they are mixed turbid, or whey-like.

In most instances of inflammatory exudation, perhaps in all, this fibrin and these corpuscles exist together, but in very varying proportions; and (to use Mr. Paget's language) "the preponderance of fibrin in the lymph is generally characteristic of the *adhesive* inflammation: the preponderance of corpuscles or their sole existence in the liquid, is the general feature of the *suppurative* inflammation."

The occurrence, in a given case, of one or of the other of these forms of inflammation, appears to depend partly upon the state of the patient's general health, the adhesive form prevailing in those who were previously sound and strong, the suppurative in those who were weak and cachectic; partly, as I shall show you by-and-by, upon the particular textures affected; and partly, as I have already stated, upon the degree of intensity of the inflammatory process itself.

Even the preventing or the allowing the access of air to the surface of a recent cut will make all the difference between adhesion and suppuration. And the same influence of the air in promoting the suppurative process in preference to the adhesive is remarkably seen in various other cases. In simple pleurisy—from exposure to cold—we seldom have any liquids effused, except coagulable lymph and serous fluid. But if the inflammation have been caused by a punctured wound from without, or by laceration of the *pulmonary* pleura by the sharp end of a fractured rib, or by a perforation of the pulmonary pleura from the extension of a vomica in the lung—in all which cases air finds its way into the cavity of the pleura—then true *empyema* results—pus is formed. So also in pneumonia: at first the inflamed lung is rendered solid by the effusion of coagulable lymph into the air cells; but if the inflammation persist, the next thing that happens is what is called by Laennec *gray* hepatization—a puriform infiltration takes the place of the lymph. The same principle is exemplified in the case of the urethra; inflammation of the *free* surface of its mucous membrane leads rapidly to the formation of pus; inflammation of its *attached* surface occasions the pouring out of fibrinous lymph, which produces stricture. And in general I think it may be said of surfaces that are open to the air, of tegumentary membranes, that either pus is formed upon them, under inflammation, without any previous effusion of plastic lymph, or the lymph is slight in amount, and transient in duration, and presently superseded by a puriform discharge. We have every day examples of this, in inflammation of the conjunctiva, of the bronchi, and of the bladder. Perhaps it is in this principle that we may find an explanation of the fact, that whereas in the inflammation of the areolar tissue, of glandular organs, and of the parenchyma of the viscera generally, the pus which forms is collected into a focus; circumscribed abscess in the substance of the *lung*, from common inflammation, such

as we are now considering, is very rare indeed. This is a point which will of course come under our consideration again.

There is, however, manifestly a close connexion in many cases between the effusion of coagulable lymph and the production of pus; although the progress and effects of adhesion and suppuration are very different. When suppuration takes place, the pain belonging to the inflammation usually abates, or ceases, except when the pus is imprisoned so as to keep up the pre-existing tension. Certain remarkable constitutional phenomena also declare themselves, which I shall notice again hereafter.

The effusion is longer continued in the case of suppuration; and the quantity of pus is more copious generally than of fibrinous lymph, especially in the serous and tegumentary membranes. When pus is diffused through the natural textures, it tends to soften and separate them—to break them down; whereas the direct effect of the deposition of coagulable lymph in the same parts is to consolidate and harden. Again, in some instances the final cause of the formation of pus would appear to be the ultimate elimination of coagulable lymph previously deposited.

When pus has been poured forth into one of the natural cavities of the body, there is said to be purulent *effusion*. When it is contained in a closed cavity, which is not natural, but formed by lymph and condensed areolar tissue, the collection of pus is called an *abscess*. It may also proceed from a free surface of the body—as the skin, or a mucous membrane, or a superficial sore.

The time required for the formation of pus is extremely variable. Suppuration sometimes very quickly follows the commencement of the inflammation; within a few hours, as in gonorrhœa. Sometimes it is postponed to a very distant period, even for weeks.

The duration of the suppurative process is also uncertain, and seems to have no fixed relation to the intensity of the inflammation by which it has been preceded or accompanied.

There are various modifications of pus; and its sensible qualities are liable to rapid alteration by various circumstances. Sometimes the globules are few in proportion to the more watery part; and then the pus is said to be *ichorous*. It is *sanious* when some of the colouring matter of the blood is poured out with it. It is sometimes viscid and slimy, from being mixed with mucus, or with an alkali; or flaky and curdled, which is common in scrofulous persons. Sometimes, also, instead of being, when cold, inodorous, it is horribly fetid. All abscesses that form in or about the alimentary canal are apt to contain pus of an offensive odour; as those which occur in the tonsils and near the rectum. A patient of mine, in the hospital, had a fluctuating tumour in the epigastrium, which Mr. Arnott opened. There came out the collapsed bags of two or three hydatids, and a quantity of stinking pus. The liver, no doubt, was the seat of suppuration in this case—and perhaps the stench might be owing to the death and decomposition of the hydatids.

Great pains were formerly taken in searching to discover some sure criterion between pus and mucus. Healthy pus and healthy mucus are so totally unlike each other, that they never can be confounded together. But sometimes we can scarcely say whether we are looking at mucus so altered as to resemble pus, puriform mucus—or at genuine, though not perhaps praiseworthy, pus. The microscope has superseded that quest. Having once had pus-globules shown you through its magnifying power, you can never fail to recognise their presence in any fluid, by the same help, afterwards.

In suppuration, some of the products of inflammation degenerate. The affected tissues themselves degenerate in what may be considered a *fifth event* of *inflammation*, namely, *ulceration*. You may remember my telling you that Kaltenbrunner observed the progress of absorption in the inflamed tissues which he examined by the help of the microscope: how the stellated spots gradually vanished from the web of a frog's foot, and the fat from the mesentery of the rabbit.

Independently of these microscopical observations, it is quite evident that absorption goes on, often very actively, during the continuance of inflammation. The effused fluids, or products of inflammation, the serum, the lymph, the pus, are partly taken up again: and not only are these products of inflammation liable to be so removed, but the original textures of the body are carried off by absorption. We cannot have a better proof of this than the progress an abscess makes to the nearest sur-

face at which the pus it contains may be discharged; the intervening textures are gradually absorbed. Perhaps a great part of the principle concerned in this progressive approach to the surface is *pressure*. The harder tissues of the body, the bones themselves, yield and disappear before the increasing pressure of an aneurismal tumour. In this case the absorption appears to be independent of inflammation.

But taking the process as one of the events of inflammation, we may say with Dr. Alison that whenever the absorption or the detachment of the effused lymph, and of the surrounding textures, takes place in excess—in a greater degree, that is, and more irregularly than seems to be required for any useful purpose—the result is *ulceration*. The term is, however, commonly restricted to those cases in which the loss of substance occurs upon some *surface*, internal or external.

Many circumstances influence the occurrence and progress of ulceration; and great differences are observable between the different tissues, in respect to the facility with which they severally ulcerate. Ulceration is most common in the tegumentary membranes. It is frequently met with also in the inner coats of the arteries, in cartilages, and in bones. But we are not always sure that it is in these cases an event of inflammation. Ulceration is rare in fibrous tissues of all kinds, in serous membranes, and in the outer coat of arteries. These differences have important pathological bearings. But I may not stop to consider these at present: they will be particularly noticed as the course proceeds. When I state that ulceration may lead to perforations of the alimentary canal, of the air-tubes, of the gall and urinary bladders, of the blood-vessels, and to the fatal escape of the natural contents of these organs, I have said enough to convince you that ulceration, so frequently the object of the surgeon's care, requires no less attention on the part of the physician.

There are certain forms of ulceration that are specific in their nature: with these I do not at present meddle. The process of ulceration is very clearly explained in Dr. Alison's admirable *Outlines of Pathology*.

There are three things generally going on at the same time in an ulcerated surface. First, there is an effusion of plastic lymph, by which what are called granulations are formed. Granulations consist of coagulable lymph which has become organized; furnished with numerous delicate blood-vessels. Secondly, there is suppuration: and, thirdly, there is absorption, or the removal of parts.

Sometimes, apparently, there is no *suppuration*: we see no pus in ulcers of the cornea, nor in certain cases of absorption of articular cartilages.

When the first of these three processes gets the better, if I may so speak, of the others, the lymph overspreads the surface of the ulcer, fills up the cavity, and the ulcer heals: cicatrization takes place.

When, on the other hand, the absorbing (or sometimes, possibly, the ejecting) process predominates, the ulcer extends itself—the excavation grows larger, or deeper—or both larger and deeper: and when this excess of removal is great, and the extension of the ulceration rapid, it is called *phagedenic* ulceration. When a part of the textures perishes during the process of the ulceration, and is separated in entire and sensible masses, the ulcer is said to be a *sloughing* ulcer. "When the process is slow, and the lymph effused at the base and round the edge of the ulcer is hardened, and the granulations on its surface are deficient, the ulcer is then said to be *callous* or indolent: and when the granulations are larger and softer, and more flabby than usual, and require to be repressed before the ulcer will heal: to this variety of ulcer the name of *fungous* ulcer is given:" and the coarse and too luxuriant granulations are called, by the vulgar, *proud flesh*. These several terms, in the senses now assigned to them, you will please to remember.

It is by regulating the three processes now described—so far as they are capable of being regulated by art—that the surgeon and the physician endeavour to obviate the threatened ill consequences of ulceration, and to promote the repair of the textures which have been destroyed.

I explained to you, in a former lecture, that inflammation may lead to a wasting of parts, although there is no suppuration or ulceration. The testis, for example, sometimes withers as a consequence of inflammation: *interstitial* absorption takes place. *Atrophy*, in short.

LECTURE XI.

Mortification, as an event of Inflammation. Inflammatory Fever. Hectic Fever. Typhus-like Fever. Modification of Inflammation by differences of Tissue: Areolar Tissue; substance of Glands and Solid Viscera; Serous Membranes; Synovial Membranes; Tegumentary Membranes — Skin — Mucous Membranes; Muscular Tissue; Arteries; Veins; substance of the Brain.

WE were occupied, when last we met, with what may be properly called the *events of inflammation*. We passed in review, 1st, *resolution* as an event of inflammation; 2ndly, *serous effusion*; 3dly, the exudation of *coagulable lymph*, constituting the adhesive form or stage of inflammation; 4thly, the formation of *pus*, or *suppuration*; and 5thly, *ulceration*. The pathology of these several events, so far as it is understood, and the change of symptoms to which they may respectively lead, were also treated of as fully as the limits of my course allow. At the close of the lecture I was about to speak of the sixth and last event of inflammation that requires to be noticed; viz. *gangrene, sphacelus, mortification*.

When mortification thus succeeds to inflammation, the part dies; it becomes cold; all circulation through it is at an end; all sensation in it is over. If it be an external part, its colour changes; from being red, it becomes mottled, purplish, green, or black; decomposition takes place; vesications appear filled with dark-coloured liquids; air is extricated also. If there be a great accumulation of fluid in the part there may still remain tension; but usually the mortified part is flaccid and boggy; and it emits a cadaverous smell.

When *internal* parts mortify under inflammation, they do not always assume this black appearance: often they are yellowish; or the soft tissue of the dead part readily imbibes fluid, and takes the colour of the substances with which it has been in contact. We see sloughs of the mucous membrane of the intestines presenting the ochrey hue of the fecal matters which had rested upon them.

What I have described as mortification, occurring externally, and succeeding to inflammation, is such as the surgeon witnesses. Sometimes it spreads, and loses and confounds itself, insensibly, with the adjoining parts, which still retain life; and which may continue actively inflamed, and subsequently perish also. Under more favourable circumstances, a distinct boundary line is formed between the dead and the living parts; and nature proceeds to amputate the portion which has lost its vitality. The process by which this is effected is extremely interesting. Adhesive inflammation constructs a barrier of lymph against any further advance of the mortification; a furrow of ulceration marks out upon the surface the commencing separation, and (supposing a part of one limb to have become gangrenous, the foot for example) the furrow gradually deepens, until the dead part is completely cut off. This very fact shows that all the textures of the body, skin, muscle, nerve, blood-vessel, and bone, are capable of being removed by the ulcerative process. Meanwhile very interesting changes occur in the part that lives: the large vessels are plugged up, to a certain distance, by the coagulation of the blood contained in them; the coagulation of the blood following its stagnation. They are further sealed up, and the smaller vessels also are closed, by coagulable lymph. Were it not for these changes, fatal hæmorrhage would follow the separation of the dead part. Now this is just what a surgeon rudely imitates when he amputates a limb; he cuts through the parts with knife and saw, and he ties the larger blood-vessels as he goes along. He follows the path which the natural processes point out: and in truth, a great part of both physic and surgery consists in learning what are the expedients of repair and preservation for which provision has been made in the living body; in promoting or controlling, in directing or imitating, those natural actions which generally tend, and often suffice, to restore health, and to save life.

Mortification is more common in some internal parts than in others. It is frequent in the areolar tissue; and in the mucous and submucous tissues of the alimentary

canal; in the throat, for example, in cynanche maligna; and in the glandular parts of the intestines in fever.

[The affection of the throat in cynanche maligna is not, strictly speaking, mortification. It has been shown by Bretonneau and others, to be a true pellicular inflammation. The false membranes, by which the fauces are covered, either in part or entirely, becoming tinged with blood, assume a dark or almost black appearance, and exhale an intolerable fœtor—which circumstances have caused them to be mistaken for sloughs—but when they become detached the mucous membrane is found to be perfectly entire, and without any appearance of softening; the peculiar odour of gangrene, also, is absent.—C.]

Mortification seldom affects the other mucous systems—those which belong to the air-passages and the urinary organs. It occurs sometimes—but not very often—in the substance of the lungs. It is seldom met with in serous and fibrous tissues. It is not at all uncommon in bone: producing *exfoliation* when it is slight and superficial; *necrosis* when the entire shaft of a long bone dies. In these cases the process of repair is slow, and we can watch it at leisure; and a beautiful process it is: but I must not stop even to admire it.

Now, mortification is no certain or constant event of inflammation. It depends, more or less, upon various causes and conditions. Sometimes upon the mere intensity of the inflammation, as in sloughing inflammation of the genitals: the progress of the mortification being best checked by those measures which are calculated to abate the violence of the inflammation. The sloughing of the cornea in gonorrhœal ophthalmia is another example to the same purpose.

Again, whatever tends to weaken the circulation in the part affected—or in the system at large—tends also to promote the perishing of the textures that are inflamed. In persons who are debilitated by fever, the mere pressure of the body against the bed is enough to produce sloughing of the integuments of the sacrum, hips, and elbows. The same phenomena are apt to occur in parts that are palsied. In dropsical patients, with feeble and impeded circulation, we find that a blister on the extremities, where the circulation is the *most* feeble, will sometimes cause mortification; while it might be applied to the chest without any risk of that event. Probably, in each of these instances, the unhealthy condition of the blood conduces to the sloughing process. Inflammation of the stomach and intestines is marked by a strong disposition to run into gangrene—and this again is consistent with what I formerly mentioned, of the depressing influence of inflammation of these organs upon the heart.

It is necessary to remember that mortification is capable of being produced by other causes, as well as by inflammation. The death of frost-bitten parts is perhaps scarcely an exception—the phenomena of mortification occur in them after the reapplication of a certain degree of heat—sufficient, probably, to give rise to more action than the frozen parts can bear without perishing. But the mere cutting off the supply of arterial blood, independently of any inflammation, will cause mortification. Ossification of the arterial trunks, and consequent stagnation and coagulation of the blood in them, is the commonest cause of the dry gangrene of old persons—the *gangrena senilis*; which, by the way, is not always dry. In the majority, however, of these cases, the gangrenous part, not being preternaturally loaded with fluid, does not so rapidly putrefy; but remains dry, and shrinks up. Again, whatever tends to prevent the return of the venous blood from a part (as a firm ligature placed round a limb—or the constriction of the gut in strangulated hernia) is favourable also to the production of mortification. Probably here too the direct or indirect pressure made upon the arteries is chiefly concerned in occasioning the death of the part. We see limbs mortify sometimes after their principal artery has been tied for the cure of aneurism, when the collateral arterial circulation fails sufficiently to establish itself: we see the same thing when the passage of the blood through the main artery of the part is stopped, by external injuries; and I shall have to show you hereafter, that the artery *may* be plugged from within.

The difference between dry and moist gangrene is mainly this: that in the first, the tissue dies through defect of blood; in the second, through stagnation of blood.

There is also a very curious form of chronic and dry gangrene, produced by the

continued use of diseased grain as food—and particularly of the spurred rye; and to this, as a distinct disease, I may perhaps have occasion to direct your attention hereafter.

The different stages and events of inflammation that have now been described are accompanied by corresponding disturbances of the system at large. These were touched upon (barely mentioned, however) in that rough and general outline which I attempted to sketch in the outset, of the various phenomena of inflammation; and to which I have since referred as a type. They require, for many reasons, to be considered somewhat more minutely.

When, as *surgeons*, you have to deal with *external* inflammation, you have no difficulty, in the first place, in ascertaining its actual existence: you see it; and you know besides, merely by looking at the part, and perhaps by handling it, what changes it has undergone. You may perceive that the opposite lips of a wound have adhered: or that a phlegmon, in which you can also detect fluctuation, has assumed a pyramidal form, and begins to look white upon its summit: or you observe that the abscess has broken, and left an ulcer behind it, which pours out pus, and which shows a tendency to contract, or to enlarge itself: or you may remark the alteration of colour and of temperature which denotes the approach of mortification, or the actual death of the part. The mere exercise of your external senses apprises you, not merely that there is inflammation, but also whether it is of the *adhesive* kind; or has reached the degree of *suppuration*; or has produced *gangrene*. At the same time you do not fail to notice the nature of the *constitutional* disturbance that may be present; and the knowledge thus obtained of the local and of the general symptoms determines your plan of treatment.

But when, as *physicians*, we have to do with inflammation of *internal* parts of the body, and when the local changes attendant upon that process are concealed from our view, the case is very different. We should often be unable to make out the nature of the disease at all, if the presence of pyrexia did not instruct us. Sometimes the constitutional disturbance is all that is apparent, until after death. And as the disease proceeds, we frequently are able to judge that this or that *event* of inflammation has taken place, only by observing the indirect symptoms which declare themselves through the medium of the system at large. Yet it is in many cases of the greatest importance to mark the transition from one stage or event of inflammation to another; and to learn whether, and in what degree, the more urgent of the symptoms depend upon the inflammation itself; or upon the effects which it has produced. I do not mean to say that we have not, sometimes, as sure indications afforded us by *direct* symptoms, cognizable by the sense of hearing or of touch, of the state of internal organs, as we *could* have if they were exposed to our view. To these direct symptoms I am not now about to refer; they must be spoken of in connexion with the diseases to which they belong. But the information which the physician gains from what may be called *constitutional* symptoms is *always* highly valuable; and it is *sometimes* the *only* information that offers any guidance to the remedial measures he ought to adopt.

Inflammation sufficiently extensive or intense to disturb the general system at all, is attended with pyrexia: and the presence of pyrexia, when the part affected is unseen, marks the *nature* of the disease. The most prominent of the symptoms that denote the existence of *inflammatory fever* are debility and chilliness; followed by, or alternating with, increased heat of skin; and increased frequency and force, and often *hardness* of the pulse; with considerable derangement of most of the natural functions of the body. Commonly there is headache and confusion of thought, languor, thirst, loss of appetite, a furred or white tongue.

Among these leading symptoms, the chilliness, often amounting to shivering, has this particular importance attached to it, that it marks the *date* of the febrile disturbance. And it is worth observing that rigors more commonly attend the commencement of spontaneous inflammation, than of inflammation caused by external injury.

Now, without going more into detail—of this febrile condition belonging to the early stage of inflammation, I make the following remarks.

1. That it generally *succeeds* the manifestation of the local symptoms of the inflammation: and that we cannot, therefore, help considering the fever as the natural *effect* of the inflammation. *

Kaltenbrunner describes an experiment of this kind. He says, if a drop of alcohol be applied to the web of a frog's foot, the blood presently flows towards the part irritated, and the circulation in it is accelerated; *congestion* takes place, and follows its known march.

If the dose of alcohol be augmented, the phenomena of congestion increase considerably, and extend over a larger space: at length points of stagnation appearing in the focus of the affected part announce the establishment of *inflammation*.

If the dose of alcohol be still further increased, we observe that, on the one hand, the inflammatory points of stagnation become larger and more numerous; and that, on the other, the circumferential disturbances of the circulation extend themselves, so as at length to implicate the whole of the circulating system: they give rise to a *fever*, which is *added* to the *inflammation*. The circulation in the web of the opposite foot is as much accelerated as in the vessels surrounding the inflamed part in the first foot. If the word congestion had not a local meaning, we might call fever (he says) a general congestion.

Probably the fever may be owing to the circulation of altered blood throughout the body. We know that the blood is altered in these cases, inasmuch as it is found to contain an unusually large proportion of fibrin; and has acquired the unnatural quality through which, when withdrawn from the body, and allowed to coagulate, it exhibits the buffy coat.

That the febrile state follows the local inflammation in point of time, is then the rule; but this rule has frequent exceptions. Erysipelas, and all the febrile exanthemata, afford instances of exception; the fever sets in before any manifestation at least of the local symptoms. These are indeed diseases of a specific kind, in which the blood is believed to be infected primarily; but the same is true sometimes of diseases that appear to be simply inflammatory; such as inflammation of the lungs, and cynanche tonsillaris. There are other cases in which the local symptoms and the general febrile disturbance appear to burst forth simultaneously: this is seen in certain instances of pleurisy, and of peritonitis.

2. Again, it is a curious circumstance that the inflammatory fever is not always proportioned, in its degree of violence, to either the size or the importance of the part inflamed. The pyrexia is often very strongly marked in that common complaint, the quinsy, inflammatory sore-throat, cynanche tonsillaris — which can scarcely ever be said to imply much danger.

3. The situation, the extent, and the degree, of the local inflammation being the same, the fever commonly runs higher in young, and in plethoric persons, and in those of sanguine temperament, than under the opposite conditions.

4. The inflammatory fever may be modified in the outset, or very early indeed, by the nature of the part upon which the inflammation has seized. I have several times mentioned the peculiar depressing effect upon the action of the heart, produced by inflammation of the stomach and bowels, and of some other of the abdominal organs; and particularly by inflammation involving their peritoneal covering. This lowering influence (which is analogous to that of certain mechanical injuries to the abdomen) has been supposed to depend upon the subduing and sickening kind of pain which is apt to accompany inflammation of these parts. Probably such pain accompanies, rather than causes, the depression. However the latter may arise, it gives a peculiar character to the inflammatory fever; lessens the amount of reaction, or abridges its duration; affects especially the quality of the pulse; and carries with it a strong tendency towards death by asthenia.

5. There is no doubt either that the character of the inflammatory fever is liable to be considerably modified, from the first, by the previous habits of the patient. In persons who have been habitually intemperate — or who have been subject to long-continued excitement of the nervous system of any kind — the fever which attends inflammation approaches more or less to the type of typhus fever, from the very beginning. The febrile reaction is less strongly pronounced. The functions of animal life are sooner and more deeply involved in the train of morbid actions. Stupor and delirium are apt to occur; with extreme debility and irregular movements of the voluntary muscles. Still more conspicuous are these peculiarities in some cases of inflamed veins; and whenever inflammation is produced or accompanied by the introduction of certain animal poisons into the system.

6. The relative duration of the inflammatory fever is subject to some variety. It may persist for a little while, for a few days even, after all the local signs of inflammation have disappeared: this happens chiefly in persons of an irritable habit. We watch such cases narrowly, not without some apprehensions of a relapse. On the other hand, a rapid abatement of the febrile symptoms sometimes takes place, while the local changes continue, or even for a time increase in extent. Nevertheless, we hail this change as a favourable augury of the ultimate result.

When inflammation, external or internal, has gone on to the formation of pus, *that* event is frequently marked by the supervention of peculiar symptoms; and the character of the fever undergoes, for the most part, a striking alteration.

It is very important to ascertain the time when this event of inflammation takes place, or is at hand: for the measures which might have been proper and necessary while any prospect remained of the *resolution* of the inflammation, may be useless and even hurtful, if continued after that prospect is at an end.

When the surgeon perceives any indication of the formation of pus in an external part, he mostly despairs of being able to bring about resolution; ceases to abstract blood from the part, or from the system; and applies perhaps warmth and moisture, by means of a linseed poultice, to promote the suppuration. And a corresponding change of plan is required in internal inflammations.

Now, the *commencement* of suppuration is often marked by rigors; and its *continuance* by *hectic fever*.

If, after the symptoms of inflammation have lasted for a certain time, the patient be attacked by cold shiverings, which are followed by some increase of heat, that circumstance alone is enough to make us suspect that pus is formed, or is about to be formed: and to teach us that the measures employed to effect a resolution of the inflammation have not been successful.

Rigors are very striking symptoms; but they are by no means necessarily connected with suppuration. They usher in, as I presume you know, most forms of fever, appearing at the very outset of the disease. They recur, at regular intervals, in intermittent fevers. Slight causes will, sometimes, produce them. For instance, they often follow the introduction of a bougie into the urethra. But when they occur *after symptoms of internal inflammation have been for some time present*, they denote, in most cases, the production of pus in the part or organ inflamed. Sometimes one such shaking fit only is observed: sometimes several take place. When they recur, it is usually at irregular intervals; but cases do happen in which the shiverings indicative of internal suppuration are so strictly periodic, that unless all the circumstances be carefully taken into account, they may be mistaken for signs of ague.

The leading symptoms of *hectic fever* (by which, I say, the *continuance* of suppuration is commonly marked) are an abiding frequency of pulse; alternations of chilliness with heat and flushing, followed by perspiration; a gradual wasting of the body; and progressive debility.

I shall hereafter have to speak of a very different kind of disease, in which, however, there is a succession of symptoms resembling more or less closely the series that characterizes hectic; I mean *remittent fever*; the succession of symptoms being chilliness, heat, perspiration. But these two disorders are in most cases discriminated from each other by the circumstances under which they occur.

The symptoms of hectic fever often creep on, at the outset, insidiously, and almost imperceptibly. "A very slight degree of emaciation, a pulse a little quicker than ordinary, with a small increase of heat, especially after meals, are often the first symptoms which can lead us to suspect the formation of hectic." Cullen has described hectic fever as consisting of two exacerbations in the twenty-four hours—one about noon, the other towards evening; but in many cases the latter alone is distinctly marked. The patient feels shivery and cold towards night; then the skin becomes hot and dry, especially in the palms of the hands and the soles of the feet, and the pulse becomes *more* frequent; and in the middle of the night, or towards morning, he wakes from short and uneasy sleep, in perspiration, which is often profuse. Sometimes, however, there are two or three fits in a day. The paroxysms are shorter and less regular than those of intermittent or of remittent fever. Each of the three phenomena constituting the series may, in its turn, be wanting: and even if the paroxysms

are regular for two or three times together, they never continue to be so. Many circumstances connected with the paroxysm itself are very distinctive. "The hectic patient," says Dr. Heberden, who has left us a very good account of this affection in his *Commentaries*, "is very little or not at all relieved by the breaking out of the sweat; but is often as restless and uneasy after he begins to perspire as he was while he shivered or burned. All the signs of fever are sometimes found the same after the perspiration is over; and during their height the chilliness will in some patients return, which is an infallible character of this disorder. Almost all other fevers begin with a sense of cold, but in them it is never known to return and to last twenty minutes or half an hour, while the fever seems at its height, which in hectic will sometimes happen."

Hectic fever is one of the fearful accompaniments, and sometimes the most strongly marked symptom, of pulmonary consumption: and where the existence of that complaint is suspected, yet a matter of doubt, we look for indications of hectic fever with the greatest anxiety and dread.

With relation to hectic fever, considered as an indirect symptom that suppuration has succeeded to inflammation, and is still going on, it will be worth your while to notice the strong contrast it offers, in many particulars, with the *inflammatory* fever that attends the earlier stages of inflammation.

The pulse loses much or all of its *hardness* and strength, but it remains permanently more frequent than the pulse of health; the appetite returns in great measure; the thirst abates; the tongue, instead of being covered with a white fur, becomes clean and moist, and towards the end is sometimes unnaturally red, or speckled with aphthæ; there is no longer headache or confusion of thought.

A few more touches will suffice to fill up the picture of hectic fever.

The face is usually pale; but during the exacerbations it is partially flushed, and very often a characteristic circumscribed red spot appears upon either cheek. Besides the evident emaciation, various minor changes mark the want of proper nourishment: the skin, when not perspiring, is harsh and scurfy; little branny scales may be rubbed from the legs, merely by the friction produced in drawing off the stockings; the hairs become fine and fall off; the finger-nails are incurvated into an adunque form; and the sclerotic coat of the eye, as seen through the conjunctiva, becomes of a pearly white. As the disease advances, œdematous swellings of the ankles are very apt to come on.

The connexion between hectic fever, and the formation of pus in some part or other of the body is so frequent, that it has been deemed, by persons of great experience and sagacity, a *universal* fact. Dr. Cullen tells us, in his *First Lines*, that he had never seen hectic in any case, when there was not evidently, or when he had not ground to suppose there was, a permanent purulency or ulceration in some external or internal part. And Dr. Jno. Thomson, speaking of the opinion that hectic might occur independently of suppuration, uses these words:—"But till facts more decisive, and cases more accurately described than any which have yet appeared, are produced in proof of that opinion, I shall think myself justified in adopting the *common* opinion; and in believing that hectic fever is in every instance connected, if not with the *absorption*, at least with the *formation* of pus."

The notion alluded to in the latter part of this quotation was at one time very commonly entertained, viz., that hectic fever resulted from the re-absorption of pus into the blood; but there are many facts decidedly opposed to this belief. Considerable collections of matter not unfrequently disappear, *i. e.*, are taken up again into the blood, without occasioning the slightest approach to hectic. Again, hectic will accompany, and be kept up by, a scrofulous joint attended with an open sore, and it will sometimes cease at once, and completely, upon the removal of the diseased limb by amputation; although a greater quantity of pus is secreted by the stump, than had been secreted in the diseased part previously to the operation. Facts like these prove, I think, that hectic is not simply a consequence of the absorption of pus into the blood: and they seem to have suggested to Mr. Abernethy the notion (which was held, indeed, by John Hunter also) that sympathetic hectic fever is a *teased* action of the system, endeavouring to throw off what annoys it: the cause of irritation being removed, it ceases forthwith.

And there is another conclusive circumstance to be mentioned. Notwithstanding

the opinions I just now quoted from Cullen and Dr. Thomson, I believe few persons who have attended to the subject, doubt, now, that there is such a thing as *idiopathic* hectic; hectic unconnected at least with suppuration anywhere. We often see hectic, or a general state of the system not to be distinguished from hectic, in mothers who have suckled their infants too long: we see it too, sometimes, if I mistake not, in newly-married husbands: and it may be noticed as occurring more or less distinctly in those who labour under diabetes. What is common to all these cases is, that there is an habitual drain upon the system beyond what the nutriment taken into it can supply and counterbalance. It is certain, too, that hectic fever sometimes happens in phthisis, not only before there has been any expectoration of puriform matter, but prior even to the softening and suppuration of a single tubercle. I call to mind one instance in particular of this. The hectic was distinctly marked, and continued long. The patient died, at last, comatose, after two attacks of convulsion. Two or three large scrofulous tumours were found imbedded in the substance of his brain. Various other organs were infested with tubercles; but the tubercles were all of them still hard and crude.

However, setting aside these rarer cases of exception, there can be no doubt that hectic fever, considered as a constitutional symptom of mischief that *may* reveal itself by scarcely any other token, and especially as a sign of suppuration, deserves all the attention we can give it; and for that reason have I spoken of it rather at large. Whenever I mention hectic fever in the further progress of these lectures, you will know all that I wish to express by that term.

I have very little to say at present respecting that modification of the general febrile disturbance, which sometimes attends *mortification* as an event of inflammation. I stated before that the fever is apt in these cases to assume those features which belong to the later stages of typhus fever; and to be characterized by sinking of the pulse, shrunken features, coldness and clamminess of the skin, a dry and black tongue, low muttering delirium or stupor, tremors of the voluntary muscles, with spasmodic startings of their tendons, and insensibility to the passage of feces and of urine. I must, however, now inform you that these typhus-like symptoms are no constant or necessary concomitants of mortification. The natural mode of death, under gangrene, is death by asthenia. But typhus in its advanced state involves the nervous functions, and tends to death by coma. Whenever, therefore, typhus-like symptoms supervene upon inflammation which ends in sphacelus, they may with much probability be attributed to some contamination of the blood by an animal poison; and such contamination may have taken place previously to the mortification, and have even helped to produce it, as when inflammation arises during the progress of the contagious febrile disorders; or it may occur as a consequence of the mortification itself, by the direct absorption into the system of some of the putrefying and poisonous elements, into which the dead part has been resolved.

One circumstance, worth bearing in mind, as sometimes indicating the super-vention of internal mortification, is the sudden cessation of pain: giving hope to the patient and his friends that the danger is over; but not deceiving the experienced physician.

So much, then, for the local and constitutional events of inflammation, considered generally.

It remains for me to make some observations upon the modifications of inflammation, according as it affects the *different tissues* of which the body is composed. Many of these observations I have, indeed, already anticipated; but it will be useful to bring together, under one view, the most material facts ascertained on this matter.

When inflammation affects the *areolar tissue*, all the events of inflammation which I have taken some pains to describe are apt to occur; and for that reason, inflammation of this tissue, as it exists beneath the skin, was chosen by me as a convenient type, or general representative of the inflammatory process. It is, therefore, the less needful that I should take up much of your time in speaking of the characters of inflammation exhibited in areolar tissue. There is a strong tendency to form circumscribed abscesses: the extension of the suppuration is prevented by a wall of lymph

built up around it. The adhesive inflammation sets bounds to the suppurative. There is a good deal of pain when the areolar tissue is so situated that tension is occasioned by its swelling.

But sometimes no such boundary wall is erected, and the inflammation spreads and diffuses itself, and becomes a very terrible disease, destroying the areolar tissue over a large and undefined space by a process compounded of sloughing and of bad suppuration. When the skin also is implicated in the inflammation, the disease is usually called *erysipelas plegmonoides*: when the skin is not involved, it has been called *diffused inflammation of the cellular membrane*. This diffused form of inflammation frequently follows the introduction of animal poisons into the system; and accompanies the inflammation of veins and of absorbent vessels. It is this disease which is so often fatal to members of our profession, when it results from wounds or punctures received in opening dead bodies. Dr. Craigie has recently put forth the opinion that in these cases of spreading inflammation it is the *adipous* tissue that is affected.

The substance of the *larger glands, and of the solid viscera* of the body, suffers changes analogous to those observed in the areolar tissue: probably because areolar tissue enters largely into their composition. Acute inflammation of the liver, when it does not terminate in resolution, leads to abscess in that organ. Abscess is rare in the lungs, perhaps for the reasons mentioned in the last lecture. Gangrene is also uncommon in the pulmonary substance: and quite unknown, I believe, in the liver, and very rare in the kidney. Inflammation of the latter organ is not unfrequently attended by purulent collections. Inflammation of the substance of the viscera is not, in general, attended with much pain.

The areolar tissue is liable to be rendered permanently thick and hard by *chronic* inflammation, as well in the parenchyma of internal organs as where it is spread out beneath the skin, or beneath serous or mucous membranes. Chronic induration and thickening of the areolar tissue which composes Glisson's capsule is no unfrequent result of slow inflammation; producing that particular change in the liver which the French pathologists denominate *cirrhose*; and of which I shall have more to say hereafter.

The inflammation of *serous membranes* is characterized by sharp and severe pain; by hardness of the pulse: and by buffy blood; by its tendency to spread; by the effusion of serous fluid, and of coagulable lymph; and sometimes, when the inflammation is very violent, or air gets admitted to the inflamed surface, by the effusion of pus. Speaking generally, however, it is *adhesive inflammation* which we most expect in this tissue. False membranes, consisting of organized lymph, belong to it: and the agglutination of contiguous surfaces. Sometimes the lymph, instead of being deposited in flakes or layers, appears in the form of numerous small granules: this is a phenomenon frequently observable in inflammation of the arachnoid, and of the peritoneum. Sometimes it has a villous or papillary or shaggy arrangement; or is cellular like a honeycomb. This is common in the pericardium. The surface (to use the happy simile of Laennec) resembles that which may be produced by separating two flat plates between which a layer of soft butter had been spread: and it probably depends upon a similar cause; since in health a perpetual sliding motion of the pericardium over the heart is going on. Ulceration of a serous membrane is very uncommon. I mean ulceration *commencing* in that tissue; for these membranes are frequently perforated by ulcers which approach them on their attached side, and which begin in other tissues, especially the mucous. Neither does mortification occur in serous membranes, except sometimes by communication from other parts. The effect of *chronic* inflammation of the serous surfaces is to thicken, harden, and pucker them. We see this effect in the omentum frequently; in the peritoneal covering of the liver; in the serous membrane which forms so large a portion of the valves of the heart.

The *synovial membranes* have a strong analogy with the serous. Gendrin includes the two in the same category: yet their behaviour under inflammation displays, in some respects, a marked distinction between them. They are less *liable* to inflammation than the serous membranes: they rarely throw out coagulable lymph, and, consequently, adhesion of their opposite surfaces is very uncommon. Joints do not become immoveable, or what is called ankylosed, in consequence of the agglutination of their synovial surfaces; but, generally, by means of granulations arising upon those surfaces after they have ulcerated. Very seldom indeed does pus form in the

synovial sacs, except (again) the inflammation has been caused by mechanical injury, which has laid open the joint, and admitted air. When this is the case, very serious constitutional disturbance is apt to take place, and the existence of the sufferer is endangered. That this does not depend upon the *mere violence of the exciting cause* is evident from the circumstance that the same acute inflammation, the same general affection of the system, and equal danger, often result from the careful incision made into a joint by the surgeon, for the purpose of removing loose portions of cartilage. I have now at the hospital an out-patient who has, among other ills, a large cartilage floating about in fluid in one of his knee-joints: but I believe that Mr. Arnott, whom I have consulted on the case, will be very slow to recommend its extraction, unless the inconvenience produced by it becomes so great as to incapacitate the patient from pursuing his employment, and other methods of relief shall fail. Suppuration of the joints is also one of the occasional consequences of phlebitis. Inflammation of the synovial membrane speedily leads to a *serous effusion* into the joint, which often, especially in rheumatism, is as speedily taken up again.

Let us next inquire into the modifications which inflammation undergoes when it affects the *tegumentary membranes*.

Considering the *skin* as *one membrane*, and neglecting its subdivisions into epidermis, rete mucosum, and cutis vera, we find that inflammation assumes a variety of forms in this external covering of the body. Many of these belong to specific diseases, and do not fall within my present purpose, which is that of noting how *common* inflammation varies in the different tissues.

When the inflammation is superficial, it frequently is denoted by a diffused red blush only, which may be banished for the moment by the pressure of one's finger, and which after a certain time disappears of its own accord—terminates by resolution; the only consequence of the inflammation being the separation of the cuticle in small branny fragments; in one word, *desquamation*. We call the superficial inflammation in this case, *erythema*. If the inflammation have been a little more intense—as in some cases of *erysipelas*, in *scalds*, and in that which we are every day exciting by *cantharides*—a serous fluid is poured out, which elevates the cuticle in larger or smaller patches of vesication. Remove the cuticle and admit air, and the *serous* effusion becomes *purulent* effusion: and if the inflammation be pressed beyond a certain point by any *other* stimulus besides that of air, we may then too have pus poured out. Erysipelatous (which is also a specific) inflammation of the skin is characterized by its remarkable tendency to spread: and a most singular circumstance attends several of the other specific inflammations of the skin—viz., that having occurred once, they never occur again: this peculiarity belongs, however, to the great constitutional diseases, of which the cutaneous affection forms merely a part.

Inflammations of the *internal tegumentary membranes*—of the three internal surfaces that communicate with the air, and are clothed with *mucous membrane*—are very interesting to the physician: and the first thing which strikes our attention in respect to them is the indisposition they manifest to adhesive inflammation: and we are struck at the same time with the beauty of this provision. If the mucous membranes were as ready to throw out coagulable lymph, and to adhere to each other, as the serous, almost every occurrence of inflammation in them would prove necessarily fatal; by closing up the *inlets* of the air passages; or the *outlets* of the urinary passages; or any part of that long mucous canal which, passing through the body, requires a free opening at both of its extremities. But the inflamed mucous membrane pours out serous fluid; or viscid mucus; or pus; or blood. The product partakes at first of the character of the secretion proper to the part inflamed; or it is mixed with some of that secretion. Inflammation of these membranes is, however, sometimes attended with the exudation of something which is very like coagulable lymph. The tracheal, bronchial, and pulmonary mucous membrane, the œsophageal, the intestinal, and that which lines the uterus, are all more or less subject to the formation of adventitious membranes under inflammation. Casts of the smaller branches of the air-tubes have, in rare instances, been repeatedly coughed up in large quantity; constituting what have been very inaptly called *bronchial polypi*. The membranous exudation of croup is well known; a tubular substance is formed in the trachea, and, sometimes, fortunately expelled: but too often it suffocates the patient. Similar concrete exudations, broken into irregular shreds, are occasionally voided by

stool. It is said that a long membranous mass of the same kind, in size and shape like an earth-worm, has been discharged from the urethra; having formed there in consequence of the injudicious use of stimulating substances, injected with the view of checking the more innocent effusion of pus. The films, or membrane-like flakes which are thus incidental to inflammation of the mucous surfaces, resemble, I say, in their general appearance and disposition, the strata or layers of coagulable lymph which are the ordinary product of inflammation of the closed serous surfaces. But they differ from these in some remarkable points. They are softer. They never contract permanent or strong adhesions to the subjacent or inflamed membrane; but are partially separated from it by the intervention of thinner matters, serous or puriform. Above all, they never become organized. They appear to consist of inspissated and altered mucus; and are composed, in a great measure, of albumen. An opinion has been entertained that the *want of apposition* of the opposite surfaces has a great deal to do with their indisposition to cohere. The mucous air-tubes are kept open and apart by their *structure*: the stomach and intestines by their *contents*, or by the frequent passage of solids and fluids through them: and therefore (it has been supposed) they have no *opportunity* of adhering. But there can be no doubt that these mucous membranes are but little disposed to throw out true plastic lymph at all: and when their opposite surfaces do grow together, I believe it will almost always be found that some abrasion or ulceration of the mucous surface had previously happened.

Inflammation affecting the mucous membranes has sometimes a strong tendency to spread and wander: sometimes, on the contrary, it is strictly confined to a small and definite space. In the former case it commonly restricts itself for a long time, or altogether, to the mucous tissue, leaving the neighbouring tissues untouched. In the latter it is apt to penetrate to the subjacent parts, and to produce obvious and enduring alterations of structure. The membrane becomes fastened to the parts which it should loosely clothe, and not unfrequently it ulcerates or sloughs.

The spreading form of inflammation is most often met with in the air-passages. Ulceration and sloughing, and circumscribed inflammation, are more common in the alimentary canal.

There is a remarkable contrast between the serous membranes and the mucous, in respect to the *pain* which attends their inflammation. Very little pain is experienced in many cases, when inflammation affects the mucous lining in any of the three systems, except towards their openings, where the membranes are about to become continuous with the external skin: in the mouth and throat, for example, the pharynx, the rectum, the vagina, the extremity of the urethra. And as inflammation of the mucous membranes is attended with less *pain*, so also it is accompanied by less *fever* than when the serous membranes are attacked; and the blood more seldom exhibits the buffy coat.

The *muscular tissue* appears to take on the actions of inflammation very reluctantly: and its vessels seldom, if ever, pour forth any of the products of inflammation. The chief effect of inflammation upon muscle is the destruction of its contractile properties. Serum and lymph, and even pus, are sometimes found diffused through muscular parts; but there is reason to believe that these effusions are rather the consequence of inflammation of the areolar tissue which enters into the composition of the muscle, and ties together its fleshy fibres, than of inflammation of those fibres themselves.

I have remarked already that inflammation of an *artery* presently leads to the effusion of lymph, and the coagulation of the blood, within the artery. But arteries do not readily inflame, except under mechanical injury: they do not often suppurate either: and they possess a singular power of resisting mortification. Dr. Jno. Thomson declares that he has seen cases of phlegmonous erysipelas, in which "several inches of the femoral artery were laid completely bare by the gangrene, ulceration, and sphacelus of the parts covering it, without its giving way before death."

Inflammation of the *veins* is much more common than that of the arteries; and it is a disease of fearful interest. In some cases it leads to a deposit of fibrin upon the inside of the vessel, "furring it over," as Mr. Hunter says. The blood soon coagulates, and blocks up the inflamed vein, or leaves, perhaps, a narrow passage in its centre. From this mechanical obstruction to the current of the blood new symptoms arise. The part from which the venous trunk receives its tributary branches becomes

œdematous or dropsical. — Inflammation of the femoral vein, obliterating its cavity, is the essence of the complaint known to pathologists under the name of *phlegmasia dolens*: a complaint which may happen to persons of any age, or of either sex; but which is most common in women, soon after parturition.

This, which may be considered the adhesive form of phlebitis, is also its most innocent form. Too frequently the inflammation runs into suppuration: and then it proves a most terrible and almost hopeless disorder. The vein remains pervious; pus, of an unwholesome and poisonous quality — or some morbid product of the inflammation — is carried into the blood; which thus scatters, in its course, the seeds of inflammation, and determines the rapid formation of purulent collections, in various and distant parts of the body, and especially in the lungs, the liver, and the larger joints. Great constitutional disturbance ensues, and fever of a type like that of typhus is often established. To this, the destructive form of the disease, parturient women are also peculiarly liable. Phlebitis of the uterine veins constitutes the source of the most dangerous and deadly varieties of puerperal fever. It is the same disease which gives to a vast majority of those surgical operations that are followed by death, their fatal character.

We hear continually of inflammation of the *brain*; but what is so called is, most commonly, inflammation of the *membranes* which invest the brain. Inflammation of the cerebral substance itself is, however, not very uncommon; but it is more frequently the result of injury than of spontaneous disease, and it is usually confined to a limited portion of the brain. Softening and suppuration are its ordinary events. Sometimes pus is met with occupying a distinctly circumscribed space; the pus is collected into an abscess. Sometimes, on the other hand, it lies loose, as it were, and surrounded by broken-down cerebral matter, or it is infiltrated into the cerebral pulp. Around the softened portions the inflamed substance of the brain is more dense and firm, sometimes, than is natural. Whether this be owing to the presence of coagulable lymph, has not (so far as I know) been clearly ascertained. Mortification must be very rare in the nervous substance. Dr. Baillie has described it as occurring after violent injury. Once or twice in my life, portions of brain have been shown to me, protruding through an aperture in the skull, dead, of a dark colour, and having an offensive smell. Excepting in these cases of *hernia cerebri*, I have never seen sphacelus of the brain from any cause.

Perhaps, however, I am incorrect in saying this. I formerly told you that portions of the brain often become soft and diffuent, when there has been no inflammation; but simply from atrophy, depending on a diseased state of the nutrient arteries of the brain. Now *this* is, by some persons, called mortification of the cerebral substance. They consider it quite analogous to the gangrena senilis, which results from a similar cause, although it happens in another part of the body. The nature of the change, they say, is the same, although its physical characters differ. If this be so, I have seen gangrene of the brain some scores of times; but still I should be able to declare, that with, perhaps, the exception already mentioned, I have never seen unequivocal mortification of the cerebral substance *as the result of inflammation*: which is what we have now been considering.

This concludes, gentlemen, what I have to say concerning the phenomena of common inflammation, as they are perpetually witnessed in the various textures of the body. I have not, indeed, gone through all the tissues; I have said nothing of the peculiar effects of inflammation in cartilages, for example, and in bones; but I have glanced at all those tissues, in the inflammation of which the physician is chiefly concerned. Upon such points as I have purposely omitted, you will be amply instructed by my colleague, the Professor of Surgery.

LECTURE XII.

Varieties of Inflammation: Acute and Chronic; Latent; Specific. Scrofulous Inflammation. Tubercles. Relative frequency of Scrofulous Disease in different Organs. Signs of the Strumous Diathesis.

WE have now, gentlemen, considered the phenomena of inflammation, local and general; its symptoms and its events; and the intimation of those events which is afforded by the state of the system at large; and we have surveyed the principal tissues of the body, and observed the modifications and peculiarities to which the process of inflammation is liable, according as it is situated in one of those tissues, or another.

In respect to this part of our subject—and indeed in respect to the whole subject of inflammation—I cannot too strongly recommend to you the diligent study of Mr. Paget's invaluable *Lectures on Surgical Pathology*.

There are still some varieties of inflammation; and some epithets applied to inflammation; which require to be explained.

Acute, and chronic, inflammation: these are words perpetually in our mouths. I have frequently employed them already. What do they mean? Is acute inflammation different from chronic in *kind*? No; they differ only in *degree*.

When the disease runs its course rapidly, and is attended with much general as well as local disturbance, it is said to be acute. When, on the other hand, the local and constitutional symptoms are less violent, and the inflammation runs a longer course, its phenomena following each other in slower succession, it is said to be chronic. The process is the same, but its features are less strongly expressed. The disease passes through similar stages in both cases, but it travels at a different pace. The characters, then, of acute inflammation are intensity of symptoms and rapidity of progress: and the characters of chronic inflammation are mildness of symptoms and slowness of progress. Inflammation can scarcely be very violent, and at the same time of very long duration. When violent, it has been likened (by Mr. Lawrence, whose language I have here adopted) to a blazing fire, which soon burns itself out. It may, however, be mild in its symptoms, and yet quickly over. The two terms acute and chronic are not directly opposed to each other: acute has more relation to the intensity, chronic to the duration of the disease; and some term is wanted—although it is hardly worth seeking for—to denote such a degree of inflammation as exists in a pimple: which is neither severe nor long-continued.

Now, in respect to intensity and duration, there are innumerable shades of difference in different cases of inflammation; and the same difficulty occurs here which always occurs when general terms are employed to express mere differences of degree. We feel no uncertainty or hesitation about those cases which occupy the two extremes of the scale; but with regard to those which lie in the middle we are often at a loss. To meet this difficulty some pathologists have invented a third epithet, viz., *sub-acute*, intending to designate thereby cases which hold an equivocal rank; which are neither decidedly acute nor plainly chronic; in which the inflammation may run a brief course, and be attended with a certain degree of fever; but attains no great intensity, works no profound changes, and does not require very energetic remedies to control it.

Do not suppose that, because chronic inflammation is attended with less tumult and disturbance, it is necessarily on that account less dangerous or less destructive than acute. The latter is commonly more obedient to the influence of remedies than the former; it is usually soon brought to an *end*: whereas chronic inflammation is often obstinate and abiding, and leads to very serious changes in the part upon which it fastens. Speaking generally, it tends to thicken and indurate when it is situated in the *interior* of organs, and to the effusion of pus when it affects *membranes*, or *surfaces*. It is more common in weakly and debilitated persons than in others; but you must not forget that such persons are also very liable to acute inflammation.

Chronic inflammation is not unfrequently a *sequel* of acute inflammation. And that the two differ merely in degree, and not in kind, is evident from this: that acute inflammation may sink or subside into chronic; and that, on the other hand, chronic inflammation may readily be aggravated into acute.

There is another, but less intelligible division of inflammation into *active* and *passive*. I believe that they who use the term passive inflammation, intend to signify by it that languid and sluggish kind of inflammation which is apt to occur under the same circumstances, and in the same conditions, with passive *congestion*. When the granulations of an ulcer are in that state in which they may be made brighter, smaller, and healthier, by the application of a stimulus: when the blood-vessels of the eye are left, after acute inflammation, turgid and tortuous; and that condition is improved, instead of being worsened, by the use of a stimulating lotion: in such cases as these, some persons would say there was passive inflammation. But I see little difference between this and chronic inflammation; nor do I know any difference between *active* and *acute* inflammation.

The term *latent* inflammation is one of modern introduction. It is applicable to those cases in which internal inflammation runs its course silently, treacherously, and unperceived; without the usual warning tokens of its presence; without its more striking and prominent signs. The smouldering fire is hidden from our view. Pneumonia, going on to disorganization of the lung, may arise, proceed, and even prove fatal, without any of the symptoms which ordinarily announce that disorder: without notable cough, or obvious dyspnoea, or complaint of pain, or the expectoration proper to pneumonia. And the same is true of other inflammations. We discover, with surprise and horror, the traces of their operation, when we come to examine our patient's dead body.

This is a most important form of inflammation; for though it does not declare itself to ordinary observation, neither does it occur absolutely without symptoms; but it requires that the symptoms should be *looked for*. The *auscultatory* signs of pneumonia, all those symptoms which are furnished by the physical condition of the affected organ, are present, and speak as clearly as in the more flagrant cases.

Latent inflammation is apt to creep on during the progress of certain disorders, whereby it is modified and masked. It belongs to those states of the system in which the sensibility is dull, and the vital powers languid. In continued fever not only have I known the lung pass into suppuration, when the existence of pneumonia had been unsuspected; but I even have seen one case in which that usually torturing accident, perforation of the bowel, took place, with the escape of its contents into the cavity of the abdomen, and extensive peritonitis—yet the patient expressed no sense of pain, and the inflammation was revealed, while he continued to live, by no intelligible symptom.

Inflammation of this insidious and lurking character is most to be apprehended in the aged, in those who are habitually intemperate, and in persons of sluggish temperament. It sometimes occurs during convalescence from acute diseases.

Besides the varieties which have been mentioned in degree, there are also differences in *kind* among inflammations. What I have been speaking of during the preceding lectures I have called common inflammation. It is the *most common* form in which that process displays itself. All persons are liable to it; and that again and again. None are at any time privileged from its attacks. But there are several forms of inflammation different from this, which are called *specific*. There are various forms of specific inflammation affecting the skin, discriminated from each other by the local appearances they exhibit, and by the constitutional disorder which attends them. The rash and the fever of measles are very unlike the rash and the fever of scarlatina; and both differ remarkably from those of small-pox, the eruption of which consists of little phlegmons. In each of these diseases the application of a specific poison is required for its production: and whereas common inflammation has a tendency, when once it has happened, to happen in the same part again—to *recur*—these forms of specific and contagious inflammation never, or almost never, occur more than once.

There is again the gouty inflammation—differing from common inflammation in several signal respects; in the production of chalk-stones; in its attacking those who are descended from ancestors who have had the disease, and scarcely any others. Then there is rheumatic inflammation, cousin-german to the gouty, yet distinguishable from

it. And another variety of inflammation is that which arises from the introduction of the syphilitic poison into the system.

Of the specific forms of inflammation now adverted to I shall speak when I come to consider gout and rheumatism, and the contagious exanthemata, as distinct diseases. But there is one variety of inflammation — I mean the *scrofulous* — which meets us on every side; and is apt to affect so many parts of the body, and so great a number of persons; and has so fatal a tendency in most cases, that it cannot be left out of the account that I have been desirous to give you of inflammation in general.

Scrofulous or strumous inflammation (for struma and scrofula are convertible terms) is a *slow* process; it falls therefore within the class of *chronic* inflammations. It is not attended with much pain, or heat, nor for some time with much change of colour; and the redness which does accompany it has often a livid or purplish tinge.

These, however, are the negative properties of merely chronic inflammation. But *suppuration* at length occurs, which also lasts long: and the pus formed is peculiar and characteristic; and by no means laudable. It is not homogeneous or smooth, but consists partly of a thin serous whey-like fluid, and partly of fragments of a substance resembling curd: and the ulceration that ensues is marked by corresponding peculiarities. The ulcers are indolent; show but little disposition to heal. Scrofulous inflammation, compared with common, or what is called healthy inflammation, is in general but little influenced by remedies.

Besides this scrofulous *inflammation*, it is necessary that I should now direct your attention to another form of disease, which is likewise properly denominated scrofulous. It is marked by the appearance, in various parts of the body, of what are called *tubercles*. These tubercles are masses of unorganized matter — also resembling curd or new cheese, more or less; but of various shapes and sizes. They suffer gradual changes; soften or break down; undergo a sort of suppuration; and the softer matter into which they thus (as it were) melt, has the characters that distinguish the pus of a scrofulous ulcer or abscess.

Now *tubercles* and scrofulous *inflammation* occur very continually in the *same individuals*: and what is remarkable, although they affect a very large portion of the whole human race, and conduce more often and more surely than any one thing else to shorten the natural period of human life, yet they belong, almost exclusively, to certain *classes* of persons. We can tell, beforehand, that such and such persons are likely to become affected with scrofulous inflammation, or with tubercles: and we say of those persons that they have the scrofulous *diathesis*. I will not positively affirm that these forms of disease *cannot* be produced in any or in all persons; but thus much is certain — that some persons are particularly prone to them: fall into them as it were spontaneously; or on the operation of very slight external causes; and even when all possible care is taken to prevent the operation of every ascertained cause; while other persons never show *any tendency* to scrofula, even when continually exposed in the same manner: or if they do become scrofulous at all, it is only when the external influences most favourable to the production of such disease have been intense in degree, and protracted in their application.

The occurrence of scrofulous *inflammation* in various parts constitutes distinct diseases; and the occurrence of *tubercles* in various organs, constitutes *other* diseases. It will facilitate our future inquiries into these several diseases, if I take this opportunity of stating to you what is known respecting the scrofulous diathesis generally: and of the modifications of inflammation which are determined by its presence.

A good deal of discrepancy, obscuring the whole subject, and puzzling the student, has existed — and I believe I may say still exists — among pathologists, as to the nature, and origin, and precise seat of tubercles, and as to the changes which they undergo.

In general they have been loosely described as being *round* masses of firm but friable matter, deposited in various parts of the body. Laennec, who paid great attention to tubercles, states that they are, at first, small, firm, greyish, semi-transparent bodies, which gradually enlarge and become opaque. In that condition he calls them *crude tubercles*. At length, after an indefinite period, these crude tubercles begin to grow soft in their centres, and are by degrees converted each into a liquid mass, having the consistence of cream. There is much of error in this description.

Andral, another great authority, says that tubercles are, in the outset, small, round,

opaque, yellowish bodies, unorganized, and of various degrees of consistence. He ascribes their softening (not to any spontaneous changes in their central parts, but) to the admixture of pus, poured out by the textures immediately surrounding the tubercle; which has irritated and inflamed those textures as any *other foreign body* might.

In some respects this statement is nearer the truth than Laennec's.

But in the account which I am about to give you, I shall chiefly follow our countryman, Sir Robert Carswell, the first Professor of Pathological Anatomy in University College; who is one of the latest, and, as I think, most satisfactory writers on the subject. His opinions were formed after a long and careful examination, for himself, of the parts infested by these tubercles. He devoted several years to the study of morbid anatomy, in Paris, where he made a very large collection of drawings, in which various diseased appearances are beautifully, and doubtless faithfully delineated. Some of these he has since published. I show you enlarged copies of those which relate to tubercle. They bear out some novel opinions which are stated in the letter-press that accompanies them.

After all, the points in question possess more of curious interest than of practical importance. But as you cannot help forming some notions respecting them, I think myself bound to lay before you those which most recommend themselves to my own judgment. At the same time you are to understand that I do not vouch for their absolute correctness.

Tubercles, then—or rather tubercular matter,—is deposited from the blood. Whether it is something totally new, something foreign to the natural materials of the body, introduced into the blood from without—or whether, as seems more likely, it is the result of some defect or error in the due elaboration of the blood itself—I cannot satisfy you. If, as has been supposed, the deposit be at first fluid, it afterwards becomes firmer, through the absorption of its more watery particles; and there then remains a “pale yellow or yellowish grey, opaque, unorganized substance.” This tubercular matter, so deposited, does not always assume a round form: far from it; the shape in which it appears depends upon the nature of the part wherein it is planted. It used to be held that the tubercular matter was always laid down in the areolar tissue. But Sir R. Carswell asserts that its most favourite seat (if one may so speak) is the *free surface of mucous membranes*. In whatever organ it is met with, if mucous tissue enter into the composition of that organ, that particular tissue is either (he says) exclusively affected, or much more extensively affected than any of the other component tissues. These remarks apply to the lungs, the alimentary canal, the liver, the urinary organs, and the organs of generation; but the presence of the tubercular matter is much more easily detected in the mucous tissue of some of these organs than in that of others. It is very conspicuous in the fallopian tubes and uterus.

But tubercular matter is often deposited on serous surfaces also; among which Sir R. Carswell includes the plates of the areolar tissue. It is even to be seen sometimes in the blood itself: not indeed while it is yet retained in its proper vessels, but when it is collected in the cells of the spleen. You know that the spongy texture of that organ allows the blood to accumulate in it in considerable quantity: and the tubercular matter may be seen forming in the blood at some distance from the walls of the cells in which the blood is contained. In one cell, according to Sir R. Carswell, you may perceive simply the blood coagulated: in another, it may be coagulated and deprived of its colouring matter: and in another, converted into a mass of solid fibrin, having in its centre a small nodule of tubercular matter.

Now when a speck or morsel of tubercular matter has been any where deposited, it is liable to increase. It grows larger by continued accretion; by additional deposits upon its surface. This being the case, we see plainly enough how it happens that tubercles assume different shapes, according as they occur in different parts. The round form which is so often observed is purely accidental. When a tubercle is deposited in the substance of the brain—and becomes larger by the repeated accession of fresh tubercular matter upon and around it—it naturally takes a spherical form, because there is nothing to limit its enlargement, except the soft cerebral matter itself, which presses it with equal force on every side. For the same reason tubercles deposited in the *areolar* tissue are globular. In like manner, if tubercular matter be laid

down in one of the pulmonary vesicles, so as to fill it up, it exhibits the roundish form of the vesicle. When it fills the cavity of a mucous follicle, it has a similar figure. But in the smaller bronchi it takes a more cylindrical arrangement. When (as often happens) it occupies one of these tubes, and also all the air-cells to which that tube leads, then we have twigs of tubercular matter, with cauliflower terminations. You see this depicted in the drawings before you. In the cavity of the uterus, and the fallopian tubes; in the infundibula and pelvis of the kidney, and in the ureters; and in the lacteal and lymphatic vessels; the tubercular matter is moulded to the forms of these parts respectively. We are more in the habit of examining tubercles in the lungs than anywhere else: and you will observe that in making sections of these organs, and looking only at the surfaces of those sections, we may easily overlook the branch-like disposition of the tubercular matter in the smaller bronchial tubes. We see the transverse section only of the tubes, which is necessarily more or less circular. On the surfaces of serous membranes, whether natural or adventitious, the tubercular matter will assume a rounded, or a lamellated form, according as the morbid secretion in which it originates has taken place from separate points, or from a continuous surface.

From what has now been stated, you will perceive that no alteration can be expected to take place in the tubercular matter after once it has been deposited, except through the agency of the parts around it and in contact with it. It is never organized, or capable of organization; and, consequently, no vital change in its consistence can originate in the tubercle itself. If any spontaneous change arise, it must be a chemical one: and of such we have no evidence at all.

It may therefore seem odd, that so accurate an observer as Laennec should have persuaded himself that the softening of tubercles begins in their centre.

Now Sir R. Carswell has given what appears to me a sufficient explanation of this mistake. Take the lungs; the morbid conditions of which were the most especial object of Laennec's investigations. The tubercular matter is effused (principally) upon the mucous surface: upon the inner lining of the air-cells, and of the bronchial tubes communicating with them. Yet it need not so accumulate as to *fill* these cavities; and it often does not; there is left a central vacuity, which contains mucus, or other secreted fluids: and if the lung be cut across under these circumstances, the divided air-vesicles will look like rings of tubercular matter grouped together; and each divided bronchial tube will present also the appearance of a tubercle, with a central depression, or soft central point. On the other hand, when the tubercular matter has completely filled and blocked up these cavities, both vesicles and bronchial tubes will look, when divided, like sections of round solid tubercles. These Laennec seems, in fact, to have regarded as *crude tubercles*: while he mistook the former appearances for tubercles which were beginning to soften in their centres.

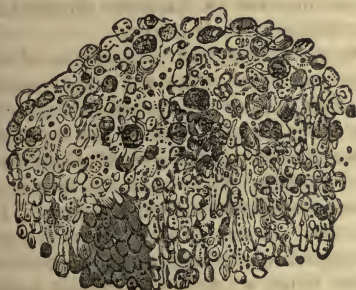
But you sometimes find large masses of tubercular matter in the lungs, or elsewhere: and in these masses you see that the process of softening is going on at several points, within the mass, at the same time. How is this to be explained? Why these large masses are formed, in fact, by the aggregation of many smaller masses, which, lying near each other, have coalesced as the deposit continued to increase: and the areolar and other tissues originally intervening between these coalescing masses at length suppurate; and by their suppuration, they soften, and gradually break down the tubercular matter which they enclose, and by which they are also enclosed. This is just the process by which tubercles are frequently expelled from the body. They increase till the surrounding parts take on inflammation, just as they might do if any foreign body exercised the same degree of pressure upon them. The inflammation thus excited is of the scrofulous kind; the thin pus which is thrown out pervades and loosens the tubercular matter; a process of ulceration goes on in the surrounding textures; and at length (supposing the lung to have been the seat of disease) the detritus of the tubercle is brought up, gradually, by coughing.

The account which I have now given you, and which I hope I have made intelligible, is, I think, extremely interesting—and much credit is due to Sir R. Carswell for having so greatly simplified our views of a subject which had previously been wrapped in profound obscurity. In no earlier writer, that I know of, is there to be found so complete and credible an explanation of the origin of tubercles; of the forms

they assume; of the phenomena attending their enlargement, and subsequent softening, and occasional expulsion.

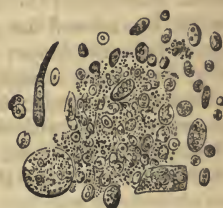
[According to Barthez and Rilliet, who have given the most complete and accurate description of tubercle, the various forms under which tubercle is found are the following:—the miliary or grey tubercle, the yellow or crude tubercle, the grey and yellow forms of infiltration, the gelatinous infiltration, and tuberculous dust. There is no reason for ascribing these forms of tuberculous matter solely to tubercle of the lung. As M. Valleix remarks, incontestible proofs may be adduced to prove that the grey granulations of Laennec occur in all the organs, and as the researches in pathological anatomy have been more carefully conducted, these proofs have been multiplied. M. Papavoine, in his interesting memoir "*On Tubercle considered particularly in Children*," expresses himself thus:—"We cannot admit the seat of the grey granulations to be only in the pulmonary vesicles: forms of alteration exactly similar, are to be met with in the lymphatic glands, in the liver, in the spleen, and on the serous membranes, especially in certain cases of general and acute tuberculization." The statement of M. Nelaton leaves no doubt of the development of grey semitransparent granulations in the osseous tissue. He has been able, repeatedly, to determine that the tubercles of the bones, like those of the lungs, recognise for points of departure, the grey semitransparent granulations described by Laennec, Louis, and others (*Recherches sur l'affection tuberculeuse des os*, 1836). Dr. Glover has observed, in

FIG. 13.



Yellow tubercle; crude mass.

FIG. 14.



Grey tubercle; miliary granulation.

FIG. 15.



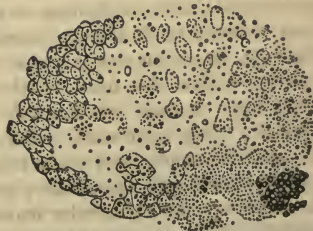
Tubercle-corpuscles from the peritoneum. a, the same, after the addition of acetic acid.

FIG. 17.



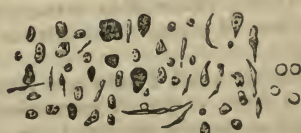
Tubercle-corpuscles, from a mesenteric gland.

FIG. 16.



Tubercle-corpuscles, granules, and molecules, from a soft tubercular mass in the lung. 250 diameters linear.

FIG. 18.



Isolated tubercle-corpuscles. On the right are four blood-globules.

granular meningitis, the forms of grey granulation, and yellow particles, answering very well to the yellow points which appear in the grey granulations of the lungs. The miliary tubercle may exist in all the organs, and it is in the form of grey infiltrated matter, granular to the microscope, more or less diffused through the substance of a gland, that we detect the first occurrence of mesenteric tubercle; afterwards we find a more crude or yellow appearance of the tubercle matter, as in the lungs. The appearance of infiltrated grey matter is especially marked in effusions organized between the tunics of the intestinal canal. Some of the illustrations presented by Dr. Glover (*Pathology and Treatment of Scrofula*) show the tubercular effusion in a mesenteric gland:—1stly, in a diffused form throughout the hypertrophied tissue of the organ; 2dly, forming striæ and patches, varying in hue from grey to yellow; 3dly, in cysts filled either with a tuberculous powder or with a curdy matter; 4thly, in masses of lardaceous consistence, implicating either the whole gland, or more or less of its structure. We have specimens, likewise, remarks the same author, of bronchial glands, and bronchi sprinkled over, as it were, with a tuberculous powder, and studded with cretaceous particles.

"The grey granulation," as Rilliet and Barthez observe, "exists in all the organs, not only in the intestines, peritoneum, and pleura, but in the spleen, the liver, the kidneys, the lymphatic glands and cerebral meninges."—(*Maladies des Enfants*.) In fact all the forms of tubercle which occur in the different organs are brought about chiefly by mechanical causes, and differ very slightly in a physiological sense, never in their more minute anatomy. Thus, for example, the liver is an organ, in which tumours generally are of large size, and of various and irregular shapes; in the lungs, the air-cells and the membranous character of the tissue tend much, if the effusion be not rapid, to surround it, while progressing and stiffening, with envelops of a membranous nature. The brain, from its structure, must oppose pressure in every direction in which the progress of a deposit in its interior can take place, and thus we find tubercles of the brain generally of small size and regularly circumscribed. Between layers of membrane we find the deposit stretched out in flakes. In the sub-cutaneous cellular tissue it forms irregular masses. On the free surfaces of mucous or serous membranes its figure is irregular, or it occurs in superficial layers. In the bones, in general, it is in little round granules, or in very circumscribed masses.—C.]

These processes—of softening, produced by surrounding inflammation, and of ultimate expulsion—may be regarded as a natural mode of cure. Such a cure is in truth sometimes accomplished. A serofulous abscess forms in the glands of the neck: and pus and tubercular matter are discharged. At length the ulcer heals, and no trace of the diseased process remains, beyond a scar. The same thing takes place also in the lungs; and, if there have been only one or two masses of tubercle deposited, the patient may thus get quite well: but unfortunately, as the serofulous matter is extirpated from one part of the lung, it is apt to be multiplied in another, till at length we have death by hectic, and all its melancholy accompaniments.

But I am desirous of pointing out to you another way in which tubercular disease may be said to be cured by a natural process. And this also has been better described by Sir R. Carswell than by any preceding writer. One form of serofulous disease, exceedingly common too, especially among children, is what is called "tabes mesenterica." Tabes and phthisis, the one a Latin and the other a Greek word, signify, I need scarcely tell you, the same thing: a wasting away, or a consuming: and phthisis is applied to the same disease in the chest, to which tabes is applied in the belly. The common English word is consumption; and we might very well speak of thoracic consumption, and of abdominal consumption; but the technical name of the latter complaint is tabes mesenterica. This is not only a very common but a very fatal disease in children and young persons. The glands of the mesentery enlarge and become charged with tubercular matter: but they very rarely suppurate. Their enlargement is commonly connected with serofulous disease and ulceration of the mucous follicles of the intestines; and the little patients die, because the lacteals are no longer able to take up from the food a sufficient supply of nutriment: they die starved. But some few do recover from tabes mesenterica. Sir R. Carswell relates an interesting case in which such recovery took place, and in which he had an opportunity of examining the glands at a subsequent period. He says, "The patient, who when a

child had been affected with *tabes mesenterica*, and also with swellings of the cervical glands, some of which ulcerated, died at the age of 21, of inflammation of the uterus, seven days after delivery. Several of the mesenteric glands contained a dry cheesy matter, mixed with a chalky-looking substance; others were composed of a cretaceous substance; and a tumour, as large as a hen's egg, included within the folds of the peritoneum, and which appeared to be the remains of a large agglomerated mass of glands, was filled with a substance, resembling a mixture of putty and dried mortar, moistened with a small quantity of serosity. In the neck, and immediately beneath an old cicatrix in the skin, there were two glands containing in several points of their substance (which was otherwise healthy), small masses of hard cretaceous matter."

Now what Sir R. Carswell here saw in the mesentery and in the neck, is what sometimes occurs in other parts of the body: in the lungs; and particularly in the bronchial glands at their root, and about the bifurcation of the trachea. From these situations, the hard chalky matter left by the absorption of all the more watery part of the morbid deposit, and by the concretion of its earthy salts, is often coughed up. But it may remain, when the tubercles are few, and there is no tendency to their increase, for years, as an inert, and almost harmless mass.

I mentioned just now that the secretion or separation of the matter of tubercle from the blood takes place, by preference, upon the free surface of mucous membranes, and very frequently also upon the surface of serous tissues, including the areolar.

It may not be uninteresting to inquire into the relative frequency of scrofulous disease in different organs, or in different parts of the same organ. The facts which we possess on this head afford us very valuable assistance sometimes in respect of diagnosis.

During the periods of childhood and youth, the lymphatic glands are exceedingly prone to scrofulous inflammation: especially the mesenteric and the cervical glands. But in adult age tubercles are, beyond all comparison, most frequent in the *respiratory organs*; and they occupy the summit of the lung much more commonly and thickly than any other part. The superior and posterior portion of the upper lobe is the spot in which, if any tubercles at all exist in the lung, they are almost sure to be found. It is here also that they first begin to suppurate or soften. This law has long been well known: and so constant is it, that Sir R. Carswell holds the formation of tubercles in any other portions of the lung to be always of secondary occurrence. He declares it to be the result of his experience (and few persons can have had more opportunities of examining diseased lungs), that there is no deviation from this rule, except when some other portion of the lung may have been the seat of an inflammatory attack, which has determined the priority of tubercular disease in that portion. We shall see hereafter what a very important bearing a knowledge of this law has, in settling the nature of a complaint which might, without it, be doubtful.

Scrofulous ulceration of the larynx, or trachea, when it occurs, is usually the concomitant of tubercular deposits in the lungs.

Next, tubercular or strumous disease is exceedingly common in the digestive organs: most of all in the mucous follicles of the small intestines; both in those follicles which are separate, and are called *glandulæ solitariae*; and in those which are collected into roundish or oblong groups, the *glandulæ agminatæ*. It is secondarily to these affections of the follicles, in many cases at least, that the glands of the mesentery become implicated. Tubercular deposits are frequent also in the solitary glands belonging to the cæcum. The ulceration which follows the evacuation of the strumous matter from these parts gives the interior of the bowel an appearance somewhat resembling that of a moth-eaten garment. Tubercular matter is seldom deposited in any other parts of the intestines, great or small, than those which I have mentioned. Sir R. Carswell supposes that it may often be secreted upon the free surface of the membrane, but that, not being entangled or confined in any mucous crypt, it is removed as soon as it forms. It is not often that scrofulous tubercles are found in the liver of adults: they are not very uncommon in that organ in children, but even then they are few in number and small in size. It is a curious fact that they are much more frequently seen in the spleen also in children, than in grown up persons. The uterus, the testicle, the prostate gland, are all liable to them: they are common enough upon the surface of the peritoneum.

In the nervous system tubercles are by no means unfrequent: they are met with oftener in the brain than in the spinal cord. That fearful disorder of childhood, known by the name of hydrocephalus, occurs principally, if not altogether, in connexion with the scrofulous diathesis.

Strumous deposits are rare in the organs of circulation. Tubercles have been seen, I believe, in the muscular substance of the heart: but this must be a very uncommon thing. Scrofulous disease is not at all unfrequent in bone, especially in the bodies of the vertebræ, and in the spongy extremities of the long bones.

It is very seldom indeed that scrofulous tubercles occur in any one organ only. Almost always they are met with in at least two, and frequently in all the parts at once which are liable to be infested by them. Sometimes the lungs alone are affected; but generally both the lungs and the intestines are occupied by the disease. It has been affirmed by a great living pathologist, M. Louis, that if you find tubercles in any other organ, you are sure to find them also, and in greater number, and further advanced, in the lungs. But this, though true as a general rule, is not without exceptions. I have seen the peritoneum crowded with myriads of these tubercles, when the most careful examination could not detect a single one in the lungs. And similar examples have fallen under Sir R. Carswell's observation.

[The general diffusion of tubercular matter is much more common in children than in adults. Thus, in 358 cases where tubercles existed in the lungs in adults, M. Louis notices the existence of tubercular matter in the brain or its membranes only *once*; in the bronchial glands it was detected in about *one-fifth* of the cases; in the mesenteric glands, in *one-fifth*; in the liver, only *twice*; in the kidneys, *five times* in one hundred and seventy cases; on the other hand, ulceration of the larynx existed in *one-fourth*; ulceration of the bowels, in *five-sixths* of the cases. In 180 cases in which tubercles of the lungs existed in children, Dr. Green found the brain to be affected with tubercles in *one-ninth* of the cases; the bronchial glands, in 100 out of 112; the mesenteric glands were tuberculous, in *one-half*; the liver, in *one-ninth*; the kidneys in *one-eighteenth* of the cases; but ulceration of the larynx occurred only *once*, and ulceration of the bowels, *sixteen times* in 112 cases.

M. Cless, of Stuttgard, has also published the results obtained from the examination of upwards of 180 bodies affected with tubercular disease. In 152 examinations of adults, M. Cless found the lungs free from tubercles *six times*. In 21 examinations of children, he only found the lungs free from tubercles *once*. This was in a boy eleven years of age, who, besides a considerable serous effusion into the ventricles of the brain, had two large masses of tubercle in the cerebellum, many small ones on the surface of the liver, and caries of the vertebræ.

In 146 adults affected with tubercles in the lungs, there were only *thirty-five* in whom the disease was confined exclusively to the lungs. In children there were only *three* cases out of *twenty* in which *all* the other organs were free. M. Cless never found the bronchial glands in children affected with tubercular deposit without the existence of tubercles in the lungs also. In *thirteen* adults and *one* child, there were tubercles in the pleura. In *sixty-one* adults, and *four* children, the tubercles were limited to the peritoneum *eight times*. The four children were between six months and ten years of age. In 152 adults affected with tubercles, the small intestines were affected *eighty-three times*, and the large intestines *thirty-seven times*, and in *twenty-one* children, the small intestines were affected *seven times*, the large ones only *once*. Among 152 adults, *thirty-two* had tubercles of the mesenteric glands, while they occurred in these glands in *seven* out of *twenty-one* children. In all the cases tubercles were found in the other organs. Tubercles of the liver occurred *once* in an adult, *twice* in children, while other organs were also affected. In *four* adults, and *twelve* children, the spleen was affected with tubercles, these at the same time existing in other parts of the body. In children, M. Cless remarks, the parenchyma of the spleen is often completely invaded by tubercles. In the kidneys, tubercles were met with *four times* in adults, and *three times* in children; of *five* children, aged from eight months to eleven years, in whom the membranes of the brain presented tubercles, four died of acute hydrocephalus. In all these there were tubercles in the lungs and other organs also. The tubercular granulations had always their seat on the external surface of the arachnoid, between this membrane and the pia mater, never

within the cavity of the arachnoid. In *twenty-seven* children who died from tubercles, *four* had tubercles of the brain, as well as in other organs; M. Cless never found any in the brain of adults. Besides their existence in the mesenteric and bronchial glands, M. Cless found tubercles in the glands of the neck in *five* adults, and *one* child. See *Condie on Diseases of Children*, 4th edition.—C.]

The question has been much and eagerly discussed, whether the deposition of tubercular matter be not, what I should call, an *event* of inflammation. Some persons have strenuously argued that the curd-like substance is nothing more than a particular kind of vitiated or imperfect lymph, and that it is never poured out except as a consequence of inflammation; and they cite cases of patients who always had enjoyed good health, until inflammation was accidentally excited in their lungs; immediately after which the well-known signs of phthisis began to display themselves; and, after death, the lungs were found full of tubercles. But they forget to take into the account another fact equally well established, viz., that tubercles are found, in great abundance, in the lungs of persons who were never known, in their lives, to have any functional disturbance of those organs; and whose lungs present, after death, no other traces of having been inflamed. We even find tubercles in the lungs of unborn children. Not that this is conclusive; for inflammation does sometimes attack the fœtus in utero, and leave permanent and unequivocal traces of its action.

Moreover, inflammation continually happens, in all the component textures of the lung, in the forms of bronchitis, pneumonia, and pleurisy, without the subsequent development of tubercles. I admit that this fact, to be of weight, should be proved of persons who possess the scrofulous diathesis; and I believe the proof might be found: but the search for it would require much carefulness and candour.

In my own opinion, there is not a shadow of evidence to show that the deposit of tubercular matter is always and necessarily preceded by inflammation. Yet an undoubted and most important *connexion* obtains between the occurrence of inflammation and the occurrence of tubercles. Tubercles will cause inflammation, and inflammation will determine the development of tubercles. The enlarging tubercles excite inflammation in the surrounding textures by the pressure they exert upon them; and probably in other ways; by mechanically interfering with the healthy circulation of the blood, for example: and the inflammation lit up is usually of the scrofulous kind; it is slow, and partial, and easily *quieted* by treatment, though scarcely to be *cured*. On the other hand, there are numerous facts to prove that, in a person having the scrofulous diathesis, the occurrence of inflammation within the chest may rouse that previously dormant tendency into action, and become the exciting cause of the secretion or separation of tubercular matter from the blood. The cases in which other parts of the lung than the apex are found exclusively occupied with tubercles, are also cases in which, apparently, the same parts had been the seat of inflammatory action: of which we sometimes see other traces, in adhesions of the neighbouring pleura.

The connexion between tubercles and inflammation is shown also by their occurrence in the substance of false membranes. And the same phenomenon marks the fact that they are something distinct and different from coagulable lymph.

You must not suppose, from anything I have said, that persons of the scrofulous habit are not susceptible of *common* inflammation: we know that they are, by the readiness with which slight injuries often heal in such persons; but there is always much reason to apprehend that inflammation occurring in them will take on the scrofulous form; become chronic, if it were not so at first, suppurate tardily, and produce that unhealthy kind of puriform secretion which is characteristic of strumous disease.

Another question relating to tubercular diseases is, whether they are *contagious*. capable, *i. e.*, of being communicated from one individual to another. The general belief, in this country, is that they are not. Indeed, their very dependence upon a peculiar diathesis would seem to disprove the supposition. Yet some practitioners, even here, have, I know, misgivings on the subject; and in some parts of the continent, in Italy particularly, consumptive patients are shunned, from the persuasion that their complaint is infectious. I shall revive this question when I speak of phthisis hereafter.

I have stated that scrofulous disease appears, almost exclusively, in certain *classes* of persons, of whom, therefore, we say, that they have the *scrofulous diathesis*.

It is both interesting and useful to be able to distinguish those in whom the scrofulous habit of body, or the predisposition to strumous disease, exists.

Now there are certain physical and moral characters which teach us to apprehend the existence of a tendency to scrofulous disease, even when there has not, hitherto, been any local manifestation of such disease.

Again, we infer the scrofulous diathesis, in many persons, from knowing that scrofula has existed among their progenitors.

On these two points I have a few observations to make: and first, on what may be considered the external tokens of a scrofulous constitution.

The persons, in whom scrofulous disease is *most* apt to declare itself, are marked, during childhood, by pale and pasty complexions, large heads, narrow chets, protuberant bellies, soft and flabby muscles, and a languid and feeble circulation. They present many of the features belonging to that pattern of body which is denominated the leucophlegmatic. But the strumous disposition very often indeed accompanies a variety of the sanguine temperament also; and is indicated by light or red hair, grey or blue eyes with large and sluggish pupils and long silky lashes, a fair transparent brilliancy of skin, and rosy cheeks. This red colour, which is well defined in general, is easily changed, however, by cold, to purple or livid; the skin is thin and readily irritated; the sclerotic has often a peculiar pearly lustre; and the extremities are subject to chilblains. Such children are, many of them, extremely clever and ready of apprehension, of eager tempers, and warm affections, lively, ardent, imaginative, and susceptible. This precocity of mind and intellect, while it delights the fondness of the parent, awakens the fears of the more far-seeing physician.

But the disposition to scrofula is by no means confined to persons of the serous or of the sanguine temperament. It is frequent, though less common, in what has been called the melancholic or bilious temperament; in persons of dark muddy complexion and harsh skin; in whom the mental and bodily energies are more sluggish and dull. And it is remarked that in persons of this cast, scrofula, when it does occur, is even more than usually obstinate and intractable.

Scrofula does often indeed appear in persons who exhibit none of those signs of a strumous disposition which I have been enumerating; but it is *more likely* to appear, *cæteris paribus*, where those signs are observed.

There are several alleged marks of a scrofulous *diathesis*, which are, in fact, instances of scrofulous *disease*. Such, for example, is that chronic lippitudo, which so frequently disfigures strumous children, rendering them what is called blear-eyed: and chronic inflammation of the conjunctiva, lasting long, without much redness or heat, and with extreme impatience of light, and a tendency to form little pustules near the edge of the cornea. The tumid and chapped upper lip; the redness and swelling of the columna nasi, and lower parts of the nostrils, so common in children, especially during winter, are early fruits of the strumous taint. Certain maladies of the joints, what are popularly called white swellings, are instances of scrofulous disease. So may perhaps rickets be considered; at any rate, ricketty children are very often affected with scrofula also. Moist eruptions behind the ears; chronic enlargement of the glands of the neck; that slow, eating, ulceration of the nares, termed lupus; may all be included within the class of strumous disorders.

When any one of these scrofulous affections has once shown itself in any person, we know, by that circumstance, that he possesses the strumous constitution; and we look for the recurrence of his complaint in the same part, or in other parts.

In a former lecture I mentioned scrofula as one of those distempers the hereditary tendency to which is indisputable. The scrofulous *diathesis* is hereditary: and sometimes scrofulous *disease* is so too. I have seen lungs, taken from the body of a fetus, stuffed with tubercles. There were some fine examples of this in Mr. Langstaff's museum, in the city. We have, therefore, in respect to scrofula, the rare conjunction of congenital disease, and hereditary disposition. I need not repeat here the remarks I made before, respecting hereditary diseases in general. No one, of the least observation, can doubt that the disposition to consumption is very often transmitted from parent to child. We see whole families swept away by its ravages. Like other hereditary tendencies, it may skip over one or two generations, and reappear in the

next, just as family likenesses are known to do. There are other families in which you can trace no such predisposition; but such families are perhaps few. A little leaven is sufficient, sometimes, effectually to taint a whole pedigree. The tendency, however, exists in various degrees. It may be so strong that no care, no favourable combination of circumstances, will prevent its local manifestation; and it may be so faint that it would never break out into actual mischief if the exciting causes of scrofulous disease could be warded off. It is important, therefore, to know what these *exciting causes* are.

"They may all be ranked together (to use the language of Dr. Alison) as causes of *debility*, acting permanently, or habitually for a length of time, although not so powerfully as to produce sudden or violent effects."

The circumstances to which, acting separately or in combination, we most confidently ascribe the power of developing scrofula, are insufficient nutriment, exposure to wet and cold, impurity of the atmosphere, the want of natural exercise, and mental disquietude. To estimate the separate effect of each of these causes may be difficult; but their combined influence is unquestionable.

There can be no doubt that improper diet, or rather imperfect nourishment, is one main exciting cause of scrofulous disease. Yet of this it is not an easy thing to obtain evidence, which shall be entirely free from fallacy. The disease occurs very often among the poor; but then it very often occurs also in the families of the rich. There is one fact which has always struck me as very instructive and convincing on this point. Infants at the breast, supplied with good milk, and with plenty of it, seldom show any signs of scrofulous disorder: whereas, as soon as they are weaned, they become subject to various complaints of a strumous kind. When an unweaned child is brought to us with ophthalmia, we expect almost always to discover inflammation of the common and acute kind; the purulent eye. In nine children out of ten who come after weaning, we look for and find some form of scrofulous inflammation, such as pustular ophthalmia.

Mr. Phillips (*Scrofula, its nature and causes*), has succeeded in establishing very conclusively the influence of bad and deficient food, and of an excess of vegetable aliment, in the production of scrofula. — C.]

The greater prevalence of scrofulous disease among the poor may be ascribed, in great measure, to their frequent exposure to wet and cold.

[Barthez and Rilliet were only able to find *two* cases out of 314 tuberculous children, where exposure to humidity appeared to be the sole cause of the tubercles. The researches of Mr. Phillips would tend also to prove that the influence of humidity has been much overrated. — C.]

Scrofula seldom breaks out in the mild and dry weather of summer. The influence of climate in fostering or repressing the disease is notorious. There is no climate in which it flourishes more than in our own. Consumption is called, in some parts of the continent, the English disease. Persons who migrate from this country to warmer and more equable climates, seldom become scrofulous; nay, it very often happens that the incipient indications of strumous disease are completely arrested or quieted by the change. Phthisical patients, much troubled by symptoms here, are sometimes so thoroughly freed from them soon after their arrival in Madeira, as to be deceived into the belief that their case had been mistaken. They think themselves well. A return to this country undeceives them. The native inhabitants of hot regions are by no means, however, exempt from struma, in any of its forms. When they come into these latitudes they are more subject to scrofula than we ourselves are. And the same effect of climate is very distinctly visible in the lower animals. The physicians in ordinary to the inmates of the Zoological Gardens will tell you that the beasts and birds which are brought hither from warm latitudes perish in great numbers from scrofulous diseases. John Hunter observed this long ago in respect to monkeys.

Of the debilitating influence of impure air I spoke in a previous lecture. That it promotes the evolution of scrofulous disorders we have proof, on a large scale, in the great mortality produced by such disorders among the lower classes in large cities as

compared with agricultural districts. The per-centage of deaths from consumption, hydrocephalus, and various other diseases which spring from a strumous habit, is much greater in London than in the country. Even in individual cases this influence is too manifest to be overlooked or mistaken. It is impossible to question the beneficial effect, upon children afflicted with scrofula, of a removal from London to the sea-coast.

I said, when I first began to speak to you of inflammation, that it was the only disease which we were able to excite at will: that we could cause inflammation, in various ways, whenever we desired to do so; but that to make a cancer or a tubercle was beyond our power. Now in strictness of language, and in the practical meaning of these words, this assertion is quite true. It is certainly true as respects *exciting* causes; it is probably true as respects *predisposing* causes also. But the latter of these propositions has been denied, and experiments have been appealed to in disproof of its correctness. It has been said that, by so arranging external influences as to heighten and concentrate their hurtful tendencies, tubercular disease may, in the lower animals at least, be engendered. It is affirmed that tubercles may be produced at will in the liver of a rabbit, by shutting the animal up in a cold, damp, dark, and narrow place, and feeding it upon food not natural or suited to it. Mr. Simon has shown the strong probability that all such experiments have been fallacious. A liver, occupied with so-called tubercles, thus produced, was brought to him for examination. He found that the supposed tubercles were really minute oblong eggs, or larvæ, of some entozoon. He then made many experiments himself, to try if he could create tubercle artificially. He dissected also many rabbits on which no experiments had been made. The result was, that he never saw a tubercle in a rabbit. It may be questioned whether the animal be susceptible of that form of disease.

I have lately met with the following curious statement, bearing upon this question, as it regards the human animal:—"According to the very trustworthy report of Dr. Schleusner, who was sent by the Danish Government a few years since to investigate the sanitary condition of Iceland, no combination of what are commonly accounted the predisposing causes of consumption and scrofula could be more complete than that which exists among the mass of the Icelandic peasantry. Whole families are huddled up with their sheep, not only during the night but during the greater part of the day, for half the year, in most miserable hovels, destitute of any ventilation but that afforded by the chimney. Their clothing is not once put off or changed during the whole of that time; their food is scanty; and the external atmosphere is both cold and damp. The unhealthy condition of the population is evidenced by its extraordinary liability to epidemic disorders; and by its want of increase, or even in some districts by its absolute diminution. And yet amongst this remarkable people, the best-educated peasantry in Europe so far as regards what is commonly accounted education, scrofula and consumption are unknown."

The writer (in the *Westminster Review*) ascribes this immunity "to the highly oleaginous nature of their diet, which consists in great part of the oily bodies of piscivorous birds." A more likely cause, to my mind, for such immunity, if it really exist in that race of men, is their probable exemption from the scrofulous *diathesis*.

Of course, no experiments like those practised upon the rabbits can be purposely made upon a healthy man; but accidental opportunities arise of witnessing an approach to a similar trial of the human species. Instances are recorded of persons, previously well (but having probably the strumous diathesis), becoming affected with scrofula after being confined in the dungeons of a prison, and there scantily fed.

Something of this kind I have, very recently, had the opportunity of seeing.

A number of male prisoners, chiefly young men, began to exhibit glandular swellings of the neck, after incarceration for some length of time in the Penitentiary at Milbank. The circumstances of their health led to a relaxation of their punishment. Instead of being kept in solitary confinement in a coldish cell, and on the prison diet, they were permitted to work, for several hours daily, in each other's company, in the garden of the establishment. Some porter was at the same time given them, and their allowance of meat was increased. The improvement in their condition was rapid and striking. Here we have the disorder germinating under one state of external circumstances, and checked immediately under the opposite state.

If you consider the way of life of the children of the poorer classes in this metro-

polis, and in our large manufacturing towns, you will find that they are much exposed to most of those injurious influences, the combination of which has been thought likely to generate tubercles. They live usually in an atmosphere made stagnant by narrow streets; and in small, crowded, ill-ventilated, and dark rooms in those narrow streets. The stagnant atmosphere is contaminated in a thousand ways. They are very insufficiently protected from transitions of temperature, against cold and wet, by their clothing. They are commonly ill fed — their diet being frequently scanty, and generally of a kind quite unsuited to their growing years. We need not be surprised, therefore, at the ravages which scrofula, in its manifold shapes, makes among the children of the poor in large and populous towns. If ever scrofula be generated, it is in them. But in most cases I believe it is the latent disposition that is called into action. Moderate exercise, in pure air, and in the open daylight, with suitable nourishment, sufficient clothing, and attention to the state of the bowels: these circumstances comprise nearly all that we can attempt, in a given climate, towards preventing the development of struma: and from each and all of them many of these poor children are habitually debarred.

LECTURE XIII.

Cancer: its Species or Varieties. Scirrhus; Encephaloid Cancer; Colloid Cancer, Its mode of Growth and Dissemination. Habitudes of the several Varieties. Treatment of Inflammation. Antiphlogistic Regimen. Blood-letting.

I HAVE more than once coupled cancer and tubercle in the same sentence. Though very different in many respects, they are alike in their intractable character and destructive tendencies. Of the two, cancer, while it is happily much the more rare, is also much the more painful, loathsome, and hideous in its consequences. It is to cancerous diseases that the epithet *malignant* especially belongs. Not resulting from any change in the natural textures of the body, but constituting an addition to them, and therefore assuming, usually, the shape of tumours, they are commonly and correctly spoken of as cancerous *growths*. But there are other growths which, by comparison, are *innocent*; which do not imply any necessary destruction of contiguous parts, nor any inevitable danger to life, nor even any marked deterioration of the general health. Such are certain fatty tumours, and fibrous tumours, and osseous tumours. All these last, as their names denote, resemble in their sensible qualities some one of the healthy and natural textures. They have accordingly been styled *analogous*, or *homologous* growths; while cancer and tubercle, which find no counterparts in the sound body, are said to be *heterologous*. Some varieties of cancer are, however, very similar in outward appearance to the substance of the brain; and microscopic observers say that in their minute and original structure there is no perceptible distinction between the most innocent and the most malignant growths; nay, that both agree in their primary corpuscular elements with the healthy tissues of animals, and even of plants.

[This statement is not perfectly accurate. The microscope has not, it is true, thrown much light upon the nature and causes of morbid growths; it has, however, shown that in many of the particulars of their intimate structure, they not only differ from the healthy tissues of the body, but that they differ in this respect from each other.

That such is the case, is rendered evident by the result of all the more recent researches into the intimate structure of *cancerous formations*.

By Müller and other pathologists, cancerous formations have been arranged in two

great families or groups—the *encephaloid* and the *scirrhus*.—Of the first there are three subdivisions.

1. *Medullary Carcinoma*; in which there is a predominance in the medullary mass, of *round globules* over loose fibrous tissue. The globules are of various sizes; but the smallest are larger than pus-corpuscles. Each contains a granular substance or nucleus within. They are very similar, in many respects, to those of common cancer, and of reticulated carcinoma or scirrhus.

2. *Medullary Carcinoma*, consisting of pale, *elliptic, non-elongated corpuscles*, and of a fundamental cerebriform mass. These corpuscles are usually twice or three times as large as the globules of the blood. There is never any appearance of fibres proceeding from their surface, and they rarely exhibit any traces of nuclei within them.

3. *Medullary Carcinoma*, with *fibrated or puriform corpuscles*. This species of encephaloid structure has, at times, on laceration, a sort of fibrous aspect, when the puriform corpuscles are arranged in a somewhat determinate direction; according to which the morbid mass will present a radiated or a tufted appearance. In many cases their directions are so various that the lacerated surface exhibits no traces of fibres anywhere. The puriform corpuscles are sometimes nucleated, at others they contain granular points, but without distinct nuclei. They are elongated, on one or two sides, into fibres of different lengths. They may be considered as cells that are arrested at the period of transition from the cellular to the fibrous condition.

The three forms of disease now described, may, most probably, be regarded as so many degrees or stages in the development of the same tissue; these successive stages being characterized, 1, by rounded nucleated globules; 2, by elongated oviform globules, which are either non-nucleated or indistinctly so; and 3, by puriform globules.

These several kinds of globules may be regarded as so many successive epochs of evolution through which *a cell* must pass before it can become a *fibre*. Thus we find, it is true, that in an encephaloid mass there is the same transformation of the primitive elements, as occurs in many normal tissues—with this difference only, that the process of evolution is not complete—being arrested before the fibrin is perfectly formed.

The essential element of an encephaloid tumour is the presence of cells. In some cases the entire mass is composed of them, placed one alongside of the other, but without having any perceptible bond of union, while in others there is a network of fibrous or cellular tissue interposed between the cells. When the fibrous tissue prevails, the encephaloid then approaches in character to the scirrhus structure. In the latter the existence of the two elements, cells and fibres, is always more distinctly marked than in the former. The fibres are often quite perceptible to the naked eye. Sometimes they are lengthened, and run parallel to each other; at others, they form rounded capsules, within which the globules are contained. As in the case of the newly-formed fibres of the cellular tissue, so those of a scirrhus formation are destroyed by acetic acid, leaving nuclei or nucleated fibres behind. The fibres sometimes exhibit, at different points, a sort of varicose enlargement, within each of which a nucleus is found. This appearance is often observed in fibrous tumours—not genuine scirrhus—of the uterus and other parts.

In the *reticular carcinoma* of Müller, the white network which encloses the scirrhus globules in its meshes, is formed of round, opaque granulations, three or four times as large as the blood globules; they are, occasionally, agglomerated into rounded masses. The *genuine scirrhus tissue*, of a pale greyish colour, is composed of globules that, on the whole, resemble those of the first stage of an encephaloid formation. These globules are either round or somewhat oval; along with them, according to Vogel, we find free *nuclei* with their *nucleoli*.

From a variety of observations, it may be reasonably concluded that the cells of scirrhus are formed around the nuclei of which M. Vogel speaks; their contents are at first granular and almost opaque. When the process of softening commences, the granulations disappear, the globules become transparent, and within them are formed new cells, which at first are few in number, and gradually multiply, until they entirely fill the parent cell. M. Valentin, who, in part at least, admits this account of the progress of the cell, declares, that the parent cells eventually burst and discharge their cellules; we may thus account for the presence of young free cells in scirrhus formations that have become softened.

The inter-cellular substance seems to undergo certain modifications corresponding with the evolution of the cells; the granulations or granular points which it often contains, usually disappear, and it becomes limpid, while, at the same time, the space which it occupies is diminished by the enlargement and multiplication of the cells.

The fibrous network does not appear to follow, in its alterations, the development of the cells: it may remain firm and resisting while the cells are far advanced in their evolution. Even when a scirrhus tumour has become completely softened, this tissue sometimes forms shreds that retain their original character.

In *alveolar cancer*, the basis of the morbid tissue consists of white fibres and lamellæ, which cross and intercross with each other, containing, between the meshes thus formed, limpid cells, either closed or communicating with each other, of various sizes, from that of a grain of sand to that of a large pea, and filled with a transparent, gelatinous substance. In this substance there are cells, which contain other cells more minute. The smallest of these cells exhibit, at one point of their parietes, a distinct dark yellowish nucleus, and sometimes, also, many free and unattached granules floating within them. To this species Müller refers the *gelatiniform* and *alveolar cancers* of Laennec and Cruveilhier. The cells of this species of the disease appear to be only an advanced or more mature degree of the cells of scirrhus.—C.]

This very agreement, if it really be so complete, shows that in *classifying* morbid growths we must reject the aid of the microscope, and attend to their grosser and more palpable features. And, inasmuch as cancerous formations have, by some pathologists, been ascribed (very erroneously, in my opinion) to *inflammation* as their cause, I shall scarcely be going out of my way if I state here some of the broad facts which have been ascertained upon this very interesting subject.

Cancer, or carcinoma, considered as a *genus* of disease, comprehends two or three *species*, which present among themselves very striking differences, and of which the varieties have received a puzzling multiplicity of names; scirrhus, stone cancer, medullary sarcoma, encephaloid or cerebriform disease, soft cancer, fungus hæmatodes, colloid or gum cancer, and several more. The simplest division, founded upon the consistence of the morbid growth, is into hard and soft cancer. But the most modern and scientific system recognises *three* species,—viz. scirrhus; encephaloid, or brain-like cancer; and colloid, or gum-like cancer. The physical characters of these three species offer strong points, not merely of difference, but even of contrast.

Scirrhus, as that word implies, is remarkable, in its early stages, for its hardness. It is as firm as cartilage, and creaks when divided by a sharp knife. The surfaces exposed by its division present a glistening, satiny appearance, and a white, or grey, or bluish-white colour. Athwart this greyish and semitransparent substance run opaque intersecting bands, having a fibrous aspect. By strong pressure a thin juice may be made to ooze from a slice of the scirrhus tumour.

Encephaloid cancer is also well named. It is composed, in great measure, of a soft, white, opaque, pulpy substance, very closely resembling, both in colour and in consistence, that of the healthy brain. This cerebriform pulp is traversed and circumscribed by fibrous septa, which are sometimes extremely thin and delicate. In both these species of cancerous growth, therefore, there is a contained and a containing element.

The same feature is still more distinctly marked in the third species, the *colloid cancer*, which exhibits the appearance of small portions of a greenish-yellow transparent gum, or jelly, arranged in regular cells. Hence it is sometimes denominated *alveolar cancer*.

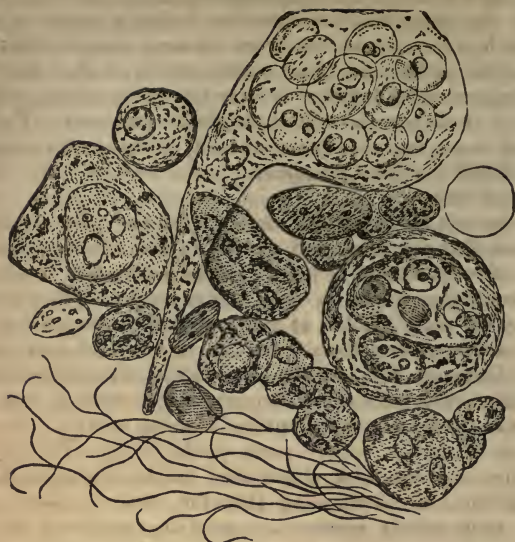
You may ask upon what principles structures so dissimilar in their physical appearance have been assigned to the same genus? Why, for these reasons. They are all strictly destructive or malignant forms of disease. Although in any shape they are of somewhat rare occurrence, yet when they do occur, two, or all three of the species

FIG. 19.



Fibrous stroma of scirrhus.

FIG. 20.



Cells from Encephaloid of Tongue (rapidly growing).

FIG. 21.

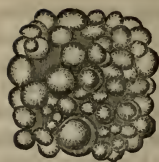
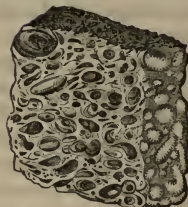


FIG. 22.



Colloid tumor. FIG. 21. External appearance. FIG. 22. Internal structure. From a preparation in the collection of Dr. Gross.

are often found to co-exist in different organs of the same individual; nay, in contiguous parts of the same organ. More than this: if a tumour consisting of one species be amputated, and a fresh growth spring (as too often it does) from the same spot, this secondary growth is frequently of *another* species. There can be no doubt that all are connected by some very intimate bond of union; and the facts I have just stated suggest the question, whether instead of being different species of the same genus, they ought not rather to be regarded as mere varieties of the same species.

Of all three it has been ascertained, by much and fatal experience, that occurring in any one part of the body they are prone to multiply in various other parts; that they are commonly attended, during some part at least of their progress, with very severe pain; that they are uncontrollable by any known remedy; and tend always, sometimes slowly, sometimes with frightful rapidity, to augment in bulk; eating away contiguous parts by their invasion and pressure; breaking out, when near the surface, into foul and repulsive ulceration; producing often the most ghastly disfigurement; and ultimately destroying life. Sometimes vital parts are slowly disorganized by the corroding extension of these tumours; sometimes large blood-vessels are laid open, and death is suddenly brought about by hæmorrhage; and sometimes the powers of life sink gradually under the wearing influence of the disease, and that degeneracy of the blood which it causes or accompanies.

There is scarcely an organ or texture of the body which is not liable to be attacked by this terrible foe: the brain, the eye, the lip and face, the lungs, the stomach, the intestines, the liver, the kidneys, the breast, the womb, the testicle, the bones. But some parts are more often the seat of cancer than others. Among these may be reckoned the female mamma, the uterus, the stomach, the liver, and the testicle.

The mode in which cancer *originates* is uncertain; the modes in which it *spreads* and *multiplies* are better understood. An individual tumour may enlarge by the progressive insinuation of the cancerous matter into the interstices of the neighbouring tissues, which, thus fastening upon, it consolidates. The disease may be communicated, by imbibition, from one organ to another which is in mere contact with it. But how does it come to occupy at the same time, or in quick succession, several separate and distant organs? This is a question of the greatest interest and importance, and it admits of a distinct reply.

Cancer often makes its appearance in a single spot on the surface of the body; in the female breast, for instance. We see and feel it there while it is yet small, and while the general health of the patient seems to be otherwise perfect. By degrees the tumour increases, and at length it softens in some places; the glands of the axilla

become swollen, hard, painful, and filled sometimes with cancerous matter; the tumour breaks perhaps through the skin, and presents the shocking spectacle of "open cancer;" the general health gives way, and the skin assumes a straw-coloured tint. During this process, unless the patient dies prematurely, or the original disease is removed by a surgical operation, cancerous tumours form in one or in several of the internal organs, and give notice of their presence by appropriate symptoms. There is an original morbid growth, and there are subsequent morbid growths; a primary tumour, and secondary tumours; and the latter are *caused* by the former. This is a most important fact, if indeed it be true.

Now, Müller has discovered, by means of the microscope, and the discovery has been confirmed by other observers, that the contained matter, in the several species or varieties of cancer, consists of very minute cells, with nuclei attached to their walls, and of granules still more minute, which are supposed to be the rudiments of new cells. It is (apparently) by the amplification of these granules into cells, and by the development of the nuclei into other cells, and by the growth and evanescence of young cells, which, in some instances, are included generation after generation within parent cells, that the original tumours enlarge and extend themselves; and it is by the transference of certain of these cells and granules from the original tumour that a crop of secondary tumours is sown in remote parts of the body. The cells, and probably the granules also, are endowed with a power of self-increase and propagation, whenever they find a fitting soil. Possessing, like the seeds of plants, an inherent vitality of their own, they merely require, in order to germinate, to be placed in contact with some living tissue, wherewith they may form vascular connexions, and wherefrom they may draw the materials of their nourishment. Cohering together, for the most part, with but little force, they are easily detached from the parent mass. It is matter of fact that the secondary tumours form most surely and most rapidly when the primary tumour is of a soft kind; and that when they succeed to scirrhus, it is after the process of softening has commenced in that originally hard structure. These germs—which present in their forms and mode of generation, striking analogies with those of some of the lower animals, as well as with those of plants—these germs are carried sometimes through the lymphatic vessels to absorbent glands in the vicinity of the primary growth; from a cancerous breast, for example, to the glands of the axilla: but there can scarcely be a doubt that the *blood* is the main channel by which the seeds of this dreadful malady are conveyed from its first to its subsequent sites, and thence perhaps, if life continue long enough, to tertiary locations. The gross matter of cancer is often to be found in the veins that proceed from the primary tumour,—nay, in large venous trunks at a distance; so that some distinguished pathologists have too hastily conjectured that it may originate in the veins. You are doubtless aware that foreign substances, circulating with the blood, stop or are entangled more often in some organs than in others. Minute globules of mercury, when that metal has been introduced into the veins, are found strewed through the substance of the lungs, and of the liver. Pus, received into the blood in phlebitis, is arrested, and forms scattered points of inflammation and abscess, in the same organs; and it is in the liver and the lungs that separate tumours of secondary cancer are most commonly met with. If this be the true theory of secondary cancerous formations, I need scarcely point out to you the urgent importance of the rule which prescribes to the surgeon the *most complete* extirpation of the primary tumour, *at the earliest possible period* of its existence.

The disseminated cancer-germs are not scattered to this organ or to that, to the lungs or to the liver, indiscriminately or by chance. Their distribution bears a certain relation, as you may readily conceive, to the situation of the primary disease. Since the blood, in its return from the stomach, the intestines, the rectum, passes through the portal system of veins before it reaches the lungs, you would expect that cancer-cells conveyed from those parts would be stopped in their journey through the capillary vessels of the liver—and it is so. A few, in very rare instances, pass on to lodge and grow in the lungs. Again, the blood from the breast goes direct to the vena cava, and the lungs are the first to receive and entangle whatever seeds of disease it may carry: for what is true of cancer-germs is true of pus also, and of other morbid materials. The pulmonary capillaries, however, are more easily permeable by such matters than the hepatic. Some of the germs pass through them, and enter the

general circulation; and the liver, as well as the lungs, frequently becomes contaminated: and germs may thus settle in other parts. In these facts we see one reason why cancerous growths are more often met with in the liver than in any other organ of the body. Primary cancer of the kidney is, in like manner, apt to be disseminated through the medium of the vena cava; and we might expect that cancer-germs from the uterus would take the same course, and affect the lungs earlier and more often than the liver. But that is not always the case. The liver is liable to be secondarily affected from malignant disease of the womb; and Dr. Budd gives the explanation of this apparent anomaly. The rectum and the hæmorrhoidal veins, which return the blood to the *vena portæ*, very frequently become implicated in the uterine disorganization. Fortunately, cancer of the uterus, which is a fearfully common disease, is not so often disseminated as cancer of the stomach, or of the mamma.

The origin of primary cancer is involved in much obscurity. It seems, however (and this, after what has just been stated, you might expect), that the germs of the disease are capable of being transferred from one human being to another; and even to an animal of a different species. Langenbeck injected cancerous matter, just taken from a living body, into the veins of a dog. After some weeks the dog began to pine away, and was then killed, and cancerous growths were found in its lungs. Several instances have occurred—I have myself known of two—of cancer of the penis in men whose wives laboured under cancer of the uterus. Here it is presumable that the cancerous germs received upon a delicate and vascular surface, and suffered perhaps to lodge there through neglect of cleanliness, might fasten upon the part, take root there as it were, and grow. One very curious circumstance connected with this subject is, that the frequent contact of common soot seems to have the power of producing cancer. There is a form of carcinoma, affecting chiefly the scrotum, and familiar to surgeons as the chimney-sweeper's cancer. A case is recorded of cancer of the same variety occurring in the right hand of a gardener, who for years had been in the habit of sprinkling soot over his flower-beds with his hands.

There are not wanting, then, plausible grounds for the hypothesis, that the seeds of cancer may be introduced, in some way which eludes observation, from without; that cancerous growths are strictly parasitic, and independent of the body, excepting so far as they derive their pabulum from its juices. The difficulties involved in this supposition are not greater (as we shall see hereafter) than those that hang over the source and origin of certain entozoa, with which the body is liable to be infested. But whether this hypothesis be true, or whether the cancer cells and germs are merely morbid elements of the native tissues of the body, developed by some perverted energy of the formative process, remains yet to be determined.

From the tables contained in the Reports of the Registrar-General, it would appear that women are more subject to this fearful disorder than men, in the large ratio of five to two. It fixes chiefly upon the female organs of reproduction; the mammæ and the uterus.

The mortality from cancer, estimated with due reference to the whole number of persons existing at different ages, increases steadily as life advances.

There are still some general habitudes of the different varieties of cancer, with which I should wish you to be acquainted.

The secondary formations are most commonly of the encephaloid kind, whatever the primary form may have been.

Encephaloid cancer, as compared with scirrhus, is abundantly furnished with blood-vessels: and upon this difference in their degree of vascularity other remarkable differences between the two varieties seem to depend. First, encephaloid tumours generally augment with much greater rapidity, and attain a much larger size, than scirrhus tumours. Occasionally their magnitude comes to be enormous. Again, cerebriform growths seldom happen singly, but occupy several organs of the body at once. Scirrhus, increasing slowly, occurs also in fewer sites; it is sometimes even solitary. More tissues, too, appear to be obnoxious to the soft than to the hard variety.

Now (as Dr. William Budd has well remarked) a large apparatus of blood-vessels, bringing a proportionally plentiful supply of nourishment to the parasitic tumour, accounts sufficiently for its rank and rapid growth; and the same condition, especially when conjoined with *softness* of the parent mass, affords obvious facilities for the

liberal dissemination of its germs through numerous returning channels. In fact, the soft varieties alone have, as yet, been found in the veins.

The same multitude of its blood-vessels, and slender cohesion of its component parts, serve to explain another peculiarity of the cerebriform species. Intermixed with, or diffused through, the brain-like substance, there is often to be seen a quantity of extravasated blood: and when the disease breaks out into ulceration, red, ragged, and *bleeding* growths, of fungous aspect, sprout rapidly from the open surface. To these *accidents* of cancer the term *fungus hæmatodes* is to be traced. We do not find scirrhus to be the seat of similar interstitial hæmorrhages.

Encephaloid cancer has less tendency to contract adhesions with contiguous parts than scirrhus has.

Of the alveolar variety, which has been more lately discriminated from the others, and less studied, less is known. It occurs principally in the abdomen, affecting the pyloric orifice of the stomach, and the omentum. It appears also occasionally in the bones, in the breast, and in the testicle. It spreads chiefly by contact and inoculation; the cells of this species of cancer being too large to be readily conveyed to distant parts through the veins. Although sometimes combined with the two other species in the same person, it is often alone, and limited to a single organ. I believe it has not been met with except in adults.

For more minute information on this subject, so interesting and important both in its pathological relations and in its practical bearings, I must refer you to Professor Walshe's very able and elaborate work on cancer; and to a short but admirable essay on the same topic, by Dr. William Budd, published in the *Lancet*. From these sources has been derived much of what I have now been stating.

[We may also refer the reader to the full and excellent paper of Dr. Carswell, on Scirrhus, in the *Cyclopædia of Practical Medicine*.—C.]

Returning to our current theme, I proceed, in the next place, to speak, in a general manner, of the measures to be adopted when we are called upon to administer to the relief of a person labouring under inflammation: of what is sometimes called the *cure*; but, more correctly, of the *treatment* of inflammation.

In describing the phenomena and progress of inflammation, I took *external* inflammation as a *type*, and I shall keep that type principally in view in what I have to say respecting its *treatment*: making, however, such reference to the inflammation of internal parts as the subject will permit. You will bear in mind that my design at present is merely to explain the *principles* of treatment, generally: I shall point out, by and by, the application of those principles, and the modifications they may require, in respect to particular cases. I speak also, now, of *common* inflammation, occurring in a previously healthy person. There are many observations that concern all inflammations alike, whether external or internal, and by despatching these in the outset, I hope to avoid much repetition hereafter.

In all cases of inflammation, our first object is, if possible, to obtain *resolution*: and if that be not possible, we next aim at securing *that event* of inflammation which would be the most fortunate in the particular case before us. In external inflammations *good suppuration* will generally, next to resolution, be the most desirable event: in internal inflammations it will be sometimes *suppuration*, sometimes *adhesion*.

It is necessary to keep in view the distinction between the treatment proper for the inflammation itself; and the treatment that may be required for the *effects* of the inflammation. At present we are concerned only with the inflammation itself.

I stated to you in a former lecture, that a knowledge of the *cause* of a disease might help us in its treatment. Knowing the cause, our first care must be to remove it, if we can. In the case formerly supposed, we should extract from the inflamed arm the fragment of glass. If the inflammation have been excited by the extremity of a *fractured bone*, of a broken rib for example, we take measures for bringing the separated bones into their proper places, and for keeping them there: if the mere *displacement* of a part have occasioned the inflammation, as the dislocation of a joint, the protrusion of the bowel in hernia, the first thing to be attended to is the restoration of the part to its natural situation: if there be any *chemical* source of irritation

(in the stomach, for instance, threatening or producing inflammation there), we eject, neutralize, or dilute it.

I know of but one exception to this rule, and it belongs to surgery: *to wit*, when a bullet or a splinter is so lodged in the interior of the body, that its extraction would be more hurtful or hazardous than its being left where it is.

A knowledge of the cause of an inflammatory disease may help us in another way. We do not treat a joint that is inflamed in consequence of external violence, as we should treat the same joint when inflamed in rheumatism.

But it is very seldom, except when the inflammation is external, that we can accomplish the removal of its cause. In most internal cases, either it cannot be got at, or it has already ceased to be applied; as when the inflammation has been excited by exposure to cold. Yet it may be possible, and it is of the utmost importance when possible, to prevent any re-application or repetition of the same cause, which would be likely to frustrate our endeavours to bring about resolution.

Next in importance to the removal and avoidance of the exciting cause, must be placed, in most instances, the observance of what is called the *antiphlogistic regimen*. This may seem an old-fashioned phrase, but it is a very convenient one; being a brief form of expressing the sum of several distinct provisions for the welfare of the sick, and for the conduct of their attendants. The word antiphlogistic is derived, indeed, from an obsolete theory; but we retain it as a useful arbitrary term, without reference to its etymology, or to its original meaning.

The object of the antiphlogistic regimen is to put and keep the patient in that state which is most favourable for the spontaneous subsidence of the disease, or for the sanative influence of remedies. This regimen consists in the avoidance of every stimulus or disturbing influence that can be avoided, whether external or internal. Common sense will suggest to you the details. It implies a total abstinence from solid animal food, and from strong drink of all kinds. It prescribes the exclusion of all that might excite or exercise the mind, or produce a strong impression upon the senses: noise; bright light; great heat or cold. The patient should be kept in a temperature of about 62°, and in a well-ventilated apartment. He must not be allowed to converse, nor to attend to matters of business; unless, indeed, his mind happens to be disturbed and anxious about some point which one short interview with a friend may effectually settle. All causes of strong emotion, and mental agitation, should be strictly guarded against. Whatever tends to quicken the circulation is to be shunned; and therefore not only those influences which operate through the nervous system, but also all needless bodily effort and exertion, must be prohibited. The patient (in the serious cases I am now contemplating) must remain in bed: and in a position which facilitates, or at least does not impede, the free return of the blood by the veins from the suffering organ. If the inflammation be seated in or about the head, that part should be elevated by pillows. If one of the lower extremities be affected, even when the disease is not so intense as to require confinement to bed, the limb must be sustained horizontally, or be even still more raised up. On the same principle it is that we suspend an inflamed hand or forearm in a sling. In some cases of internal inflammation—in pleurisy for example—the patient will choose his own position. He is admonished, by the pain and distress they occasion, that certain postures would be hurtful or dangerous, and he carefully avoids them. We often derive much information from this instinctive caution on the part of our patient.

The function of the organ inflamed should also be spared its exercise whenever, and in as great a degree as, that can be done. As you would not allow a patient to move an inflamed joint, so you must not permit him to gaze with an inflamed eye; to speak more than may be absolutely necessary with an inflamed lung, or larynx; to exert by thinking, and by attention to external excitements, an inflamed brain. This last rule is essential, even when the brain is not the seat of the inflammation: it is to be observed in all febrile disorders.

The adoption of this antiphlogistic regimen is not, indeed, necessary, nor even proper, in all cases and stages of inflammation. The inflammation may be so slight as not to require it; particularly in external cases, of which the causes and the extent are known; as slight contusions, trifling wounds, and some kinds of eruption. But this exception must always be applied with great caution to cases of internal inflammation, about the causes, and extent, and tendencies of which we may be less sure.

In chronic forms of inflammation again, as in scrofulous inflammation of the lymphatic glands, or of the eyes, attended with but little pain or heat, the antiphlogistic regimen would often fail to be beneficial: the state of the general system being such as to require support and strengthening measures, more than the local symptoms require an opposite treatment. So also when suppuration or gangrene has supervened, the antiphlogistic regimen must generally be modified, or abandoned.

But in the outset of all cases of serious inflammation, when the strength is entire, and the inflammation intense enough to produce pyrexia, all the particulars of the antiphlogistic regimen may require to be observed.

Of all the *direct remedies* of inflammation, the abstraction of blood, bleeding, or blood-letting, as it is called, is by much the most powerful and important. We should, I think, be prepared to expect this, prior to any experience of it. Blood being the natural stimulus of the heart, we should deem it probable that the removal of a portion of that fluid would diminish the force with which the heart contracts: and as an inflamed part contains a preternatural quantity of red blood, and as (with the exception of resolution and mortification, which really are *terminations* of inflammation) all the *events* of inflammation depend upon the exudation of certain parts of the blood from its containing blood-vessels, we should be inclined, *à priori*, to believe that the amount of those exudations would be checked and limited by lessening the supply of blood to the inflamed organ, as well as by abating the force with which the blood reaches it. And we find it in fact to be so. The results of experience confirm, in this matter, the suggestions of our reason. Blood forms the pabulum of the whole process. "If," (says Mr. Lawrence), "we may be allowed to use figurative language, the obvious increase of heat in the part is analogous to that of fire; and blood is the fuel by which the flame is kept up: in fact, if we could completely take away its blood from the part, we should be able entirely to control or arrest the increased action."

But it is not every case of inflammation that requires or warrants the abstraction of blood; and when blood-letting *is* requisite, the mode of taking away the blood, the quantity proper to be taken, and the propriety of repeating the bleeding, all vary greatly in different cases. It is obviously of vast importance that you should learn so to use this valuable remedy as not to abuse it. Its power is great for evil as well as for good; and in rash or inexperienced hands it too often becomes an instrument of fatal mischief.

There are, as you all are aware, several modes of abstracting blood: phlebotomy, arteriotomy, scarification, cupping (which is merely a variety of scarification), the application of leeches. Bleeding performed in either of the first two of these methods is called *general bleeding*. The rest are, in most instances, topical or *local*: but they are not *merely* topical in all cases. The main object of general bleeding is to diminish the whole quantity of blood in the system, and thus to lessen the force of the heart's action. The object of local bleeding is, in most instances, that of emptying the gorged and loaded capillaries of the inflamed part. Sometimes the blood is thus taken directly from the turgid vessels themselves; more often, I fancy, topical blood-letting produces its effect by diverting the flow of blood from the affected part, and giving it a new direction, and so indirectly relieving the inflammatory congestion. General bleeding has also incidentally a similar tendency to deplete the vessels concerned in the diseased process: and, on the other hand, a dexterous cupper, under favourable circumstances, will take away blood from a part as copiously and rapidly as if it were made to flow from an opened vein; and then the effect upon the *system* will be alike in the one case and in the other. The same may be said of leeches, when they are applied in the enormous numbers which our neighbours, the French, are fond of using. In whatever way the blood is drawn, whether from a vein or from an artery, or by the pressure of a cupping glass around a surface previously scarified, or by the suction of leeches, the general effect upon the system will be in proportion to the quantity of blood abstracted in a given time. The most convenient and effectual mode of general bleeding, upon the whole, is certainly the common one, from the veins at the bend of the arm. But sometimes those veins are small or deep, especially in fat people: and we fail in our efforts to get the blood to flow from them in a full stream: and then we may open some other vein or an artery, or call in the cupper to our assistance, or cover

the neighbouring surface with leeches; according to the situation of the part inflamed, and to other circumstances.

Let us now briefly consider what the indications are by which we judge of the expediency of taking away blood. We are guided very much by the degree of pyrexia; by the quality of the pulse; by the importance of the organ affected; by the violence of the inflammation, in what manner soever that may be measured; by the period or stage of the disease; by the age, and sex, and general condition of the patient; and frequently also by the ordinary character and course of the disease, when inflammation happens to be, or to accompany, an epidemic disorder. It is not one of these circumstances alone, but several of them, that we have to take into the account, in most cases; and what I have now to say in reference to them must needs be very general.

The presence of pyrexia, especially when the febrile disturbance is well marked, admonishes us, indeed, to search after other indications of the propriety of blood-letting, and confirms them if they are found; but is not, of itself, a sufficient reason for resorting to that remedy. There may be high febrile symptoms without any inflammation at all; as in the hot stage of an ague fit. Again, a smart attack of fever may spring out of local inflammation, and yet the known course of the disease, or the nature of the part affected, may render the abstraction of blood unnecessary, and therefore improper. Nay, the presence of fever, when it is not the consequence but the precursor of the inflammation — when it depends upon a specific poison in the blood, and the inflammation has arisen as one of its casual complications — may *prohibit* any form of blood-letting.

Our judgment is more often determined by the quality of the pulse, although we are by no means to be *wholly* directed by this. The quality of the pulse which — other things being the same — bespeaks the necessity of blood-letting in inflammation, is *hardness*. I described this quality to you in a former lecture: it may coexist with a large or a small, a slow or a frequent pulse. Most commonly (and yet the exceptions are numerous) in acute inflammations the pulse is full and frequent as well as hard. The hardness is ascertained and measured by the resistance which the throb of the artery makes to the pressure of your finger. The pulse is sometimes said to be incompressible; which means that, although you apply your finger with considerable firmness, the blood still forces its way through the vessel beneath it.

Now this hardness or strength of the pulse is sometimes our best warrant for active depletion by means of the lancet; yet I say we must not trust to this alone; for a hard pulse may habitually exist, where there is no inflammation. Certain chronic diseased conditions of the heart may occasion it; and it probably results also sometimes from some unnatural state, which is not inflammation, of the whole of the circulating system. When you happen to know your patient, and have ascertained what kind of pulse he has when he is well, and are previously aware that his pulse during health is *not* a hard pulse, you learn from that circumstance that the new quality it has now acquired denotes the presence of inflammation; and usually of active inflammation, likely to go on, if not controlled, to the destruction of the part it has seized upon.

Many persons, and young practitioners in particular, are apt to look to the *frequency* of the pulse, when they wish to ascertain the expediency of blood-letting; but really its frequency is very subordinate in importance to its hardness or softness: and this is very unlucky, because anybody with his stop-watch in his hand can *count* a pulse; but it is not every one who can tell a hard pulse when he feels it. The finger requires a certain education for that purpose; and there are some persons who seem never to attain the *tactus eruditus*. I should advise you to attend particularly to this quality of the pulse, and to compare your perceptions of the hardness or softness of the pulse in individual cases, with those of your companions, and of your clinical teachers.

The frequency of an inflammatory pulse ranges for the most part between 90 and 120. When the hard pulse is much more frequent than this, it commonly occurs either in young children; or in persons who are more than usually nervous and susceptible; or in persons who were previously labouring under some chronic and wasting complaint, in which the pulse was already frequent, though not hard: as, for example, in phthisical patients, when acute pleurisy supervenes upon tubercular dis-

ease of the lungs. In all such cases, the extreme frequency of the pulse is, *per se*, dissuasive of the use of the lancet.

As the hardness of the pulse is, with certain exceptions at which I have just glanced, our lawful warrant for general bleeding, so the disappearance of that hardness is a token that the blood-letting has been carried far enough.

Again, the nature and importance of the organ affected will influence our judgment in respect to the question of abstracting blood. If the organ inflamed be a vital organ; or if we are not sure about that, but have any reason to suspect that it may be a vital organ; I need scarcely say that, other indications concurring, we must act upon the worst supposition, and bleed. But when the part is of less importance in the economy of the body; or when inflammation is known ordinarily to run its course in that part without producing any abiding damage; it may not be worth while, even though the fever be high and the pulse hard, to have recourse to this potent remedy, for the sake of subduing inflammation which is attended with so little danger. In this predicament may be placed many instances of cynanche tonsillaris, and of acute rheumatism. The subsequent debilitating effects of the loss of blood upon the system may be more certain and more hurtful than the effect of the bleeding upon the local inflammation is likely to be beneficial.

[Without pretending to advocate the employment of blood-letting in every case of cynanche tonsillaris and of acute rheumatism, we must dissent from the correctness of Dr. Watson's position, that "even though the fever be high and the pulse hard," it may not be worth while to bleed "for the sake of subduing inflammation which, in these affections, is attended with so little danger." When acute rheumatism occurs in young, robust, and plethoric subjects, and is attended with considerable fever and a hard and accelerated pulse, we know of no remedy from which such decided advantage is to be obtained as from a well-timed resort to the lancet. The extent to which the bleeding is to be carried and the propriety of its repetition, must be determined by the circumstances of each case, and the effects produced by the first operation. We are no advocate, under any circumstances, for the profuse and repeated bleedings recommended by M. Bouillaud as a means of cutting short an attack of acute rheumatism; we have had, however, ample experience of the very decided relief obtained by a prudent and well-timed use of the lancet in this disease.

In many cases of cynanche tonsillaris, also, an early and full bleeding will have the effect of arresting the inflammation, and thus of saving the patient from much suffering, if not danger. Even when the arrest of the disease is not affected by it, the inflammation is reduced in violence and shortened in duration. — C.]

The period or stage of the disease forms a most important element, the most important indeed of all, in the question before us. It is of inflammation while yet in its earliest progress, that blood-letting may emphatically be pronounced the cure; while the disease is still within the possibility of resolution; before there is any great amount of exudation, or any serious disorganization of structure. The sooner we bleed; the more surely will the inflammatory process be moderated and limited, even when it cannot be wholly quenched. In no case within the range of medical practice is the maxim "*principiis obsta*" more imperative. Those among you who happen to be attending the wards of the Middlesex Hospital may wonder indeed, after hearing my estimate of the power of blood-letting over inflammation, that I so seldom prescribe venæsection there. The truth is, not that I undervalue the remedy, but that the time for its employment has generally gone by. The poor are unwilling to relinquish the occupations by which they subsist: they struggle on as long as they can, and resort to hospitals only when they are compelled to do so by the exigency of their malady. Many of them, labouring under inflammation, have been freely bled before admission. It is commonly too late, when they present themselves, to expect that the course of the disease can be so arrested. The first effect of blood-letting is to deplete and relieve the labouring circulation. But when it is again and again repeated, it becomes (as the French say) *spoliative*; it robs the vital fluid of its nutrient and plastic materials. Pushed still further, it produces a peculiar state of the nervous system, marked by great weakness and irritability. Now although blood-letting is the *summum remedium* for active inflammation at its very commence-

ment, there is a point beyond which it not only does no good, but is positively injurious. And this point it is not always easy to hit. On one side is the danger that the inflammatory action may continue and extend; on the other the danger that the strength of the system may be so reduced as to prove unequal to the process of restoration: for, to remove the interstitial extravasations, and to repair the damage that has accrued, a certain degree of vital power is requisite, and a sufficient quantity of healthy blood. Bleeding may cure inflammation, but it will not always cure the effects of inflammation; nay, it may render them lingering in their departure, or even determine their fatality. I cannot too often, or too strongly inculcate the precept, that in order to extinguish or check acute inflammation, you must, above all, bleed *early*.

We judge that the bleeding has been carried far enough when the inflammatory fever subsides, or changes its character; when the pulse regains its softness, or undergoes some marked alteration; when any of the signs (already specified) of supuration appear. Upon these points I hope to give you more explicit instruction when we come to special instances of inflammation.

Whenever inflammation supervenes on other chronic disease; whenever it arises in the progress of idiopathic fever, or whilst the constitution is contaminated by some specific poison; whenever suppuration is inevitable, or even probable: in all these cases general blood-letting *may* be necessary, but it must always be employed with great caution.

Nor can we, safely, neglect the age, the sex, the general condition of the sick person, when we are turning in our minds the propriety of bleeding. The very young, the old, the feeble, the cachectic, do not bear well the loss of much blood. This consideration is not to deter you from bleeding such persons when they are attacked by dangerous inflammation; but it especially enforces, with respect to them, the general rule, that no more blood should be abstracted than seems absolutely requisite to control the disease.

It is also very necessary to study the character and tendency of the reigning epidemic: whether that may depend upon some predisposition silently and gradually wrought in men's bodies by the agency of causes that are but little understood: or whether it may result from some peculiarity in the exciting cause of a particular epidemic disease. I have been long enough in practice in London to have learned, in common with others, how much the character of continued fevers may alter. Since about the time when the virulent form of cholera made its first appearance among us, continued fevers have neither needed nor borne the abstraction of blood, as they did bear and need it for some years prior to that period. Perhaps some variation in the intensity of the poison may partly explain the comparative malignity—the greater tendency, I mean, to what is called lowness of type—which marks certain epidemics of scarlet fever, small-pox, and measles. The influenza, or epidemic catarrh, which was almost universal in this town and kingdom in the years 1833, 1837, and 1847, afforded a striking illustration of the point I am endeavouring to set before you. The inflammatory symptoms—the bronchitis, and sometimes pneumonia—were in many cases strongly marked, and it appeared necessary to abstract blood; but persons suffering under influenza bore bleeding exceedingly ill, and where the use of the lancet could not be avoided, it was never resorted to without reluctance and misgiving. I would not, however, limit these remarks to fevers, or to specific inflammations. I share in the belief which has grown out of the experience of many thoughtful and observing men, that in this country at least, the human constitution has for several years been suffering a gradual change: that almost all inflammatory disorders assume now-a-days a more adynamic type, and require less energetic treatment than in the early part of the present century.

When we bleed in acute inflammation of an important organ, we endeavour, I say, to effect our purpose as speedily as possible, and with as little expenditure of the vital fluid as possible. It would be quite ridiculous to pretend to give any precise direction as to the number of ounces of blood that should be taken. You must stay by the patient, and bleed, in such cases as I am now contemplating, until you produce some distinct impression by the bleeding; and one of the best guides in this matter is the state of the pulse. If you find, as you sometimes will do, that the most pressing symptoms give way while the blood is still flowing—that the pain, for instance, is

mitigated — that the respiration (when the lungs are concerned) becomes easier and deeper — that (in affections of the brain) the patient emerges from a state of stupor or delirium — you may be sure that you are doing right in bleeding; but you must keep your finger upon your patient's wrist, and suffer the blood to flow, until the hard pulse is sensibly softer, or until symptoms of impending syncope appear; and then you had better tie up the arm, and wait a few hours, and repeat the bleeding if the symptoms which at first demanded it again become urgent.

As it is desirable to produce the necessary effect upon the system as quickly as may be, the blood should be taken *pleno rivo*; i. e., a sufficiently large orifice should be made in the vein: and sometimes it may be right even to open a vein in both arms: and the patient should be bled in the upright position. Faintness and syncope depend upon a defective supply of blood to the brain; and therefore will be likely to occur the sooner when the force of gravity facilitates the descent of the blood from the head through the veins, and retards its ascent towards the head through the arteries. And conversely, the first thing to be done towards remedying syncope is to lay the person flat in a horizontal posture, or even with his head lower than his body.

If you neglect these smaller matters, and make an insignificant slit in the vein, and suffer your patient to lie down whilst you are bleeding him, you will be obliged to take much more blood in the end; or you may drain him of his blood and of his strength by repeated bleedings of this sort, and make no impression after all upon the disease. It is one of the numerous cases in which parsimony is not true economy.

The quantity of blood requisite to be taken in order to produce the due effect is exceedingly various. It is a remarkable circumstance, well worth attending to, and much insisted upon of late years, especially by Dr. Marshall Hall, that a patient under the influence of mere inflammation will bear to lose a far greater quantity of blood without becoming faint, than he could bear in health: that the state of the system produced by the presence of inflammation supports it against the ordinary consequences of loss of blood. The amount of the bleeding requisite to occasion syncope will be in proportion to the exigency of the case. This fact—if it be really a fact, as indeed I believe it is — is evidently one of the highest value and importance, for it furnishes, what is always so desirable, especially in an uncertain art like ours, a simple rule of practice. Yet it is not a rule so firmly established as not to admit of exceptions. If the mere state of syncope were the curative influence required, we should have no difficulty. That the faintness does constitute a part of that influence I fully believe. Dr. M. Solon relates a case in which it even sufficed to the cure of erysipelas of the head and face, attended with high fever. The patient fainted from alarm before the vein was opened. The inflammatory symptoms thereupon ceased: but with returning animation they presently recurred. Again preparation was made for venæsection; and again the young lady lapsed into syncope; and this time the inflammation and fever disappeared, never to return. She is described as having been quite well the next day. I cannot, however, entertain a doubt that the withdrawal of a certain quantity of blood is, in almost every fit case, conducive to the permanent control of common acute inflammation, attended with strong pyrexia: and it may be advisable to keep persons, who, like M. Solon's patient, are of a timid disposition, and liable to syncope from slight causes, in a recumbent posture, in order that the necessary discharge of blood from the system may be obtained.

In equivocal cases (and there are many such), where it is questionable whether the symptoms proceed from inflammation or not, the diagnosis may often be settled by observing the quantity of blood which, taken in the upright posture, suffices to bring on incipient syncope. Another criterion — more exact perhaps than this, but requiring more time, knowledge, and skill for its due application — is afforded by the proportion of fibrin in the blood abstracted.

Dr. Hall's book, *On the Effects of the Loss of Blood*, is well worth your attentive perusal. He suggests that a scale of diseases might be formed, representing the sustaining influence of some maladies against the effects of blood-letting; and the opposite influence of some others in producing preternatural susceptibility of those effects. "It would begin (he says) with congestion of the head, or tendency to apoplexy; inflammation of the serous membranes, and of the parenchymatous substance of various organs, would follow; then acute anasarca; and lastly, inflammation of the mucous membranes. This part of the scale would be divided from the next by the con-

dition of the system in health. Below this would be arranged fevers; the effects of intestinal irritation; some cases of delirium; reaction from loss of blood; and disorders of the same class with hysteria; dyspepsia, chlorosis, and cholera morbus.

As to the propriety of *repeating* venæsection, it is his remark, that if at the first blood-letting much blood flowed before any tendency to syncope manifested itself, an early repetition of that remedy will probably be required—and at any rate an early repetition of our *visit* to the patient will be proper. But this last precept is of universal obligation in all cases of serious inflammation.

I am almost afraid to tell you how much blood I *have* seen taken at one bleeding, lest I should seem to encourage you to imitate such heroic practice. I once stood by, and saw, not without trembling—although I was quite free from responsibility in the matter—a vein in the arm kept open until seventy-two ounces (four pints and a half) of blood had issued from it: and then, and not till then, did the patient become faint. The event of the case quite justified the bleeding in that instance, for the man got rapidly and perfectly well. It was a case of general dropsy, which had come on suddenly, in a young and robust man. It occurred in the clinical wards of the Infirmary at Edinburgh: the physician had desired the clinical clerk to bleed the patient in the erect posture, until some sensible effect was produced upon his pulse: and no such effect could be perceived until the enormous quantity I have mentioned had been abstracted. It is very seldom that such large bleedings are required, or defensible: you will generally find that from sixteen to twenty or thirty ounces taken properly, will be sufficient to accomplish the purpose of the measure. Sometimes one such bleeding will extinguish, as it were, the inflammation, or curb its destructive force; sometimes two or three, or half a dozen may be necessary: and we judge of the propriety of repeating the venæsection by the effect of the former bleeding; by the character of the pulse; by the appearance of the blood already drawn. It would be impossible, in a general account like the present, to lay down any minute directions on this head.

I have hitherto been speaking of bleeding as we perform it for the cure of active inflammation, occurring in a person previously healthy, affecting an important organ, and attended with high febrile disturbance of the system. But the removal of blood is scarcely less valuable as a remedial measure in *chronic* inflammation, when the system at large scarcely sympathises at all with the local disease. And here it is that what is properly called local bleeding is so useful—by cupping glasses, or a moderate number of leeches. The object is always the same, viz., to unload and relieve the turgid capillary vessels of the part: and this we could not do by general bleeding without carrying it to an extent which would be dangerous to our patient's welfare. These local bleedings for chronic inflammation usually require to be often repeated. Considered as a remedy, blood-letting resembles some other remedies in this, that it must be proportioned and adjusted to the rate of progress, and the duration, of the disease. The remedy must be used chronically when the malady is chronic. A patient may lose, on the whole, much more blood for the cure of a chronic inflammation, than for the cure of one that is violent and acute; but then the bleeding must be spread over a larger space of time.

With respect to the relative merits and advantages of cupping and of leeches, as topical remedies for local inflammation; it may be said in favour of cupping, that the precise quantity of blood taken away is more accurately determined in that manner, and the operation is sooner over, and is less fatiguing, than the suction of leeches. But on the other hand the leeches seldom bungle in the operation; while the surgeon sometimes does. It requires a good deal of practice to become handy and dexterous in the application of the glasses—to avoid torturing and burning the patient—and therefore it is that in large towns, as in this metropolis, cupping is an art carried on by a distinct class of expert persons. You may apply leeches also to parts where the cupping glasses could scarcely be used.

General bleeding then is best adapted to acute inflammations; and topical bleeding is most appropriate in those which are chronic and slow. But a combination of the two is often highly proper and useful. You may lessen the tension and force of the general circulation by venæsection; but the small vessels of the inflamed part may remain unable to rid themselves of their excess of blood, and continue dilated and full. Such, at least, we may reasonably suppose to be sometimes the case: and

certainly we often act successfully upon that theory; that is, we bleed from the arm, and at the same time, or presently after, we empty the capillaries of the labouring organ, or the neighbouring vessels, by the help of leeches, or by the scarificator and exhausted cup. The good effect of local bleeding, after the general febrile disturbance has abated under venæsection, is often very marked in the relief of *pain*.

I have recommended blood-letting to you when, among other circumstances, the pulse is full and hard; and have stated that the blood should be suffered to flow until some distinct impression is made upon the system. But I wish also to apprise you, that you ought not to be deterred from bleeding merely because the pulse is small. It is very apt to be so in dangerous inflammations within the abdomen; and it is a very curious thing that the pulse will often rise, and the artery develop or expand itself even while the blood is issuing. Now you must look upon *that* circumstance as a distinct impression made upon the system, although it is one of a rather different kind from what I spoke of before. You had better, in my opinion, pause when this effect is fairly obtained: for so great is the tendency to death by syncope in abdominal inflammation that it would not be prudent to urge the influence of the blood-letting further, at one time, than the change I have just mentioned. Wait, therefore, and repeat the venæsection if the circumstances should again render it necessary.¹

¹ The principles laid down in the foregoing lecture have, even while these pages are passing through the press, been arraigned as unsound and fallacious by a physician whose eminence compels attention and respect to every deliberate expression of his opinions. When no less a person than the Professor of the Institutes of Medicine in the University of Edinburgh proclaims his belief that, with respect to internal inflammations, "the principles on which blood-letting and antiphlogistic remedies have hitherto been practised, are opposed to a sound pathology," I, who, for one, still adhere, from conviction, to the same principles which I have formerly taught, can scarcely help taking upon myself to review—very briefly, and in all courtesy and candour—some parts of the Professor's reasonings, and to inquire into their validity.

Although Dr. Hughes Bennett intends his observations to have a general application, he chooses inflammation of the lungs for the main topic and illustration of his argument; and he begins by setting aside as worthless all so-called experience of that disorder, of an earlier date than the invention of auscultation. Cullen and Gregory, and writers yet older than they, were not able (he says) to recognise pneumonia in the living body, and therefore cannot be said to have had any real or certain knowledge of its behaviour under remedies.

Now, it may be granted that neither Cullen nor Gregory could assure himself, as any student of the second year might now do, of the changes wrought by pneumonia in the living lung; but they certainly were competent to ascertain, beyond all doubt, that inflammation was going on somewhere within the chest. Against such inflammation they learned, by watching, the efficacy of early venæsection. They obtained most trustworthy evidence and experience of its power to control *inflammation*; which is precisely what Dr. Bennett contests. That they might not be certain as to the exact seat of the internal inflammation, is nothing to the purpose. This part of Dr. Bennett's argument flies wide therefore of its mark.

I venture to call in question the accuracy of Dr. Bennett's positions, that inflammations can never be cut short; but whether they are to end favourably or unfavourably, must and always will run through a certain course: that it is the physician's proper business to promote rather than to impede this their natural progress: that the formation of pus-corpuscles is essential to the elimination of the products of inflammation from the body. I believe, on the contrary, that inflammation may sometimes be extinguished in its very infancy, before any of its customary products have occurred; and that even after they have some of them occurred, the intensity of acute inflammation may be abated, and its extension stayed, by the judicious use of the antiphlogistic regimen and remedies. I cannot think, as Dr. Bennett seems to think, that every step after the very first step in the inflammatory process is to be regarded as nature's mode, and the only mode, of bringing that process to a satisfactory termination.

In affirming it to be impossible that bleeding from the arm can directly affect the coagulated exudations of inflammation, Dr. Bennett combats a doctrine which, so far as I am aware, has no defenders. Who treats, knowingly, the extravasated products of inflammation by general bleeding? The primary object of that measure is to anticipate, and if it may be to prevent, such products. Still, in my judgment, it is not improbable that the abstraction of blood may sometimes promote the reabsorption of the matters exuded. Whatever may be the ultimate beneficial result (and I believe that it will be immense) of that scrutiny, chemical and microscopical, into morbid textures which modern science has achieved, the information thus obtained is not yet complete or ripe enough to warrant any exclusive reliance upon it as a guide to treatment; more especially when its teaching appears to clash with the prior teaching, for hundreds of years together, of well-conducted though empirical observation.

And see what experience has really attained in this matter. Facts which no one can gainsay, attest the immediate influence of blood-letting in incipient inflammation. The emergence from coma, or from delirium, while the blood is still flowing, in intracranial inflammations—the sudden relief of pain, tightness of the chest, and restricted breathing in pneumonia itself, its

presence being further assured by the pneumonic crackling and the pneumonic sputa,—are familiar facts of that kind. Dr. Alison has testified to the unmistakable benefit experienced by himself under the employment of the lancet in a sharp attack of pleurisy; and I have no doubt whatever that my own life was once rescued by bleeding in inflammation of the bowels, so prompt, unequivocal, and decided was the amendment which followed that remedy. Facts such as these being abundant on all sides, and undeniable, to allege that the patients were ultimately the worse for the treatment pursued—that they would have recovered sooner, or more thoroughly, had no bleeding been instituted,—is a mere begging of the question at issue, which we, of the older belief, may fairly decline to grant.

That which at first sight appears to be the strongest point in Dr. Bennett's argument, is his appeal to the evidence of statistics. But the "numerical method," as it is called, though of excellent use in many researches, and indispensable to the acquirement of exact information of some kinds, has no conclusiveness at all, but, on the contrary, may easily mislead, when it is applied to the *treatment* of separate cases of disease. To be guided by statistical results here, is to adopt the irrational and dangerous rule of prescribing after the *name* of a disorder; whereas each case requires its special study, speaks its proper language, furnishes its peculiar indications, and reads its own lesson. Take the very disorder considered by Dr. Bennett—take pneumonia, as certified to exist by its auscultatory signals. Surely no sane person professes to treat all instances of it in the same manner. The great majority of such cases are, by common consent, treated without venæsection, and probably much as Dr. Bennett would himself treat them; some are properly treated by stimulants even; some by opium; some with mere "expectation." The exceeding value of statistical returns in determining the *causes* of disease has been admirably set forth by Dr. Alison; but, for directing the treatment of individual cases, it is far more profitable (as some one has well expressed it) to *watch*, than to *count*. To use or to withhold a given remedy simply because it is found, by numerical calculation, that in cases nominally the same, recoveries have been more frequent when that remedy was employed on the one hand or omitted on the other, would be to sacrifice the plain and perhaps pressing indications of a particular case, to the statistical averages of diseases having merely a common denomination. To repeat what I have said elsewhere—we do not necessarily take the same symptoms as indications of treatment, which we trust to as signs of disease. We treat, indeed, not the so-called disease, but its accidents; the vital manifestations which proclaim its character and intensity, foreshow its tendencies, and illustrate its course.

With respect to pneumonia, it is very true that there is much less bleeding practised now than formerly; partly, I do entirely believe, from a change in those vital manifestations to which allusion has just been made; partly because we more easily and surely estimate the extent, conditions, and progress of the inflammation. Or rather, perhaps, it should be said (setting aside what may be due to the mere fashion of the day) there is *apparently* less bleeding, because many more instances of disease are brought within the category of pneumonia, by the testimony of the ear. We grant that bleeding, like all other potent remedies, is powerful for evil as well as for good; but we advocate its prudent use, not its careless adoption, or its routine abuse. We believe that the plan which would dispense with blood-letting in all cases of acute inflammation, is too simple and facile to be the right or a safe plan.

It is objected to venæsection that "it deteriorates the blood, rendering it poorer in corpuscles and richer in water," and therefore (presumably) less fit for the purposes of repair. But it should be remembered that the blood is liable to deterioration of a more pernicious kind by the presence and persistence of acute inflammation. Herein consists a marked difference between fevers, and common inflammation occurring in a previously healthy person. In fevers the blood is primarily diseased. In inflammation there is reason to believe that it is the very part inflamed which gradually spreads infection through the general mass of the blood; and this contamination we prevent or limit, if we can arrest the inflammation.

After all, Dr. Bennett admits that though large and repeated blood-lettings are opposed to a correct pathology, benefit may nevertheless accrue from a limited abstraction of blood, when there is no great debility. He even thinks it probable (speaking, however, of what he infers to have been obstruction to the circulation rather than inflammation) that the "inexpressible relief" derived in some cases of "great dyspnoea and pain," from the loss of only a few ounces of blood, may have arisen from its "diminishing the tension of the whole vascular system." In these admissions I apprehend that the whole matter in dispute is virtually conceded. For who shall say, in a given case of severe inflammation, what is a large, and what a small bleeding? These are relative and even convertible terms. So that the Professor here appears to me to come back to the ancient ways, and the accredited practice; which is, neither to exclude bleeding, nor to bleed in excess of the present necessity. So to bleed as to secure the advantages of the remedy, and to avoid its disadvantages, is the precept I believe of all teachers.

I have no room to discuss the question, answered in the negative by Dr. Bennett, whether febrile inflammatory diseases may change their type. He takes pains to show that the process of inflammation, in its several steps, its products, and its local effects, are at all times the same. What he has not shown is, that the human constitution is incapable, from influences to us unknown, of undergoing alterations, in respect to the manner in which it is affected by inflammation, and by the reputed remedies of inflammation. For my own part, I am firmly persuaded, by my own observation, and by the records of medicine, that there are waves of time through which the sthenic and asthenic characters of disease prevail in succession; and that we are at present living amid one of its adynamic phases.

LECTURE XIV.

Treatment of Inflammation, continued. Recapitulation. Bleeding. Purgatives. Mercury. Antimony. Digitalis. Colchicum. Opium. Local Remedies. External Cold. External Warmth. Counter-Irritation.

AFTER pointing out to you, yesterday, the necessity of guarding your patient, as much as possible, from all stimulants and sources of irritation, both internal and external, the avoidance of which constitutes what is called the *antiphlogistic regimen*, I began to speak of the *remedies* of inflammation.

Now the great remedy in acute and dangerous inflammation is blood-letting: and when this remedy is used at all, it should be used freely, and so as to produce a decided impression: and its efficacy for good will always be the greater, in proportion as it is applied in the earlier stages of the inflammation. The objects of the abstraction of blood are two-fold: to lessen the force of the heart's action is one object; to ease the gorged capillaries of the part inflamed is the other. We effect the first of these objects, or both of them at once it may be, by making an orifice with a lancet, in the trunk of some convenient vein or artery, and allowing the blood to escape; we accomplish the second by making little incisions with a scarifier through the skin as near the inflamed part as we can, and forcing the blood through these little wounds by the pressure of the atmosphere: *i. e.*, we take off the pressure from the part scarified, by placing over it a glass cup, from which the air has been in great measure exhausted, and then the unbalanced weight of the atmosphere upon the surrounding surface forces out the blood; or we suffer leeches to scarify the skin, and to suck out the blood. These two modes of drawing blood, from the trunks of the blood-vessels on the one hand, and from the capillaries on the other, we call, respectively, general bleeding, and topical bleeding.

I say the main point to be achieved in general bleeding is so to manage the operation as to make a decided impression, as quickly as possible, upon the pulse or the heart: and to do this we place our patient in an upright position, and make a free orifice in the vein of one or both arms.

And when the force of the general circulation has been thus abated, it will in many cases be proper and necessary to take away blood from the capillaries also, in the neighbourhood of the suffering organ. This is almost always safe and good practice: there can seldom be any reason for abstaining from it, unless the general bleeding has had so great an effect that the abstraction of a few more ounces in any way might be hazardous. But the employment of local depletion presently after general is *then* especially indicated, when the *local* symptoms remain unrelieved; when, although the indirect symptoms which manifest themselves through the medium of the system at large have been moderated by the general blood-letting, yet the direct symptoms belonging to the part, and disturbing its functions, the pain, for example, or the labouring breath, or the stupor, have not undergone a proportional improvement. Under such circumstances, the unloading the oppressed capillaries by means of leeches or cupping-glasses will often be attended with the happiest effects.

I mentioned that the most common way of performing general blood-letting in this country is by venæsection; and that the veins chosen, as the most suitable for that purpose, are the cephalic and basilic veins at the bend of the arm: but that when, from accidental circumstances, blood cannot be obtained easily and abundantly from those veins, any other large and superficial blood-vessel may be opened. It matters little which, in my opinion, so far as regards the effect of the *abstraction of blood* upon the disease. Some persons are fond of opening the temporal artery when the inflammatory disease is situated in or about the head: and certainly, when we see this vessel starting from the surface like a cord, and tortuous from its fulness, and visibly throbbing, we feel tempted to give vent to the blood which is distending it. But arteriotomy is not so easily managed as phlebotomy. It is sometimes difficult to get the blood to flow properly; and it is sometimes difficult to stop its egress when we

wish to do so; and sometimes there are after-consequences which are far from being pleasant: little aneurismal tumours are apt to arise. It is, besides, desirable to avoid the necessity of bandaging the head, in order to restrain the further efflux of blood from the artery. Other practitioners recommend opening the external jugular vein in head cases, especially in children, whose veins in the arm are small. This is a plan which I have never adopted, and which, I am bound to tell you, I do not like: first, because I think it seldom can be necessary; secondly, because I think it is often unsafe.

It is seldom necessary: for in children we can always get as much blood by topical bleeding as will be equivalent to a general blood-letting. And it is unsafe in two ways. In the first place, it is not always an easy matter to stop the bleeding from the jugular vein, especially in a struggling and unmanageable child; and the difference of a few ounces of blood may be a *fatal* difference. Here also any compression of the neck, to stay the hæmorrhage, might affect injuriously the cerebral circulation. Again, there is a distinct and peculiar danger attending the incision of this vein, that, namely, of admitting *air* into it. You perhaps are aware that if air enter a large vein near the heart, and pass on to that organ, it kills outright. If you open the jugular vein of a horse, and blow forcibly into it towards the heart, the animal drops down dead. The celebrated Dupuytren was performing some operation about the neck, in the course of which he cut across one of the veins there situate; some bubbles of air rushed in at its open mouth, with an audible clucking noise, and, in an instant, his patient expired. The same frightful accident has occurred during operations performed in this country, and in America. I was told very lately that in one of our metropolitan hospitals it was thought right, for some reason or other, to bleed an adult patient by opening his jugular vein. The opening was made very near the clavicle, so that pressure between the orifice and the heart was difficult to effect. Of course the blood soon leaves the portion of the vein nearest the heart: and whether by some suction power of the heart itself upon the veins, or how, one scarcely knows, but air rushed in, and the patient was presently a dead man. Perhaps misadventures of this kind may be capable of being prevented by using great caution in such cases; but as it is the etiquette for physicians to direct but not to perform these manual services towards the sick, and as, therefore, I should incur all the responsibility, and at the same time be able to ensure none of the necessary care, I confess that I am shy of recommending venæsection to be made in that particular place.

Whether, all other things being the same, the abstraction of arterial blood may be more or less effectual in restraining inflammation than the abstraction of venous, is more than I can tell you.

When topical bleeding is employed with the view of disburdening the turgid capillaries, it would seem most expedient to get as near to the part affected as we can. To apply, for example, our cupping-glasses or our leeches to the temples, or behind the ears, or just below the occiput, in inflammatory affections of the head; to the chest or the præcordia, when the lungs or heart are the seat of the disease: to the surface of the abdomen, in inflammation of the liver, or stomach, or intestines, and so on. And this is the plan which I have almost always adopted; and with such satisfactory results that I have felt little inclination to try any other. But many persons do believe that local bleeding is more useful when it is performed at some distance from the affected part. They would put leeches, for instance, on the insteps, to relieve an inflamed throat; and they attribute the benefit that ensues to what is called *revulsion*. They suppose that the suction of the leeches solicits the blood, as it were, to that quarter, and diverts it from the vessels of the part that is inflamed. It seems to me that the revulsive influence of *topical* bleeding would be greater in the neighbourhood of the inflamed part than far from it. I know, however, some very practical men who have been much struck with the results of this distant blood-letting, which they had seen practised in the Parisian hospitals. Leeches are also sometimes applied at a distance from the seat of the inflammation, on another principle — that of drawing the blood directly from the veins which communicate with the diseased part. In abdominal affections, in inflammation of the liver or intestines, the French are in the habit of applying leeches in great numbers to the verge of the anus; because, they say, the blood is then abstracted from the very veins through which it is returning towards the already overloaded organs. It is right that you should be aware of these

opinions, and of this practice. I can say but little of it from my own knowledge. I can well believe, however, that it is good and useful practice: but in this country we should find it difficult to persuade many of our patients to submit to have leeches planted round the anus: and I have seldom been disappointed of the benefit I expected from topical bleeding, when it has been employed at the surface, as near the part inflamed as possible.

The *evacuation* next in importance to blood-letting, is *purging*. This is an expedient which in cases of violent inflammation, or high general fever, should scarcely ever be omitted. To keep the bowels what is called *open*, forms indeed a part of the antiphlogistic *regimen*; but in acute inflammatory diseases, active purging is often of very great service. These two points are gained by it. The stomach and intestines are freed from accumulated fæces, or other matters which, by their bulk or their acrimony, might prove irritating: and at the same time depletion is carried on by means of the serous discharge which is produced from that large extent of mucous membrane. There are some cases of inflammation in which the operation of purgative medicines is of especial benefit; as in inflammatory affections of the head, either external or internal, of which part these medicines assist or cause the depletion in a very sensible manner. We have an illustration of this in the paleness of the face, which often, during health, accompanies the action of a brisk cathartic. The usefulness of repeated purgatives is less distinctly seen in inflammations situated within the thorax; although in these cases also they are often very beneficial. They are efficient remedies too in all inflammatory conditions of the liver. But when inflammation has fastened upon the stomach or bowels themselves, although it may be indispensable that they should be unloaded of their contents, which are often composed of irritating ill-digested food, and of morbid secretions no less teasing and hurtful, the propriety of going beyond this point is extremely questionable. I believe that much harm is often done by pressing the inflamed alimentary canal with active purgatives. But to all these points I shall have occasion to return.

Next to blood-letting, as a *remedy*, and of vastly superior value upon the whole, to purgation, in serious inflammations of various kinds, is *mercury*. This mineral is really a very powerful agent in controlling inflammation; especially acute, phlegmonous, adhesive inflammation; such as glues parts together, and spoils the texture of organs. It is of the greatest importance that you should accurately inform yourselves concerning the various effects of mercury upon the system: the changes it produces; the changes it arrests or prevents; the cases in which it does good; the cases in which it does harm; that you should learn, in short, how to wield a very potent, but a two-edged weapon.

If we inquire what mercury does when it is administered to a person in health, we find three very marked effects following its internal use. They vary, indeed, in different cases, and under different circumstances; but we know that the employment of mercury under any of its usual forms of exhibition is often followed by increased watery evacuations from the intestines; or by an increased discharge of bile; or by an increased flow of saliva: that is to say, it determines (as the phrase is) to certain secreting organs—the mucous membrane of the bowels, the liver, the salivary glands; it augments their natural secretion; and in this augmentation of secretion is implied an increased afflux of blood to the secreting part. It is probable that mercury has a similar influence on most or all the secreting surfaces of the body, altering the condition of the capillary circulation throughout. And an explanation of its curative power in inflammation has been drawn from this fact: it has been supposed that mercury thus tends to *equalize* the circulation; that by causing the blood to be distributed in larger quantity than common upon *several* surfaces at the same time, it obviates, *pro tanto*, its excessive congestion or accumulation in any one organ. Whether this hypothesis in respect to the *modus operandi* of mercury be true or not, I will not pretend to say; but it certainly is not an unreasonable hypothesis.

If you push this remedy in healthy persons, other effects ensue: inflammation is actually *produced*; the gums become tender, and red, and swollen, and at length they ulcerate; and in extreme cases, and in young children especially, the inflamed parts may perish: the cheeks, for example, sometimes slough internally. Not only the gums, but the throat and fauces, grow red, and sore, and sloughy.

Now you will do well to observe what is the *character* of the inflammation thus

produced. It is superficial, spreading, erysipelatous: it leads to ulceration without any distinct occurrence of suppuration; the ulcers enlarge. Of the three processes which I formerly pointed out as going on in different degrees, at the same time, in an ulcerated surface, that of absorption is vastly predominant; and you will find that persons in whom this local affection, this condition of the parts within the mouth, has been produced, get rapidly thin: their fat disappears: they become emaciated. That is, the absorption of the old materials throughout the body exceeds the deposit of new matter. Patients who are kept under the influence of mercury grow pale as well as thin: and Dr. Farre, who has paid great attention to the effects, remedial and injurious, of this drug, holds that it quickly destroys red blood; as effectually as it may be destroyed by venæsection. As an example of this he was in the habit of relating in his lectures the case of a lady who was attacked with hæmatemesis: and whose gastric system and liver were gorged with blood. "Her complexion," said the Doctor, "was compounded of the rose and the violet. Under a course of mercury she was blanched, in six weeks, as white as a lily."

There are still other, occasional, effects of the continued introduction of mercury into the body: a peculiar eruptive disease; a peculiar condition of the nervous system: but with these I do not now meddle; they will come under our more particular consideration hereafter. At present I am desirous to place such facts before you as may help you to determine in what cases mercury is a fit remedy for inflammation; in what cases it would be improper to give it. The facts I have already mentioned show that it has a loosening effect upon certain textures; that it works by pulling down parts of the building.

But the great *remedial* property of mercury is that of stopping, controlling, or altogether preventing the exudation of coagulable lymph; of *bridling adhesive inflammation*; and if we, in our turn, could always bridle and limit the influence of mercury itself, it would be a still more valuable resource.

From the little I have now said you will readily understand in what description of cases mercury is likely to be useful. In common adhesive inflammation, whether of the serous or the areolar tissues; whenever, in fact, you have reason to suppose that coagulable lymph is effused, or about to be effused, and mischief is likely to result from its presence, then you may expect benefit from the proper administration of mercury; as an auxiliary, however, to blood-letting, when blood-letting is indicated, — not as a substitute for it.

On the other hand, mercury is likely to be hurtful in those forms of disease "where the morbid action approximates to its own action:" in cases of erysipelatous inflammation having a disposition to gangrene; in scrofulous diseases; in inflammatory complaints attended with general debility, and an irritable condition of the nervous system, or a manifest tendency to take on a low and typhus-like character.

When we have to contend with acute inflammation, and desire to prevent or arrest the deposition of coagulable lymph, our object is, after such bleeding as may have been proper, to bring the system as speedily as possible under the specific influence of mercury. How may this best be done? and how are we to know that it has been achieved?

I will answer the last of these questions first. We know that the whole system has been brought under the specific influence of mercury, as soon as its effects become even slightly perceptible in the gums and breath of the patient; and in adults we cannot be sure of it before. The gums grow red and spongy; the patient complains that his gums are sore; and that he has a metallic taste, a taste like that of copper in his mouth. At the same time, an unpleasant and very peculiar fœtor, easily recognised again when it has been once perceived, is smelt in his breath. These symptoms are enough: you need not in general look for any more decided affection of the mouth, such as ulceration of the gums, swelling of the glands beneath the jaw, and of the tongue, and a profuse flow of saliva. Formerly, when it was believed that the material cause of the disease was carried out of the body with the saliva, the mercurial treatment was continued with the view of producing the discharge of many ounces, and even of a pint or two, in the twenty-four hours: but all that is requisite is that the gums should become distinctly tender, and that the mercurial fœtor should be unequivocally manifest, and that these symptoms should be kept up for a certain time.

Now this is best effected, usually, by giving some form of mercury in equal and

repeated doses, by the mouth. For urgent cases calomel is the best form in which it can be administered: two or three grains given every four or six hours, will generally suffice to touch the gums in the course of thirty-six or forty-eight hours. If it act as a purgative, its specific effect upon the whole system will be postponed by that circumstance; and it then becomes expedient to combine with it just so much opium as will prevent its passing off by the bowels. A quarter of a grain of opium with two grains of calomel—or a third of a grain of opium with three or four grains of calomel—will generally be sufficient to restrain the purgative operation of the latter. When a speedier effect is desirable we give larger doses; such as five or ten grains every three, or even every two hours: or we combine mercurial inunction with the exhibition of calomel by the mouth. It is impossible to lay down any precise rule that will fit all cases.

Blue pill, or else the *hydragryrum cum cretâ*, may, in certain cases, be preferable to calomel; but they must be given in greater quantity. Some practitioners believe that a combination of blue pill and calomel acts sooner, and answers better, than a proportional dose of either, given alone.

This mode of administering mercury, so as to affect the system at large, is eminently useful in many instances of acute phlegmonous inflammation, after bleeding has been carried as far as the circumstances of the case may warrant. I repeat that it must not be allowed to supersede blood-letting, when that remedy would of itself be eligible. Previous bleeding renders the body more readily susceptible of the influence of mercury; and the operation of the mercury comes in aid of the salutary effect of the abstraction of blood. The two remedies accomplish by their joint power what neither of them might be able to accomplish singly.

It is important to know that different persons accept, or resist, the specific agency of mercury, in very different degrees; so that in some patients the remedy becomes unmanageable and hazardous, while in others it is inert and useless. It is most grievously disappointing to watch a patient labouring under inflammation which is likely to spoil some important organ, and to find, after bleeding has been pushed as far as we dare push it, that no impression is made upon his gums by the freest use of mercury. Such cases are not uncommon; and unfortunately they seem most apt to occur when the controlling agency of mercury is most urgently required. On the other hand, there are persons in whom very small quantities of mercury act as a violent poison; a single dose producing the severest salivation and bringing the patient's existence into jeopardy. This history was told to Dr. Farre by a medical man, under whose notice it fell. A lady whom he attended said to him, at his first professional visit to her, "Now, without asking why, or speculating about it, never give me mercury, for it poisons me." Some time afterwards she met with the late Mr. Chevalier, and spoke to him about her complaints; and he prescribed for her, as a purgative, *once*, two grains of calomel, with some cathartic extract. She took the dose, and the next morning showed the prescription to her ordinary attendant. "Why (said he) you have done the very thing you were so anxious to avoid; you have taken mercury." She replied, "I thought as much, from the sensations I have in my mouth." Furious salivation came on in a few hours; and she died at the end of two years, worn out by the effects of the mercury, and having lost portions of the jaw-bone by necrosis.

Another medical man informed me that he knew a person so susceptible of the influence of mercury, that when his wife had rubbed a very small quantity of white precipitate ointment upon her neck, for some cutaneous affection, his gums were tender for three or four days, after his sleeping with her, and slight salivation took place. This did not happen once only, but three several times. On one occasion this same man took two blue pills, as preliminary to a common purge, and he was salivated profusely for six weeks. Cases similar to these occur now and then to most medical men; we cannot tell beforehand in whom such effects are to be looked for, but it is never prudent to neglect any warning which the patient gives of his own previous experience on this point. You will generally find that where the affection of the gums and salivary organs goes on to a troublesome or distressing extent, it has supervened upon the employment of a very moderate quantity of mercury.

So distressing sometimes are these effects of mercury upon the mouth, that I may pause a moment to tell you what I know about the means of remedying them. You

will constantly be called upon to do something for the relief of this disease (for so we must call it), which you yourselves, or some of your brethren, have with the best intentions inflicted. I have tried all sorts of expedients; and I have asked a great number of my friends what is the best plan to adopt in such cases; but I never could get much satisfactory information from them. Some thought purging was the best thing. Others recommended alum gargles, or gargles made with the chloride of soda; and these last certainly have one good effect, that of correcting the fœtor. A dilute solution of chlorine in water, much used at the Middlesex Hospital, is better still. Others believed that sulphur, which has long been prescribed in such emergencies, was really of service; and some advised that the patient should be as much as possible in the open air; a few commended iodine. All admitted that they knew of no certain remedy. Neither do I. But there are two or three expedients which I am confident are often of very great use in checking the violence of the salivation, and in removing the most distressing of its accompaniments. If there be much external swelling, treat the case as being, what it really is, a case of *local inflammation*: apply eight or ten leeches beneath the edges of the jaw bones, and wrap a soft poultice round the neck, into which the orifices made by the leeches may bleed; and I can promise you that, in nine cases out of ten, you will receive the thanks of your patient for the great comfort this measure has afforded him. Pure tannin, moistened and smeared upon the spongy gums, is remarkably efficacious in rendering them the firmer and more comfortable. But this is not always to be procured: and when the flow of saliva, and the soreness of the gums, formed the chief part of the grievance, I have found nothing more generally useful than a gargle made of brandy and water; in the proportion of one part of brandy to four or five of water. This last piece of practice I learned from the present apothecary to the Middlesex Hospital; I have tried it over and over again; and I tell it to you as a thing worth remembering. These little points are by no means to be despised. A very fashionable and successful physician, now dead, used sometimes to say when he met others of his brethren in consultation, "It is all very well to speculate about the exact situation and the precise nature of the disorder, but the question with me is, 'what is good for this, that, or t'other thing?'" A wise physician will seek to combine with an accurate knowledge of disease, and settled principles of treatment, those practical expedients and minor appliances which are picked up by casual experience; which could never have been reasoned out; and which sometimes constitute nearly all that we can do for our patient's benefit.

But to return to mercury as a remedy against inflammation. It is of great service in many cases of *chronic* inflammation; and I may repeat here the observation I formerly made when speaking of blood-letting—that the treatment must *keep pace*, as it were, with the disease. When textures have been slowly altered by a gradual deposition of coagulable lymph, we should gain little or nothing by suddenly or speedily salivating our patient. The lymph, if it can be dispersed at all, must be *gradually* taken up again: and mercury, given with the view of promoting its absorption, must be slowly and gradually introduced into the system; and its specific influence, when at length it is felt, must be sustained for a considerable length of time.

You must not expect any good, but the contrary, from the exhibition of mercury in scrofulous inflammations; and where the scrofulous diathesis is well marked, you should be cautious in giving mercury at any time. But I am certain that many men are *too* scrupulous in this respect; and that, through over-tenderness of your patient's constitution, you may risk his life, by withholding mercury because he shows tokens of scrofula. You may recollect my stating that scrofulous persons are not exempt from attacks of *common* inflammation; and in some cases the possible aggravation of their general ill health, by mercury, is not to be put in competition with the immediate danger from the local inflammation. I have again and again seen scrofulous patients benefited by moderate salivation; which, if it proved injurious at all to their general condition, was certainly less injurious than the unchecked local complaint would have been.

There are some other remedies for acute inflammation which, in this general account of its treatment, I must briefly notice. *Antimony* is one of them; and a very valuable remedy it is in some forms of inflammatory disease. Antimony, properly administered, subdues the action of the heart and arteries, producing nausea, paleness, and sinking of the pulse, and frequently great relief to the local symptoms.

You bring the circulation into that state into which it may be brought by free blood-letting. But when the violence of the inflammatory symptoms recurs again and again, you cannot again and again employ the lancet: or if you do so employ it as at length to extinguish the inflammation, you reduce your patient to a state of pitiable, and even perilous, debility. Now you may continue or repeat the depression of the circulation by means of antimony, without any dread of such subsequent weakness. Antimony, so far as my own observation goes, is admirably suited to cases of active inflammation, in which mercury would either be not so useful, or could not be brought to bear. It is in inflammation of the mucous membrane of the air passages that antimony is so signally beneficial. You will see a patient labouring for breath, unable perhaps to lie down, with a turgid and livid countenance from imperfect arterialization of the blood. He has been ill but a short time; it is an acute affection; and upon listening at his chest you hear that peculiar wheezing sound which we call *sibilus*, in every part of his lungs. I shall have to describe this sound, and its causes, and its meaning, in a future part of the course. You give such a patient repeated doses of antimony; he becomes sick, vomits perhaps, but he feels nausea: his pulse becomes less forcible, his face grows pale, and he can breathe again. The nausea is not a pleasant sensation; but the want of breath is a far more distressing one; and that is greatly mitigated. Perhaps free secretion takes place from the congested membrane, and then the patient is easy and safe. Now you could not effect this change so quickly and readily, or so conveniently, by mercury, and perhaps not at all. Bronchitic affections are very common in children, in whom it is usually difficult to induce the specific influence of mercury.

On the other hand, antimony does not appear to be nearly so valuable a remedy as mercury, when serous membranes are inflamed.

The French and Italian physicians place much reliance upon antimony for the cure of inflammation; and they seem to know little or nothing of the remarkable agency of mercury upon that disease. For my own part, I do not see how any useful comparison can be made between these two substances in respect to inflammation, considered generally, as we are now considering it. There are some particular forms of inflammation to which the one remedy is better suited, and there are others in which the other is most effectual. I must content myself for the present with having adverted to these distinctions.

As to the form in which the antimony should be exhibited, I apprehend that we shall all come at last to freshly-dissolved tartar emetic. The antimonial powder is of very uncertain strength; and the antimonial wine contains too much spirit to allow of its being given in large and frequently-repeated doses. It is a curious circumstance, that although vomiting and purging are apt to be produced by the first two or three doses, they usually cease when the same quantity is persevered with. *Tolerance* of the remedy is established. But although these unpleasant primary effects cease, the curative agency of the antimony appears to continue. When you desire to obtain its full influence in a short time, you may dissolve a grain of the tartar emetic in two ounces of hot water, and give a fourth part of the solution every half-hour. If the patient become pale and sick, you pause awhile and allow him to recover himself; and if the inflammatory symptoms return, you repeat the medicine. It sometimes acts violently upon the bowels, and then it is necessary to add a few drops of laudanum to each dose.

[The remarks of Dr. Watson upon the remedial effects of tartar emetic in inflammatory affections, are perfectly correct in reference to these diseases as they occur in the adult; in the inflammatory diseases of infants and young children, however, the tartar emetic is not a remedy that can be safely employed; it produces in them always very considerable and enduring disturbance of the stomach, and in many cases a state of extreme, and, as remarked by a late writer—Dr. Wilton, (*Prov. Med. and Surg. Journ.*)—even fatal depression of the vital powers.—C.]

Digitalis is another powerful medicine, from which as a remedy for active inflammation much was at one time hoped; but this hope has been in a great measure disappointed. It is not a manageable remedy in such cases. Its singular property of retarding the circulation, of bringing down the number of the heart's pulsations, and

abating its force, led to the expectation that it might render the use of the lancet unnecessary; that it might check the inflammatory process without permanently reducing the strength of the patient. But if you give moderate doses of digitalis, its peculiar effect upon the pulse comes on at very uncertain periods, and may be postponed until it is too late to be of any service. If, on the other hand, you give it in such quantity as speedily to affect the heart's action (which is what we want in acute and serious inflammation), then you are never secure against what may be called its poisonous effects; deadly faintness, frightful syncope, and even death itself. Most practitioners can tell of cases in which patients, who were taking full doses of digitalis, have suddenly expired; and when the remedy has appeared to have had a greater share than the disease in producing the fatal event. There are men, however, and I know one of them, who affirm that digitalis may be given, after due depletion, and in acute inflammation, in very large, and I should say startling doses, with the very best effects—doses which range from half a drachm to half an ounce, and even six drachms of the official tincture. I confess to you that I should be very unwilling to sanction this mode of using digitalis. I never attempt to employ it with the view of knocking down acute inflammation—to which alone you will observe that my present remarks apply. Digitalis is often of great service in other complaints; but I am not at present discussing the remedial virtues of digitalis, or of any other drug, except so far as they relate to the cure of recent and active inflammation.

Colchicum is a drug which is often prescribed in inflammation. It is a most valuable remedy in certain *specific* forms of inflammation. But for repressing common phlegmonous inflammation we have much more certain and better remedies. For this purpose *colechicum* is, I believe, a very unimportant medicine.

I have formerly been asked, by students attending here—and therefore I anticipate the question now—respecting the utility of *opium* as a remedy in inflammation. Certainly opium, like most of our powerful medicines, may do much good, as it may do much harm, in different inflammatory diseases; and it is not very easy to point out clearly, in a general view of the treatment of inflammation, the rules for its administration by which we must be guided in different cases. Yet there are a few general observations which I may make now on this subject.

The administration of a full dose of opium has been strongly recommended after that free and effective bleeding which I have already described. It prevents the rekindling of the inflammation which is apt to result from irritation of the nervous system—a kind of irritation, you will remark, which the copious abstraction of blood is calculated to produce, or to augment if it find it already existing. The opium soothes this nervous irritability; and it must be given, when given at all, in doses which will have that effect. It is best adapted to those cases in which a natural irritability is inherent in the constitution of the patient—to those in which such irritability has been acquired by bad habits of life—and to those in which the local disease is attended with much *pain*, which is in all constitutions a source of irritation.

However, this is a remedy which requires to be used, in inflammation, with great caution and discrimination. In cases of active inflammation within the cranium, its propriety is very questionable. It is apt to confuse both the patient and his physician, who is unable to say, after a full dose of opium has been given, how much of the stupor that follows is owing to the disease, how much to the drug. It is a very ticklish remedy in pectoral inflammations. I believe that by the free use of opium I saved the life of a relation of my own, an old lady, who was in danger of being worn out by the cough and bronchial affection which attended the influenza. On the other hand, I certainly have known more than one person, labouring under extensive and severe bronchitis, so effectually quieted by a dose of the same medicine, that they never woke again. As a general rule, I should say that you must be very careful how you venture upon opium in inflammatory diseases that tend to produce death by *coma*, or by *apnœa*. If there be any unnatural duskiness of the face, if ever so slight a tinge of purple mingle itself with the red colour of the lips, this is an appearance which (with certain exceptions, to be specified hereafter) should warn you against opium. It shows that the blood is imperfectly arterialized; and imperfect arterialization of the blood, as I hope you all know now, either results from, or conduces to, a state of coma.

On the other hand, it is, *cæteris paribus*, in cases where the tendency is towards

death by *asthenia*, that the use of opium, as a remedy for inflammation, is most serviceable. It has a capital effect often, after free bleeding, in cases of peritonitis, and of enteritis. It probably does good in various ways: by quieting the nerves — by sustaining the faltering action of the heart — by keeping the inflamed parts at rest. There are some frightful accidents in which we can expect little from blood-letting, but in which the judicious employment of opium affords some glimmering of hope. I allude to those cases of intense and general peritonitis which arise upon the escape of irritating substances into the cavity of the belly; the contents of the intestines, from ulceration or from external injury; urine from rupture of the bladder; and so on. If there be any hope in such cases, it is to be found in the continued exhibition of opium in considerable doses. But upon all these points I shall go more into detail when we come to consider individual diseases.

A very few remarks, in respect to *external remedies* in cases of inflammation, will terminate both this lecture and what I have to say, thus generally, of the treatment of inflammatory complaints.

The application of external *cold* will aid us very powerfully, in certain serious cases of inflammation; and especially in cases of inflammation within the cranium. It is really wonderful what a sedative and soothing effect this expedient frequently has in allaying delirium, the result of active inflammation of the brain or its membranes. Thin folds of linen, kept constantly moist and cold by cold water, are placed upon and around the shaven head. We often apply ice in the same way. But I need not go at present into any detail on this subject: I will only observe, that we have a most excellent and simple guide as to the probable usefulness of cold applications to the head, in the *sensations* of our patients. It is very lucky that it is so. As long as the cold cloths, or the bags of ice, are pleasant and grateful to the patient, so long we sedulously continue to apply and renew them; as soon as the patient dislikes them, they had better be intermitted.

Cold applications to the *chest*, and to the *belly*, in active inflammation of parts situated within those cavities, have been praised by some practitioners; but I believe are very seldom employed. I have no personal experience either of their utility, or of their hurtfulness. I confess that I should not like to use them. I should fear that the effect of the cold, in driving the blood from the cutaneous vessels, and accumulating it in internal parts, might even be injurious.

The totally opposite measure, that of applying *warmth* to the surface, is of very great service in many cases of internal inflammation, especially in inflammations of the abdominal organs. We speak of cold *lotions*, and of hot *fomentations*. These last are managed in various ways, into which I do not at present enter. They seem to do good by determining to the surface; they promote perspiration; they mitigate pain, and persuade to sleep.

In cases of *external* inflammation, sometimes cold applications are found to be of use, and sometimes warm. In this matter also the sensations of the patient furnish the best criterion. Both of them tend, in different circumstances, to promote resolution. We have an illustration of the beneficial agency of cold applications for this purpose in the treatment of recent burns and scalds, particularly when the injury is superficial, and the skin has not been destroyed. Probably there is scarcely any one present who has not experienced the relief given to the *pain* of a burned finger, by dipping it in cold water; and the return of the pain upon taking the finger out again. The cold may be so constantly applied that the pain will cease to recur when the application is at length suspended. Dr. John Thomson relates a case in which a burned arm was kept immersed in cold water for two days and two nights incessantly; and inflammation was thereby wholly prevented. I have known this expedient fail, however. A nurse in the Middlesex Hospital fell as she was carrying a pail of hot water upstairs, and in her fall thrust one of her arms into the scalding liquid. Without loss of time she plunged the same arm into cold water; but after a while was obliged to desist; the cold immersion bringing on severe rigors.

In erysipelas, I am persuaded that warm fomentations not only afford more comfort, but are more effectual and safer, than cold lotions.

Independently of their occasional influence in promoting *resolution*, warm applications — warm soft poultices for instance — are often used with the view of forwarding *suppuration*. Hence this rule. Whenever resolution of the inflammation is

possible, but suppuration is *likely* to ensue, warm applications are the most proper: because under their use we have an equal chance of obtaining resolution, with less hazard of retarding or rendering untoward the process of suppuration, in case resolution does not take place.

Counter-irritation, by means of blisters, sinapisms, embrocations, irritating ointments, setons, issues, or moxas, is often very beneficial. It probably operates by attracting blood into the neighbouring parts, and in the same degree diverting it from the inflamed part. It is most serviceable in chronic inflammations, and towards the decline of those which are acute. It is particularly adapted to scrofulous affections. There is an objection to the use of counter-irritation during the height of the inflammatory fever, on account of the increase of general irritation which it might then occasion. Neither in local inflammation should counter-irritation be applied *very near* to the inflamed part. Blisters upon the head, or neck, are not proper therefore, at least in the early stages of the disease, in acute inflammation within the cranium; but they are sometimes applied in such cases, with advantage, to the lower extremities. To the chest, however, in thoracic inflammation, and to the belly in abdominal, blisters are often not only perfectly safe, but of the greatest use, as will, I trust, be apparent as we go on.

LECTURE XV.

Hæmorrhage:—most commonly Capillary. Habitual Hæmorrhages. Vicarious Hæmorrhages. Idiopathic Hæmorrhages. Active and Passive. Symptomatic Hæmorrhages. Usual Situations of Hæmorrhage. Symptoms and Diagnosis Principles of Treatment.

IN the course of that somewhat cursory account which I have been endeavouring to give you of the general facts and doctrines of pathology, as a preparation for the better understanding of special forms of disease, we reached, some lectures back, the subject of local plethora, or congestion. From that point our road branched off in three several directions. We have pursued the first and main branch to its termination; that which led to the discussion of inflammation. We must now go back to the same point again, and follow first the one and then the other of the two remaining tracks, which conduct respectively to the consideration of *hæmorrhage* and of *dropsy*. These tracks are shorter than that along which we were last travelling; but they are not uninviting; they will open to us, if I mistake not, some interesting views of the country of which we purpose, in the end, to make a more particular survey.

You are to observe that I treat of *hæmorrhage*, only so far as it falls to the care of the *physician*. The subject is exceedingly full of interest in its relation to surgery: and it will receive at the hands of my colleague all the attention which its great importance, as a surgical accident, demands.

But *we* also, as physicians, have much to do with hæmorrhage; with what, for distinction's sake, I may call *medical hæmorrhage*; which differs in kind, in cause, in its consequences, and in the treatment it requires, from that which surgery contemplates.

In surgical or traumatic hæmorrhage the blood flows from some considerable vessel, which has been cut, or torn, or somehow ruptured. You would greatly mistake if you inferred from that circumstance (as you naturally might) that it is *usually* so—the only difference being in the situation of the vessel—in medical hæmorrhage also.

Yet that is the popular notion. When blood gushes out from internal parts, through any of the natural apertures of the body, the person is said and supposed to have *broken a blood-vessel*.

But this is rarely, though it is sometimes, the case. In nine instances out of ten, if there be any rupture at all, it is rupture of the numerous capillaries only; but even of this there is often no palpable evidence. Blood may exude abundantly from a surface which presents, to the naked eye at least, no appreciable injury or change.

When, for example, hæmorrhage has occurred so profusely from the stomach or bowels that the death which ensued could be sufficiently accounted for by the mere loss of blood, the whole track of the alimentary canal has been diligently scrutinized, and has exhibited no ruptured blood-vessel, no breach or abrasion even of its surface, nor any perceptible alteration of texture. Sometimes its mucous membrane appears, here and there, of a red colour, and, as it were, charged with blood. Sometimes it is pale and transparent, while the vascular net-work visible immediately beneath it is gorged and turgid. Sometimes the whole is colourless, the same net-work of vessels having been completely emptied by the previous hæmorrhage.

The same thing is true of other surfaces of the body: nay, in some rare cases, the process of transudation has been actually witnessed. There are well-authenticated instances on record of *cutaneous* hæmorrhage: where a dew of blood, or of its colouring matter, has appeared upon some portion of the skin, has been wiped away, and has reappeared; and that again and again, without any discernible change of the affected surface, beyond some occasional variation in its colour. Facts of this kind suggested the hypothesis that the exudation of blood from unbroken surfaces takes place precisely as sweat oozes from the skin, mucus from the lining of the bowels, and serum or synovia from the membranes that respectively furnish those fluids; and probably by the very same outlets. And this hypothesis, that the blood proceeds from the same exhalant vessels and apertures, which, in health, pour out the fluids natural to the part, appeared to receive support from the fact, that certain hæmorrhages are ushered in and succeeded by an increased efflux of the fluids which belong to the surface concerned. In hæmorrhages from the mucous membranes the following succession of events is, in some persons, habitual. First, there is an augmented flow of mucus alone; then of mucus tinged with blood; then of pure blood: and the hæmorrhage recedes by a similar but inverse gradation, towards a mucous drain, which itself at length decreases or disappears.

But I am assured by those whose knowledge of minute anatomy is much more accurate and trustworthy than mine, that the hypothesis of a mere *exhalation* of blood is untenable; that hæmorrhage from a surface without rupture of capillary vessels is physically impossible; that if the red corpuscles of the blood, which measure from $\frac{1}{3000}$ to $\frac{1}{3000}$ of an inch in diameter, could pass through lateral pores in those vessels, such pores must be large enough to become visible under the microscope. Nay, it is asserted that while it shows no such pores, the microscope does reveal a multitude of distended and broken capillaries in the structure of organs from which hæmorrhage has proceeded.

I give up therefore the phrase *hæmorrhage by exhalation*, which I have heretofore been accustomed to use in these lectures, and I adopt in its stead the less objectionable term *capillary hæmorrhage*. The distinction is broad enough between bleeding from a papable leak in a large vein or artery, and bleeding from countless capillaries, whether these be torn or entire.

Now, although internal hæmorrhage may happen in other ways; as from the bursting of an aneurism, or from an opening made in a large vessel by progressive ulceration; yet in by far the greater number of cases it takes place from innumerable capillaries. Capillary hæmorrhage is the rule — other modes of hæmorrhage furnish the occasional exception.

I must exclude, however, from this general statement one very important hæmorrhage. In the brain, the former exception becomes the rule. In most cases cerebral hæmorrhage results from the rupture of a blood-vessel of appreciable magnitude.

There are various kinds of capillary hæmorrhage. I will bring them before you, in succession, as clearly and as concisely as I can.

In the first place there are hæmorrhages which, although they do not belong to the state of health, if we take mankind in general, yet when they do occur can scarcely be called diseases. There are some persons — I believe I may say there are many persons — who are subject, during the greater part of their lives, to discharges of

blood; which happen again and again, commonly at regular intervals, without any perceptible detriment to the general health, independently of any obvious exciting cause, and (as it would seem) from some inherent property or necessity of the system.

Hæmorrhages thus occurring, I will call *habitual hæmorrhages*. They proceed more commonly from the rectum, and from the nostrils, than from any other parts; although instances are recorded of their taking place from the bladder and from the bronchi. Appertaining to the original constitution of the body, this disposition to periodic hæmorrhage has been sometimes observed to be hereditary.

You will at once be struck with the analogy which obtains between these habitual hæmorrhages occurring in either sex, and the monthly discharge which is peculiar to the female. The analogy is even closer than it may at first sight appear: but it is more distinctly marked in some individuals, liable to habitual hæmorrhage, than in others. It was one of the singular notions of the celebrated phrenologist, M. Gall, founded upon this analogy, that there is such a thing as male menstruation. The points of resemblance between the two phenomena will be manifest in the following summary of the characters belonging to habitual hæmorrhage.

Like the catamenia, these hæmorrhages do not ordinarily prevail throughout the whole course of life. In most cases they do not commence before the period of adolescence; and they cease altogether, or recur at distant intervals only, in declining age. Their first eruption is sometimes preceded by a state of general indisposition, more rarely by slight febrile disturbance, and even (according to some observers) by a sort of chlorosis similar to that which affects young girls in whom the menstrual evacuation is delayed or suspended. The hæmorrhage sometimes occurs at precisely regular intervals, and by *monthly* periods more commonly than any other: being announced, on each occasion, by the same preludes, proceeding from the same part, continuing for the same space of time, and furnishing always about the same quantity of blood. Its accidental interruption is almost uniformly the cause or the consequence of some derangement of the health: and when it becomes excessive, it becomes, like too profuse menstruation, a disease.

It forms a very curious part of the general history of hæmorrhages that they are not unfrequently *vicarious*, or supplemental, sometimes of each other, but more often of the monthly discharge from the uterus. Females are liable to perverted menstruation (so to call it) through other channels than the natural one: and here again the analogy between catamenia and habitual hæmorrhage comes into view. The hæmorrhages which belong to the constitution are apt to wander in their seat. As bleeding from the lungs, stomach, rectum, or skin, sometimes follows upon the suspension of the menses, so bleeding from the bladder, from the mouth, and from other parts, has been occasionally observed to succeed the suppression of habitual hæmorrhoids.

These hæmorrhagic deviations take place commonly by the same organ on each occasion; more seldom by different organs in succession. It is almost always in this supplementary manner that the rarer forms of hæmorrhage occur, and those of the skin in particular.

This singular migration, this interchange of place between certain hæmorrhages, seems calculated to throw some light upon the obscure doctrine of *revulsion*; a doctrine to which I have already more than once referred, and which, though it is very imperfectly understood, is of frequent avail in the practice of physic.

Vicarious hæmorrhage always denotes a disordered state of the general health; and must be considered, in itself, as a malady.

Again, there are certain forms of hæmorrhage, not habitual, which may be denominated *idiopathic*: inasmuch as they are apt to arise without any perceptible connexion with antecedent local disease.

In other respects, however, they differ considerably, and require to be further distinguished: and the terms *active* and *passive*, which are in common use, will sufficiently express the two forms of idiopathic hæmorrhage that I wish to bring under your notice.

Active hæmorrhage is preceded by active congestion, and therefore is akin to inflammation; and it often requires the treatment of inflammation.

Passive hæmorrhage often occurs without any apparent previous congestion of any kind. Hæmorrhage of this passive character has been ascribed to some change in the condition of the small vessels or channels through which the healthy exhalations are

transmitted; the change being of the nature of morbid debility or relaxation. This view of the matter derives its chief support from the occasional efficacy of *astringent* substances (either applied locally, or taken into the system) in checking the effusion of blood, when other remedies have failed. But as we are forbidden to speak of hæmorrhage by exhalation, we may suppose that in these passive hæmorrhages the capillary blood-vessels have somehow become tender and fragile, so as to give way and spill their contents under the ordinary pressure of the circulating blood.

A more probable hypothesis perhaps is that which supposes some alteration in the condition and consistence of the blood itself; which thus becomes attenuated, and capable of passing through channels or orifices that healthy blood, under ordinary circumstances, cannot penetrate. We know that the serous ingredient of the blood may and does filter through the pores of the minute vessels, and we may conceive that with it may at the same time transude the hæmatosin or colouring matter of unhealthy blood; and this supposition is consonant with the fact that hæmorrhages are known to occur where the blood is more thin, pale, and serous than common: and still more remarkably where that fluid has undergone a demonstrable change in its chemical nature, or is even visibly altered in its sensible qualities; as, for example, in certain cases of purpura and sea-scurvy. And hæmorrhages of this kind are often cured by measures calculated to repair the blood, to restore it to its natural condition by improvement in diet, or by food of a peculiar kind, such as the juice of lemons.

Whatever may be the true explanation of the differences in question, there can be no doubt that they exist, and are often strongly pronounced in cases of hæmorrhage, which, inasmuch as they cannot be traced to any pre-existent local disease, we class together as *idiopathic*. And it will be worth while to run over the distinctive characters of active and passive hæmorrhage, as in well-marked cases they are broadly and decidedly visible.

Active hæmorrhage (which is preceded, I repeat, by active congestion) occurs principally in persons who are young and robust, who live fully, and lead indolent lives, and are subject to the influence of those causes which tend to generate plethora. Occasionally the hæmorrhage can be traced to some exciting cause; it may be exposure to heat, strong mental emotion, violent exercise, or great bodily effort. More frequently, perhaps, no exciting cause is apparent. It is sometimes ushered in by a set of symptoms expressive of what has been called the *molimen hæmorrhagicum*. The patient experiences a general feeling of indisposition, with wandering and obscure pains that gradually settle in the part from which the blood is about to be discharged. A series of local symptoms, such as a sensation of weight, or of tension, or of heat and tingling, sometimes a slight degree of turgescence and redness, and a visible fullness of the larger veins, indicate the afflux of blood towards the labouring organ, and the parts in its vicinity: while chilliness, paleness, and shrinking of distant parts, and especially of the feet and hands, denote an opposite condition of the circulation in *them*. And to this state of things there often succeeds a general increase of heat, with a frequent, full, and bounding *pulse*,—a pulse which is so characteristic sometimes, as to have acquired a name: you may often hear or read of a *hæmorrhagic pulse*. The blood, when at length it breaks forth, commonly escapes with rapidity; is of a florid colour; proceeds from a single organ; and readily coagulates, though it does not always separate distinctly into serum and crassamentum. While it is flowing, the signs of local congestion diminish and disappear; warmth returns to the extremities, and the pulse regains its natural strength and frequency. The patient becomes conscious of a sensible relief, and feels stronger and more lively than before. This kind of hæmorrhage is, in some sort, its own remedy; it ceases in virtue of the discharge of a certain quantity of blood, and it is followed by morbid consequences only when that quantity has been excessive, or when it inflicts some mechanical injury upon the parts along which the blood passes.

I said that active hæmorrhage is preceded by active congestion, and is consequently akin to inflammation. Perhaps it may be more true that in some of these cases we actually have the initial stage of inflammation, of which the hæmorrhage proves the natural cure; strangling it in its birth: that remedy being applied, in the very moment when it is most effective, which I told you, in the last lecture, was the most potent of all the remedies of inflammation, namely, *early loss of blood*.

Passive hæmorrhage on the other hand is characterized by circumstances of an ex-

actly opposite nature. It occurs in those who are naturally feeble, or who have been debilitated by disease, fatigue, insufficient nourishment, great evacuations, or the depressing passions. It is not, in general, announced by any precursory symptoms, nor attended by any reaction. The effused blood is of a dark colour, serous, and but little disposed to coagulate: and it often is poured forth from several parts of the body at the same time. If the quantity lost be at all considerable, the natural debility of the patient is rapidly augmented: his face becomes pale, and his body loses its heat. The hæmorrhage leaves him in a worse condition than that in which it found him. The flow of a certain quantity of blood is not, as in the cases of *active* hæmorrhage, suspensive of its further effusion; frequently, indeed, passive hæmorrhage resists the means opposed to it the more, in proportion as it has continued longer, or has been more profuse.

Hæmorrhages of the kind I have now been describing—that is to say, depending upon no palpable disease of any organ, and, therefore, idiopathic—are of no uncommon occurrence, whether we regard the active or the passive form in which they appear: but by far the greater number of capillary hæmorrhages are *symptomatic*; that is, they result from some previous disease, either in the organ from which the blood proceeds, or in some other organ connected therewith by community or dependence of function.

These secondary or symptomatic hæmorrhages are preceded by congestion, but for the most part the congestion is not of the active, but of the mechanical kind; and has more to do with the veins of the part than with the arteries.

Thus we have hæmorrhage from the bronchial membrane, in consequence of crude tubercular matter in the lungs filling up a portion of the pulmonary tissue, and obstructing the circulation of the blood through it. This is an example of symptomatic capillary hæmorrhage, depending upon previous disease in the organ itself from which the blood proceeds.

In some of these cases the presence of pyrexia renders it probable that the hæmorrhage is the consequence and the relief of active congestion, provoked by the irritation of tubercles; rather than the result of a mechanical obstruction of the circulation.

Again, we have hæmorrhage into and from the lungs, as a consequence of such disease of the heart as mechanically impedes the return of the blood from the lungs to that organ: a narrowing of the mitral orifice, for instance. Here the blood is barred up, as it were, in the lungs, till at length the capillaries, incapable of further distension, are lacerated under the internal stress. In precisely the same way blood is poured out from the mucous membrane of the stomach and bowels, in consequence of disease in the liver, obstructing the portal circulation. These are examples of symptomatic hæmorrhage, depending upon previous disease, not of the organ itself from which the blood proceeds, but of another organ intimately connected with the former.

When I say that hæmorrhage into and from the lungs may result from such disease of the heart as implies an impediment to the circulation, you must not suppose that the lungs are the only channel through which the mechanical congestion can be relieved. Disease of the central moving organ of the circulation leads often, at length, to *universal* venous congestion; and the hæmorrhage, which is apt to be the consequence of such congestion, may burst forth from any part where the veins are so overloaded. Hæmorrhages from various portions of the mucous membranes are in truth very common effects of cardiac disease.

The influence of mechanical congestion as a direct cause of hæmorrhage is sometimes very distinctly seen in the bodies of persons who have been hanged. You know that when suffocation has been produced by suddenly cutting off the access of air to the lungs, the right side of the heart, the great veins, and indeed the venous system generally, become loaded and distended with dark blood. Dr. Yelloly examined the stomachs of five men who had been executed by hanging: he found them all exceedingly vascular: and in two of the five cases, blood was actually extravasated, and adhering to the surface of the membrane. There had been, in short, unequivocal hæmorrhage.

There are several things, worthy of notice, in respect to capillary hæmorrhage, of whatever kind.

In the first place, it occurs much more frequently and readily from some tissues of the body than from others : and most especially of all, from *mucous* surfaces. Thus we have hæmorrhage from the mucous membrane lining the nasal cavities ; from the pulmonary mucous membrane ; from the stomach and bowels ; from the urinary organs ; and from the uterus ; constituting distinct forms of disease, which we are, by and by, to investigate more particularly. *Epistaxis, hæmoptysis, hæmatemesis, melæna, hæmorrhoids, hæmaturia, menorrhagia*, are names descriptive of hæmorrhage, as it is apt to occur from different parts of one or other of the three tracts of mucous membrane met with in the body : and you will find that these comprise very nearly all the complaints enumerated by nosological writers under the head of hæmorrhage.

Now this is a very remarkable fact : and very interesting questions arise out of it. Has it any relation to the manner in which these membranes, and the tissues subjacent to them, are supplied with a capillary circulation ? or may the fact be explained by the laxity of their attachment, which facilitates and favours the accumulation of blood in the vessels of the submucous tissue ? or do the minute blood-vessels belonging to the mucous membranes receive a less firm support from the tissue in which they lie, than those belonging to membranes of closer texture ? Whatever answers may be given to these questions, you will do well to recollect the fact which has suggested them.

Capillary hæmorrhages are not, however, exclusively confined to mucous surfaces. They are liable to occur, but much more rarely, from serous membranes. In the majority of cases, however, in which blood is found effused into any of the serous sacs, it has either been an event of inflammation, or the blood has been poured out from an accidental opening in some considerable vessel. Cutaneous hæmorrhage is also very rare ; probably because the cuticle opposes a barrier to the exit of the blood : for the little red spots which characterize purpura are in fact hæmorrhages, although the blood has not penetrated the epidermis. There are cases, however, as I mentioned before, in which blood has transpired, in a sort of dew, from the external surface of the body.

Another important general fact in respect to capillary hæmorrhages is, that they proceed more frequently from certain parts of the mucous membranes than others, according to differences of *age*. Thus in children they are most common from the membrane that lines the nasal cavities ; in youth from the mucous membrane of the lungs and bronchi ; in the middle years of life, and towards its decline, from the rectum, uterus, and urinary organs. I should add here, from the blood-vessels of the brain, in old age ; except that this, as I have already intimated, is not (speaking generally) *capillary* hæmorrhage.

Of course when I say that, in the instances specified, the blood is *commonly* poured out by the capillaries, you will understand that the hæmorrhage *sometimes* occurs from the laying open of a single vessel of some magnitude. Thus hæmorrhage from the fauces may be the result of ulceration there, which has penetrated the coats of a vein or artery : hæmoptysis is occasionally produced by the laceration of a blood-vessel during the softening and expulsion of tubercles : hæmatemesis sometimes is the consequence of a breach made in a considerable blood-vessel during the progress of cancer of the stomach, or by the extension of small corroding ulcers : hæmorrhage from the bowels is no uncommon effect of ulceration, such as happens in fever, of the mucous follicles of the small intestine : calculous matter in the kidneys will often lead to the rupture of some of the blood-vessels there, and to the discharge of blood by the urethra. Aneurisms also may burst into almost any part of the body. But events of this kind are infrequent when compared with capillary hæmorrhages from the same internal parts.

In the head, however, the ratio is reversed. Blood does sometimes, I believe, proceed from the hair-like vessels of the brain or of its membranes, but much more commonly cerebral hæmorrhage is caused by the giving way of a diseased *artery* in the brain.

How, in all these cases, to distinguish whether the blood has oozed out by many small ruptures from a surface, or has escaped from a hole in the sides of a vein or artery, will form matter for future inquiry. Sometimes we *can* make the distinction ; and sometimes, it must be confessed, we cannot.

You will readily understand that hæmorrhage must vary greatly, in respect to its

importance, and to the danger which it implies, according to the part from which it proceeds, and the circumstances under which the blood is poured out. It sometimes happens that death ensues from the mere loss of blood; either at once, by one profuse bleeding, or more slowly, by repeated bleedings which we are unable to restrain: but this is comparatively rare, and when it does happen, the blood is generally found to have proceeded from one considerable vessel, which has been ruptured or eroded. The case approximates to traumatic hæmorrhage, except that we cannot cut down upon and tie the injured vessel. Much more commonly danger arises from the presence and pressure of the extravasated blood in and upon internal parts: upon the brain, for example, in cerebral hæmorrhage; in the lungs, in pulmonary.

The symptoms also are liable to much variation in different cases. Even the diagnosis of hæmorrhage is not always equally easy or certain. When the part into which the blood is directly poured communicates with the exterior of the body, the expulsion of some of that fluid will, generally, sooner or later, demonstrate the case to be one of hæmorrhage. I say *generally*, because cases have been known to occur, in which patients, previously in a state of great weakness, have died outright, by syncope, from the mere extravasation of the blood, and before any of it made its way out of the body. The stomach and bowels have been found full of blood, when none had passed either by vomiting or by stool. And when the blood does make its appearance outwardly, it is sometimes not easy to determine whether it has come from a certain organ, or from the parts that lie between the same organ, and the natural outlet by which the blood ultimately escapes. For instance, it is sometimes a matter of uncertainty whether the blood, in hæmaturia, proceeds from the kidneys, or the bladder, or the urethra.

The blood, itself, when it reaches the exterior, will generally be more fluid, and brighter, in proportion as it is effused in greater quantity, and nearer the surface; more in clots, and darker in colour, in proportion to the length of time that it has remained within the body after its escape from its proper vessels: and this length of time may depend upon the smallness of the quantity of blood effused, and the consequent tolerance of the organs through which it may have passed; or, upon the actual space traversed. Respecting the *colour*, however, of the effused blood, I shall have some curious explanations to offer you when I come to speak of hæmatemesis as a disease. It would be superfluous to enter upon them now.

If the site of the hæmorrhage do not communicate with the external air, we are without that certainty which results from the actual spectacle of the blood. But in such cases we are much assisted by local disturbances of function, springing from the pressure upon, or the laceration or distension of, the suffering organ, or of the parts contiguous to it. And we may derive good information from observing the indirect symptoms which declare themselves through the system at large; many of which indirect symptoms are the same whether the blood reach the exterior or not. They principally vary according to the quantity of blood poured out, and to the *rapidity* of its effusion; and some difference will occur according to the age and strength of the patient.

Some of these indirect symptoms have not always been imputed to their true cause. Paleness of the face, feebleness of the pulse, coldness of the extremities, and a tendency to syncope—symptoms which are apt to be connected with hæmorrhage—have sometimes been ascribed to the alarm and sense of danger which the sight of the blood is calculated to produce on the mind of the patient. This may, to a certain extent, be sometimes true; but the explanation cannot apply to those cases in which the hæmorrhage is strictly confined to the interior of the body, yet in which the symptoms just alluded to are often strongly marked. They *then* depend—and probably in all cases they *chiefly* depend—upon the actual abstraction of the blood from the circulation.

The management of individual cases of hæmorrhage must be mainly regulated by the particular circumstances under which they occur. The few observations that I have at present to make respecting their treatment cannot be otherwise than very general.

But a preliminary question, of some importance, presents itself. Is it in all cases of hæmorrhage proper, or safe, to attempt to stop the bleeding?

Without going into detail, it may, I think, be laid down as a rule, that what I have

called habitual hæmorrhages ought not to be interfered with, so long as they have no perceptible injurious influence upon the health, and so long as they proceed (as they mostly do) from parts of which the *structure* is not likely to be spoiled, nor the *function* impaired, by the repeated passage of the blood. The most common seat of these habitual hæmorrhages I have stated to be the rectum;—to which the two conditions just mentioned are, fortunately, both of them applicable. Epistaxis supplies a less frequent example of the same kind. When they deviate from their usual channel, and are transferred (as it were) to some more important organ, it will generally be right, among other remedial measures, to endeavour to *recall* the original hæmorrhage. It is very seldom that the metastasis takes place *for the better*—i. e., from a part where the bleeding is attended with danger, to one where it is comparatively harmless.

However, when these habitual hæmorrhages happen, as they often do, in plethoric persons; and when they are urged and kept up, as they frequently are, by intemperate and luxurious habits; we ought not to content ourselves with merely looking on. Hæmorrhoids often performs the office of a safety-valve in such persons; and there are many who have what are called bleeding piles, and who would rather continue to have them, than submit to any change in their mode of life, or to the employment of other means of evacuation. Certainly these are cases in which nothing should be done to stop the bleeding; yet such patients ought to be told that the hæmorrhoidal discharge is but a precarious, and often an inadequate relief of the plethora; that while the plethora is suffered to exist there is danger of a cessation of the piles, and of the supervention of serious or fatal affections of other parts, and especially of the head. Apoplexy, from cerebral hæmorrhage, has frequently been known to follow hard upon the suspension of constitutional hæmorrhoids. These patients should be admonished also that the discharge of blood from the vessels of the rectum may become excessive; that if it be aggravated by exercise or in any other way, it may lead to inflammation about the anus, and to great inconvenience; and that there are safe and tolerably sure methods of getting rid of the plethora (which is what chiefly constitutes the danger of such cases), if they will submit to the observance of them. It is in the intervals between the hæmorrhages that the danger of which they are in some sort the token may best be met.

Again, it will seldom be proper to employ *direct* expedients for stanching the flow of blood, in the small class of active idiopathic hæmorrhages; unless the quantity lost is so great as to endanger the safety or the well-being of the patient. Such hæmorrhages have commonly a tendency to cure themselves, by relieving the general plethora, or the local congestion, on which they depend. For these hæmorrhages, which bear so strong an analogy to inflammation, the *treatment* of inflammation may often be requisite, as an indirect mode in which their amount may be moderated, and their recurrence combated.

With these exceptions, both direct and indirect measures are to be used, for arresting the effusion of blood as speedily as may be.

To this end, the patient is to be surrounded as much as possible with cool fresh air, and kept in a state of absolute quiet. All motion of the body and emotion of the mind, all kinds of stimulating food and drink—everything, in short, which has a tendency to hurry the circulation, should be diligently avoided; and that position of the body should be chosen which is the least favourable to the afflux of blood towards the part affected. The horizontal posture will be proper in hæmorrhage from the bowels, the uterus, or the urinary organs. In epistaxis, and in cerebral hæmorrhage, the head should be raised.

In two words, the *antiphlogistic regimen* should be strictly enjoined in all cases of hæmorrhage sufficiently severe to require medical assistance.

Of the actual remedies used for checking the further escape of the blood, one of the most important has already been alluded to—I mean venæsection. Herein we are guilty of homœopathy; to prevent bleeding, we draw blood. After what was stated respecting the use of blood-letting in inflammation, I need not dwell upon the objects aimed at by this measure: they are, briefly, to abate the propulsive force of the heart's contractions, to lessen general vascular tension or plethora when it exists, to remove local congestion, and to divert the current of the blood from the suffering organ. The method, and the amount, and the repetition of the blood-letting, must of course be regulated by the circumstances of each particular case. And the same

objects may sometimes be effected by other modes of general depletion, especially by the use of purgative medicines.

Another important remedy for inward bleedings is *mercury*. Whatever may be the *modus operandi* of that mineral, the fact is certain, that hæmorrhage, which had resisted other modes of treatment, has, in very numerous instances, ceased at once upon the occurrence of a moderate degree of salivation.

Next to blood-letting and mercury, *astringents* constitute the great resource against actually existing hæmorrhage: and among these, *cold* is one of the chief. It may be placed in direct contact with the bleeding surface:—as when ice is swallowed to restrain hæmatemesis; or cold water injected into the rectum in excessive and exhausting hæmorrhoids; or into the vagina, in flooding from the uterus. Or it may be applied to the surface of the body, as near as possible to the seat of the hæmorrhage; as to the nose and forehead in epistaxis; to the chest in hæmoptysis; to the epigastrium in hæmorrhage from the stomach; to the lower part of the abdomen, or to the perinæum, in hæmorrhage from the intestines, uterus, or urinary organs. But the influence of cold in constringing the smaller vessels is not confined to the part with which it is in contact; it will stop hæmorrhage by the sympathetic shrinking which it produces in distant parts. Epistaxis, for example, has often been arrested by the sudden apposition of cold water to the neck, back, or genital organs. The nursery remedy consists in slipping a cold key down the back between the clothes and the skin.

Of even the mischievous power of cold in this way we have continual illustration in the suppression of the catamenia by cold and wet accidentally applied to the feet.

There is a long catalogue of medicinal substances which are esteemed to possess more or less of a specific virtue, when taken internally, in checking the flow of blood. Most of these are of an astringent nature, and some of them are eminently useful. The acetate of lead enjoys, in this country, a higher character, perhaps, than any other of these substances.

Many vegetable matters, and some artificial compounds, frequently employed in internal hæmorrhages, seem to owe their astringent and styptic properties to the gallic acid which enters into their composition. Such are the rhatany root, uva ursi, bistort, tormentil, the pomegranate, kino, catechu, the several preparations of gall-nuts, and the nostrum called *Ruspini's styptic*. It is better, however, in appropriate cases, to give the gallic acid itself, which may now be obtained in substance, in the form of a grey crystalline powder.

The power of arresting internal hæmorrhage has also been confidently ascribed, by different persons, to nitre given in large doses, to the mineral acids, to the muriated tincture of iron, to alum, to the oil of turpentine, to the secale cornutum or spurred rye, to the matico leaf, and to various other substances, a more particular account of the rules and indications for administering which, I may return to, when I have to speak of individual hæmorrhages.

LECTURE XVI.

Dropsy: its General Pathology. Passive Dropsy; Cardiac, and Renal. Active, Acute, or Febrile Dropsy. Prognosis; and General Principles of Treatment in Dropsies.

THERE remains now only one subject, of the pathology of which it will be convenient, and, I hope, instructive, to take a short general view, before we enter upon the consideration of special diseases. I proceed to speak of *Dropsies*: by which I mean collections of serous liquid in one or more of the shut cavities of the body, or in the areolar tissue, or in both, independent of inflammation.

We have already considered serous effusion when it occurs as an effect or event of inflammation. We are commonly able to say of this, that it *has* originated in inflammation; either from its being mixed with some of the less equivocal *products* of that disease, such as coagulable lymph; or from its having taken place while *symptoms* of inflammation existed. But there are numerous examples of serous accumulation, which cannot with any show of reason be regarded as events of inflammation. It is to these that I would apply the simple term dropsy. The liquid collected is serous; it is not the liquor sanguinis; it holds no fibrin in solution. This is one main distinction, which you will do well to bear in mind, between the generality of inflammatory serous effusions, and dropsies.

It has been said — and said with much truth — that dropsy is rather a symptom of disease, than a disease in itself. And it has been affirmed that it would be more philosophical and scientific to treat of the original malady upon which the effusion or accumulation depends; to erase dropsy from the list of substantive diseases, and to place it in the catalogue of mere symptoms.

But this, in my mind, is a very mistaken view of the matter. For, first, it is oftentimes uncertain, while the patient is yet alive, what or where the primary disease may be; and even after death we sometimes can discover no organic change that would satisfactorily account for the effusion. Practically speaking, in such cases the dropsy *is* the disease, and the sole object of our treatment.

And, secondly, dropsy is, in fact, to a medical eye, in *all* cases, something more than an effect or symptom of disease. The imprisoned liquid is often a *cause* of various other symptoms; embarrassing, by its pressure, important functions, and even extinguishing life. The removal of the dropsy (although its original cause, of which it was a symptom, may remain behind, untouched, to be again productive of effusion under circumstances favourable to its operation)—the removal of the dropsy will often restore a person to comparative comfort; or even to what, so far as his sensations, and powers, and belief are concerned, *is*, to him, for the time, a state of health.

You see, then, already, that in a dropsical person, whose dropsy depends upon organic disease, there are two sets of symptoms to be distinguished: those, namely, which depend on the primary disease, and those which depend on the collected fluid. The latter, often the most grievous, are often to be got rid of: the former, frequently permanent, are frequently also but little complained of or felt by the patient, except when effusion is the result.

Some persons, I fancy, have regarded dropsy as a less attractive subject of investigation than it might be if it were less frequently, in its nature, incurable. But as far as the dropsy itself is concerned, the complaint often *is* curable; and there are some forms of dropsy that are curable in a more absolute sense: that is, both the effusion, and that condition which was the physical cause of the effusion, are sometimes remediable.

Besides, it is our business to cure when we can; but whether we can cure or not, to relieve and palliate human suffering; and this, under Providence, we are able to do, in many or most cases of dropsy, to a very considerable extent.

Wherever there is a shut sac, or wherever there is loose and permeable areolar tissue, there we may have dropsy.

Thus there may be dropsy of the ventricles of the brain, or of the meshes of the pia mater, leading to death by *coma*: of the pleuræ, of the areolar texture of the lungs, or of the submucous areolar tissue of the glottis, any of which may cause death by *apnœa*: of the pericardium, producing death by *syncope*. I mention these instances in particular, to show that almost every mode of dying may result from dropsical effusion; and to win your attention to a disorder so full of peril.

When the cerebral ventricles are distended with water, we express the diseased condition by the term *hydrocephalus*. When serous liquid collects in the pleuræ, or in the pericardium, we say that the patient has *hydrothorax*, or *hydropericardium*. If the cavity of the peritoneum be the seat of the effusion, we call the complaint *ascites*. When the areolar tissue of a part becomes infiltrated with serous fluid, the part is said to be *œdematous*; and *anasarca* is the name given to the more or less general accumulation of serum into the areolar tissue throughout the body, and especially to visible subcutaneous œdema of considerable extent. Finally, the term

general dropsy signifies the combination of anasarca with dropsy of one or more of the large serous cavities.

Other local dropsies indeed there are; but as they belong entirely to surgery, I need not enumerate them.

Now what reasonable account can be given of these remarkable conditions? How is it that the hollows and interstices of the living body, or of parts of the body, become thus water-logged?

To solve this question, we must carry in our minds some physiological recollections.

The closed cavities, or the interstitial tissues, within which the fluid of dropsy is confined, are kept moist, during life and health, by a continual serous secretion from their surfaces, and they are kept *merely* moist, for the fluid thus constantly secreted is as constantly re-absorbed into the circulation.

When these tissues or cavities, without having undergone inflammation, become filled and distended with the serous fluid which they habitually secrete, one of three things must have happened. Either the quantity of fluid exhaled has been augmented, the absorption remaining the same; or the absorption has been diminished, the exhalation continuing the same; or else the exhalation has been increased, while at the same time the absorption was either lessened or not proportionally increased.

The last is a mixed case; and we need only consider the two others.

Now the balance between exhalation and absorption is often deranged, and dropsies do actually arise, in each and all of these ways.

It will best suit my purpose to speak first of those dropsies which are occasioned by defective absorption, and which are usually called chronic or passive dropsies.

The direct agency of the blood-vessels in the production, as well as in the removal, of dropsy, although indicated by many common and obvious facts, has not been generally recognised till a comparatively recent period. Perhaps I should rather say that more importance used to be assigned, in these respects, to the agency of the lymphatic absorbents, than they are really entitled to. You will find that pathologists, even in modern times, speak of a want of tone, of deficient energy, in the absorbents, as a cause of dropsical accumulations; the superfluous fluid of the part is not adequately taken up (they say) by the enfeebled absorbents, meaning the absorbents strictly and anatomically so called. And this view of the matter, connecting dropsy always with debility as its cause, has led to a corresponding plan of treatment: the object aimed at being the stimulation of the absorbents to more energetic action.

But to the doctrine that dropsy is a consequence of the deficient action of the absorbents, this obvious difficulty presents itself,—that absorption really goes on, and goes on very actively, in dropsical patients: their adipous matter disappears, they become wretchedly thin. There is no complaint in which wasting and emaciation go to a greater extent than in dropsy. You will find also that persons labouring under anasarca are readily enough affected by mercury; which must of course be absorbed before it can produce any of its specific effects.

It must be confessed that our knowledge respecting the mechanism of absorption is neither complete nor certain; but there is good reason for supposing that the process is shared among the lacteals, the lymphatics, and the *veins*; and it is probable that it may be distributed between these sets of vessels somewhat after this manner;—that the lacteals absorb the chyle from the surface of the alimentary canal, and convey into the blood the materials of its renovation; that the office of the lymphatics is to take up and carry into the blood those old and effete portions of the solid constituents of the body, which require to be removed to make way for a fresh deposit; while the veins imbibe the serous fluid exhaled from the surfaces of serous membranes, and into the meshes of the areolar tissue, as well as poisons and other substances that are soluble and dissolved in that fluid.

If this be so, the difficulty just now mentioned vanishes. Of the two sets of absorbing vessels, the lymphatics and the veins, one set may continue to perform its functions, while the other fails to do so. This theory is quite consistent with the actual phenomena of dropsical disease; and whether it be altogether true or not, a part of it is certainly true; that, namely, which assigns to the veins a *large* share in the whole process of absorption. The experiments of Magendie and of others are quite conclusive upon that point.

It has also been fully established, that fluids may and do pass into or out of the

veins, in the living body, not by any vital process, but by mere physical imbibition and transudation, through the coats of those vessels; that when the veins are distended to a certain degree with watery fluid, the entrance of more of the same fluid, through their sides, is impeded or prevented; that, when the distension is still greater, the aqueous part of the blood may even pass in the other direction out of the vessel; and that, on the other hand, when the veins are comparatively empty, the surrounding serous fluid passes readily into them, or, in common language, is absorbed. The venous absorption is explicable therefore upon the principles of *endosmose* and *exosmose*, as laid down by Dutrochet; or I would rather say, according to the more general and more simple laws of *heterogeneous attraction*, as explained by the late Professor Daniell.

Imbibition being a form of that attraction, belongs in various degrees to all the tissues of the body. Its rapidity—and even its direction in respect to the elastic coats of a vessel surrounded by fluid, and also carrying fluid of a certain consistence—will vary with the varying distension of the vessel. When the vessel is moderately full, the exterior fluid passes uninterruptedly inwards, and is conveyed away by the internal current. When, on the other hand, the vessel is kept much distended by its contents, the contained fluid, or its thinner part, passes continually outwards; and there is an intermediate degree of distension, at which the pressure is just sufficient to prevent the transit of fluid in either direction. Magendie found, accordingly, in a well-conducted and conclusive series of experiments, that by regulating the conditions of comparative emptiness or fulness of the circulating system, he could accelerate, or retard, or suspend altogether, the operation of a poison dissolved in the humours of the body. In other words he could thus accelerate, retard, or prevent, the process of absorption or imbibition through the blood-vessels.

Bearing these physiological truths in remembrance, we shall have no difficulty in showing that the chronic forms of dropsy are attributable partly, and chiefly, and in many instances entirely, to undue plenitude of the veins; and that this venous repletion is produced, almost always, by some impediment to the free return of the blood towards the heart.

When the areolar tissue of a limited part of the body becomes filled and distended by serous liquid, we call the swelling *œdema*; but this is exactly the same in its nature as anasarca. Now, *œdema* is often the consequence of some mechanical obstruction to the venous circulation. We can produce it whenever we will. Our countryman, Dr. Lower, 170 years ago, tied the jugular vein of a living dog. When a few hours had elapsed, he observed that all the parts beyond the ligature, reckoning from the heart, were much swollen: and upon dissecting the animal after death he found that the areolar tissue of the head and face was filled, not with red blood, as he had expected it might be, but with clear and limpid serum. On another occasion he placed a ligature upon the vena cava, just above the diaphragm: death soon ensued, and a large quantity of water was discovered in the cavity of the peritoneum, “non aliter quam si ascite diu (canis) laborasset.”

These experiments were not instituted with any reference to the pathology of dropsy; yet that Lower perceived their bearing upon that subject is plain from this sentence: “Quantum hæc ad ascitis et anasarcae causas investigandas conducant, aliis judicandum relinquo.” He even explains the extravasation of the thinner or serous part of the blood as taking place by infiltration, “velut in filtro,” through the pores of the vessels.

Precisely similar phenomena succeed the compression or obliteration of a large vein in various parts of the body. In operating for popliteal aneurism, Mr. Travers was obliged to tie the femoral vein: the areolar tissue of the limb was speedily infiltrated with serous fluid. Long-abiding *œdema* of one foot and ankle has been cured at once by the reduction of a crural hernia, which had been pressing for the same length of time upon the femoral vessels. You have heard, I have indeed already spoken, of the disease called *phlegmasia dolens*; a disease that is very common in women soon after childbirth, although it is not peculiar to them, nor to the female sex. The foot, leg, and thigh become enormously *œdematous*. The essence of this disorder is inflammation of the femoral vein; blocking up that vessel near the groin, and retarding or precluding the return of the venous blood from the limb. One arm often swells in the same way, and from a similar cause, in women who are afflicted with

cancer of the breast. In pregnancy, the gravid uterus sometimes presses upon the iliac veins, and obstructs the current of blood within them: the consequence is, anasarca of the lower extremities, which disappears as soon as the pressure is removed by the delivery of the woman. The flow of blood through the vena portæ is frequently hindered, by disease in the liver, or by other causes; and serous liquid accumulates in the peritoneum, constituting ascites. A French physician, M. Tonnellé, narrates several cases in which serosity was found in the cavity of the arachnoid, in conjunction with obliteration of the venous sinuses of the dura mater. In all these instances we have retardation of the venous current, undue plenitude of the veins, and dropsy of the part from which they proceed. The natural exhalation goes on, and the exhaled fluid collects and stagnates because the channel through which it ought to be drained away is choked up. The larger the vein, and the nearer we approach the heart, the more extensive is the dropsical accumulation: and if we could plant an obstacle at the very termination of the venous stream, we should dam up the blood in the whole system of veins, and produce a general dropsy.

Such an obstacle is frequently placed there by disease. The returning blood is checked at its entrance into the heart; at the confluence of all the veins of the body, where they unite to empty themselves into the right chambers of that organ: and then anasarca of the universal areolar tissue comes on, and water collects in all or most of the great serous cavities.

It is no part of my present purpose to inquire how such disease of the heart as is productive of dropsy arises. Commonly we find the right auricle and ventricle enlarged in capacity, the opening between them unnaturally wide, and the tricuspid valve unequal to its office of closing that aperture. Such a morbid state of the right heart may be occasioned by any cause which impedes the flow of blood *out* of its cavities. The diseased condition of those cavities may be primary; but it is oftener, perhaps consecutive to other disease. It may be produced by disease of the lungs, preventing the right ventricle from freely delivering its contents into the pulmonary blood-vessels. Or the retarding cause may be still more distant, in the left side of the heart, keeping the pulmonary blood-vessels unduly full, and thereby hindering indirectly the passage of the blood from the right ventricle. The dropsy may ultimately depend, therefore, upon some bar to the circulation, placed even at the mouth of the aorta. Obstacles situated anywhere in the circuit formed by the right heart, the lungs, and the left heart, have the effect of producing secondary changes in the parts behind them. But disease, thus propagated in a direction retrograde to the course of the blood, is propagated gradually, and sometimes very slowly. These are points of much interest, which we shall investigate together by and by. I allude to them now, that you may not be perplexed by a knowledge of the fact, that diseases of the heart often exist for a long while without inducing dropsy. It is with disease of the right side of the heart, whether primary or secondary, that passive dropsy is especially associated.

As if to furnish the *experimentum crucis* in respect to this doctrine, disease does sometimes, with a curious precision, dam up one only of the two great venous trunks, at the junction of which the right auricle is placed: and then the dropsy is as curiously limited to that half of the body in which the tributary veins of the obstructed trunk originate. The first example of this which I ever saw was a most remarkable one. The patient was dropsical in his upper half only. His arms were so hugely anasarcous that he could not bring his elbows near his sides: his neck and face were hideously bloated and exaggerated, and his eyes prominent and staring; while his lower limbs were of their natural size, and appeared preposterously small, and out of proportion. The poor man looked as if the upper part of his body had been stuffed, for acting some ridiculous part upon the stage. The cause of this strange and distressful state was found to be the obliteration of the vena cava superior, close to the auricle. Its sides had been pressed together by a large aneurism of the aorta; and a portion of the vein was fairly sealed up. I have seen two or three similar cases since.

Objections have, however, been taken to the accuracy of the conclusions drawn from such cases as I have related; and it is fit that you should be aware of them. Thus it is stated that veins have been found obliterated, and yet there was no dropsy. Now to this objection it may be replied, in the first place, that it is not every vein, the obliteration of which would cause manifest oedema. It must be the principal venous

trunk of the part concerned. When some of the secondary and smaller veins alone become impervious, the blood may reach, and return by, the primary branches with sufficient readiness to relieve the turgid capillaries, and prevent any serous accumulation.

But (it may be said) the principal vein itself has been found converted into a solid cord, and still there was no dropsy. Granted: but it does not follow that there never *had been* dropsy. You know that when a large artery is tied, the circulation is carried on in the corresponding limb, by means of collateral arterial branches: imperfectly indeed at first; but, at length, as the supplemental channels become more numerous and free, the supply of blood to the limb is as copious as ever. It is precisely the same, *mutatis mutandis*, with the veins; only that the anastomosing venous tubes are not (perhaps) so readily developed as the arterial. Now I am not aware of any instance in which it has been shown that the principal vein was obliterated, and yet there neither was, nor had been, any œdema of the limb. The recorded cases have been met with in dissecting rooms, and the previous history of the subject has been unknown or unregistered. Mr. Kiernan has told me that he once examined the body of a woman who had excited much curiosity among the medical men by whom she had been seen during life, on account of a remarkable and enormous dilatation of the superficial veins of the abdomen. She was not dropsical, and the cause of the huge varix was sought for with great interest after her death. The inferior cava was obliterated. Here the compensating result was obvious to the sight; the new channels had answered their purpose, and performed the functions of the original channel. The history of this case was incomplete: it was not ascertained whether the woman had always been free from dropsy.

I hold this objection therefore to be invalid, until some authentic instance shall be brought forward of the obliteration of a large venous trunk, without a corresponding accumulation of serous fluid, either at the time when the observation is made, or at some previous time in the life of that individual. It is, besides, possible enough, that the obstruction of a large vein may be effected gradually, by the slow encroachment, for instance, of a growing tumour; and the collateral circulation may begin to be enlarged with the first impediment in the vein, and may keep pace with and counterbalance that increasing impediment, till the closure of the vessel is complete: so that, from first to last, there may be no noticeable dropsy.

Again, it is affirmed, and truly affirmed, that anasarca often occurs, without any obliteration of veins, and independent of any discoverable organic disease in the heart, or anywhere else. We see this every day in weak chlorotic girls, with bloodless cheeks and pale lips. Some of you saw a case of this kind which was lately under my care in the hospital; besides the anasarca, the systolic sound of the heart was accompanied by a loud, unmistakeable bellows sound. This girl got quite well, and left the hospital without bellows sound, or any other trace of disease. There could not then have been any organic change; in fact, there was not. Yet was there, virtually, a retardation of the venous circulation; not by any mechanical obstacle opposed to its course, but in consequence of the debility of that hollow muscle, the office of which is to propel onwards with a certain degree of force the blood that reaches it. Girls of this description have weak and flabby voluntary muscles; and it is reasonable to presume that the involuntary muscle, the heart, partakes of the general debility of the muscular system, and becomes incapable of sending the blood forwards with the requisite energy. Nay, I believe that a heart thus feeble may yield a little and dilate under the resisting pressure of the blood that enters its chambers; and that so an occasional but temporary bellows sound may arise, from the altered relation between the cavities of the heart and their outlets. Certainly this view of the matter is strengthened by the *juvantia* and *lædencia*. If you are tempted, by the pain complained of by your patient, or by the violence with which her heart is throbbing, to take away blood, you find that she is ultimately made worse by the depletion; on the other hand, if you give her steel, feed her well, keep her bowels free, and place her every morning under a cold shower-bath, you find that she recovers her lost strength, that colour returns to her lips and cheeks, that her palpitations cease, and her dropsy vanishes. In proportion as the muscular system in general receives fresh tone and vigour, does that particular muscle the heart also regain the degree of power necessary for the effectual discharge of its proper func-

tion, which is very much that of a forcing-pump. Such is the way in which I should explain both the cause of the dropsy, and the cause of its cessation. In such cases our patients do not simply *recover*; they are *cured*. I should apply a similar explanation to some other forms of dropsy. Andral describes a certain *cachectic* disposition of the body as being a cause of dropsy; persons may be bled into a dropsy, or starved or weakened into a dropsy. These are genuine instances of dropsy from debility, which is what the ancients conceived all dropsies to proceed from. The thin and watery quality of the blood induced by frequent bleedings, by insufficient nourishment, by certain poisons, or by other causes, may doubtless facilitate, or even determine, the passage of its aqueous part through the coats of the veins. But admitting this as a concurrent cause, I am disposed to the belief that all passive dropsies occurring under the circumstances just adverted to, and without any apparent organic disease or change, are mainly to be ascribed to debility of the heart: and viewed in this way, they are all brought under the same general principle; viz., the retardation of the blood in the veins.

A large class, then, of passive dropsies, depending upon mechanical congestion, and defective absorption by the veins, are traceable, in their origin, to the heart; and we call them, accordingly, *cardiac* dropsies. But another class, perhaps as numerous, are connected in a remarkable manner with certain diseased conditions of the kidneys; and these, for the sake of distinction, we style *renal* dropsies. I shall say a few words respecting them, after I have briefly considered the other source of dropsical swellings, adverted to in the commencement of this lecture: namely, excessive *exhalation* of serous liquid. Dropsy so caused comes on suddenly and tumultuously, and is spoken of as being *acute* or *active*. It borders closely upon inflammation, and sometimes can scarcely be discriminated from inflammation with serous effusion. The condition of the capillary circulation is supposed to be intermediate between that in which the ordinary amount of secretion is maintained, and that in which inflammatory effusion takes place. The excessive increase of secretion is analogous to what we observe in other parts and predicaments of the body; to the abundant perspirations, for example, that are occasioned by violent exercise; to the plentiful flow of tears caused by any irritation of the eye, or by the passion of grief; to the augmented watery discharges from the mucous membrane of the bowels produced by purgative medicines; all of which may be independent of inflammation, but all of which are attended with congestion that might readily be pushed into inflammation. In point of fact, if the secretions to which I have now referred were poured into close cavities, instead of proceeding from surfaces that are situated on the exterior of the body, or that communicate readily with the exterior, they would *constitute* dropsies.

The phenomena of *active dropsy* are of this kind: a labourer is engaged in some employment, which, while it requires considerable bodily exertion, and causes copious perspiration, necessarily exposes him also to the influence of external cold and moisture: he has been digging (perhaps) in a wet ditch, in winter time, and he pauses to take his meal; or he has been unloading a wagon, and rides home, some miles, in a heavy rain that wets him to the skin; or he has been mowing, in the heat of summer, and lies down to sleep upon the damp grass. All these suppositions are derived from actual occurrences. The perspiration is suddenly checked; and in the course of a few hours he becomes universally anasarcaous. Again, a patient recovering from scarlet fever ventures out into a cold atmosphere, while the process of desquamation is yet going on; and he is attacked with dropsy of the areolar tissue; and, it may be, of some of the larger cavities also. The urine at the same time is observed to be scanty, troubled, mixed with blood.

To comprehend this rapid change from a state of health to a state of dangerous disease, we must again have recourse to the findings of physiology.

Besides the constant exhalation which takes place from the inner faces of the shut serous cavities, a large amount of watery fluid is continually thrown out of the system, by all those surfaces that communicate with the air—by the skin, the lungs, the bowels, the kidneys. Now it is well ascertained that when the excretion of aqueous fluid from one such surface is checked, the exhalation from some other surface becomes more copious. It is probable that the aggregate quantity of water thus expelled from the system in a given time, cannot vary *much*, in either direction, without deranging the whole economy. But we are sure that the amount furnished by any excreting

surface may vary and oscillate within certain limits consistent with health, provided that the defect or excess be compensated by an increase or diminution of the ordinary expenditure of watery liquid through some other channel. Sound health admits and requires this shifting and counterpoise of work between the organs destined to remove aqueous fluid from the body. This supplemental or compensating relation is more conspicuous in regard to some parts than to others. The reciprocal but inverse accommodation of function that subsists between the skin and the kidneys affords the strongest and the most familiar example. In the warm weather of summer, when the perspiration is abundant, the urine is proportionally concentrated and scanty. On the other hand, during winter, when the cutaneous transpiration is checked by the agency of external cold, the flow of dilute water from the kidneys is strikingly augmented. All this is well known to be compatible with the maintenance of the most perfect health. But supposing the exhalation from one of these surfaces to be much diminished, or to cease, without a corresponding increase of function in the related organ, or in any excreting organ communicating with the exterior, then dropsy, in some form or degree, is very apt to arise. The aqueous liquid thus detained in the blood-vessels, seeks, and at length finds some unnatural and inward vent, and is poured forth into the areolar tissue, or into the cavities bounded by the serous membranes.

Dropsy of one part sometimes supervenes suddenly upon the rapid disappearance of a watery collection from another part. It is no uncommon thing to see the swollen unwieldy legs and thighs of an anasarca patient quickly unload themselves, and resume their natural bulk and symmetry. His friends congratulate him, and each other, that his disease is leaving him; but as his legs are emptying, he becomes drowsy, forgetful, comatose, apoplectic; and after his death we find the ventricles of his brain distended with serous fluid.

Or the dropsical accumulation may be transferred from its place through a safer channel. The best instance of this that occurs to my recollection I heard related by Dr. Farre. A gouty individual had hydrocele; dropsy of the tunica vaginalis. After the disease had lasted for some time, he got very drunk one evening, with rack punch, which greatly disordered his alimentary canal, and brought on a kind of cholera. He had profuse vomiting and purging, which quite exhausted him; and at length he fell asleep. When he awoke in the morning, he found that his hydrocele, which had been a large one, was gone: and it never returned. Such an accidental cure is most instructive.

If water be injected, in some quantity, into the blood-vessels of a living animal, the animal soon perishes; dying generally by coma, or by suffocation: and when the carcase is examined, the lungs are found to be charged with serous liquid, or water is discovered in the areolar tissue of some other part, or in the shut serous membranes. If, however, the animal be first bled, and then a quantity of water be injected equal to the quantity of blood abstracted, the injection is followed by no serious consequences.

Facts like these throw, as it seems to me, a strong light upon a confessedly obscure part of pathology. It appears that under various circumstances the blood-vessels may receive a considerable and unwonted accession of watery fluid, and that they are very prone to get rid of the redundancy. When they empty themselves through some free surface, their preternatural distension is relieved by a flux. If, on the other hand, the surface be that of a shut sac, in discharging their superfluity they cause a dropsy. Why sometimes this organ, and sometimes that, is selected as the channel by which the superabundant water shall be thrown out of the vessels, we can seldom tell. We often find it difficult to determine which of the two facts in question is to be considered the antecedent, and which the consequent. For not only is it true that when the blood-vessels become overloaded with serous fluid, they readily deposit a part of it; but also that when they are in the opposite condition of comparative emptiness, when they contain less blood than is natural, they are equally ready to replenish themselves by absorbing fluids from any source to which they can find access. In the case of the man who was cured of his hydrocele upon the occurrence of profuse watery discharges from his stomach and bowels, it seems clear that the expenditure of serous liquid from one part led to its absorption into the blood from another. When anasarca suddenly leaves the extremities, and fatal coma follows, it appears probable that the absorption is the first of the changes, and the effusion the

second: and had this effusion been determined to the mucous membrane of the intestines, to the skin, or to the kidneys, it would have brought relief and safety to the patient, instead of causing his death.

We have obtained, then, a glimpse of one or two most important principles in respect to the pathology of dropsy. The blood-vessels, when preternaturally full of aqueous fluid, have a strong tendency to empty themselves; when preternaturally empty, they readily drink up watery fluid wherever they come into contact with it. From the discharge of their superfluity of water arises a dropsy, or a flux. The cause, and the cure, of many dropsies, lie in these propositions.

The application of these principles to the supposed case of active dropsy must be obvious. No doubt, in some such cases, actual inflammation takes place; but in many of them there is merely the dropsical effusion, without any other trace or evidence of inflammatory action. The two facts which it chiefly concerns us to remark are these—first, that the aqueous portion of the blood, which in health is habitually carried off to a very considerable amount by the skin, is suddenly diverted from that tissue; the perspiration, sensible and insensible, is suppressed: and secondly, that the areolar tissue, or the large serous bags, or both, become filled with serosity.

It is not by any necessity, however, that the vicarious excretion is turned upon these serous surfaces. In truth, the intercepted perspiration more often escapes, or labours to escape, from some free surface; and then we have, not a dropsy, but a flux. Diarrhœa, for example, is more common, under the supposed circumstances, than anasarca or ascites: apparently because there is a closer analogy of structure, and a more direct consent or agreement in function, and a stronger reciprocal influence, between the skin and the mucous membrane of the alimentary canal, than between the skin and the serous tissues.

Brief allusion has been made to a large class of chronic dropsies, connected with and dependent upon a particular renal disease. This important species of dropsy will require a detailed examination hereafter. It is more complex, and of more obscure pathology, perhaps, than *cardiac* dropsy. It certainly has a more direct relation also to what I have just been describing as active dropsy: of which it may almost be regarded as the chronic form. Sometimes the kidney disease, of which the dropsy is an incidental and not an essential symptom, springs up silently, and without obvious cause. Sometimes it may distinctly be traced back to its origin in an attack of acute dropsy: in which complaint the kidney always and manifestly labours, its functions being violently deranged, and the urine being small in quantity, and mixed with blood.

In this chronic and renal dropsy, the watery accumulation is accounted for by the deficient excretion through the customary channels. The blood-vessels deposit that excretion in a wrong place. The urine, in the outset of the dropsy at least, is scanty. The skin is almost always dry, harsh, and unperspiring. The anasarca usually increases or decreases, as the quantity of urine diminishes or augments. Remarkable alterations take place also in the qualities and composition of the urine itself: it has a low specific gravity, contains albumen, and is deficient in urea. The blood degenerates too; and other organs of the body, and especially the heart, are apt to fall into disease. The suppression of perspiration, and the appearance in the urine of blood or serum, unchanged by the discerning power of the kidney, form striking links of connexion between acute and renal dropsy.

In the sketch that I have been endeavouring to give you of the pathology of dropsy, I have taken extreme cases to elucidate the two varieties of that disease which have been respectively denominated active and passive. Let me once more present to you, in a summary view, the points of resemblance, and the points of distinction between them.

They resemble each other in the result; namely, in the collection of serous liquid in the circumscribed cavities and vacuities of the body. They differ in the rate at which the collection augments.

In the well-marked acute dropsies the liquid is rapidly effused, in quantity much beyond the natural amount of exhalation. In the well-marked passive dropsies the exhalation goes on as usual, but the fluid exhaled is not taken back again into the

circulating vessels with sufficient facility. In one case the circulation is disturbed and tumultuous; in the other, it remains tranquil. It is probable that in the more acute forms, the serum transudes through the coats of the arteries, or of the capillary vessels next adjacent to the arteries. In the completely chronic and cardiac forms, there is a defect of absorption by the veins. Active dropsies are sometimes spoken of as belonging to the left side of the heart, passive dropsies to the right.

But there are intermediate degrees, in which the full veins are not only unable to admit any addition of aqueous liquid, but also to retain that which they already hold; and serosity gradually exudes through their parietes.

What connects all these forms of dropsy is a preternatural fullness in some part, or the whole, of the hydraulic machine. And this seems to be the grand key to the entire pathology, as well as to the remedial management of the disease.

I scarcely need point out to you the fact, that the water of dropsy is liable to change its place, in obedience to the force of gravity. In general anasarca, when the serous accumulation slowly augments, it first becomes visible about the feet and ankles. There are two causes for this; the one occasional in its operation, the other general. The veins of the lower extremities are apt, when the patient is erect, to be more turgid than other veins; for unless the action of their valves be quite perfect, those vessels sustain the weight of a large superincumbent column of blood, which concurs with other causes to retard the upward current, and to keep the depending capillaries unduly full. Under such circumstances the effusion, or the arrest of absorption, may *take place* around the insteps earlier than in any other part. But in general it is not so. In most cases, the truer and simpler reason of the earlier manifestation of dropsical swelling about the ankles, is merely that the serous liquid which fails to be removed from the areolar tissue in all parts of the body, gravitates towards the *lowest* part; and being thus collected into a comparatively small space, is rendered more perceptible. During the night, when the horizontal posture is maintained for several hours, the œdema of the ankles disappears, but the neck and face, perhaps, become bloated and puffy. And it is obvious why, in these cases, the feet, towards evening, swell more than the hands. The hands receive the serous fluid from the areolar tissue of the arms alone; the feet, that which sinks down, not only from the legs and thighs, but from the head and trunk also. The limbs may be looked upon as bags, which fill up in proportion to the quantity of liquid detained. And the lungs are similarly bags: and in these cases we commonly may hear the crepitation of pulmonary œdema in their lowermost portions.

I mentioned an instance in which one-half only of the body was anasarca, and that the upper half. The descent of the dropsical fluid was prevented by the dress of the patient; the waistband of his trousers having compressed the areolar tissue, through which alone the gravitating liquid could seek a passage. So, sometimes, it is stopped at a lower point of its descent by tight garters, and the thighs swell earlier than the insteps. It is not at all uncommon to see persons who, in the daytime at least, and in the erect posture, are anasarca in the lower half only of the body. We do not so often meet with anasarca of one moiety of the body, the division being made by an imaginary plane drawn through its axis. Yet this does occasionally happen. This curious phenomenon is usually the result of a mere accident, the anasarca patient being unable to leave his bed, or to lie at all except on one side; and then the accumulating liquid gravitates to that side. I have, however, seen one case to which this explanation would not apply. I believe that some local obstruction to a large vein in the neighbourhood of the shoulder caused œdema there, and the fluid sank down and filled the areolar tissue of that side alone. As the man recovered, I had no means of verifying the truth of this conjecture.

Cæteris paribus, those parts of the body become the most loaded with serous fluid, and show the anasarca the plainest, of which the areolar tissue is plentiful and loose; as the eyelids, and the scrotum. But in extreme cases the liquid pervades the same tissue, where it is much more dense and compact: as where, for example, it is sub-jacent to mucous membranes. In the examination of a dropsical corpse, the mucous coat of the intestines may sometimes be seen to be elevated by the water collected beneath it. It then looks like jelly, and the *valvulæ conniventes*, which are flat and thin in their ordinary state, become round and convex. Dropsy of the submucous tissue of the air-passages is frequently a cause of death.

Many persons seem disposed to ascribe these anasarcaous swellings, especially when they make their appearance suddenly, to inflammation; and much is said about the frequency of *inflammatory dropsy*. But the facts we have just been considering sufficiently refute this theory. If the serous liquid be the product of inflammation, what is the part inflamed? It cannot be, as some appear to think, the distended areolar tissue itself; for if so, the inflammation must shift its quarters under the influence of gravity. * The term *inflammatory dropsy* may not perhaps be indefensible when applied to that class of dropsical affections that have been spoken of under the head of active dropsy. I am far from denying the frequent agency of inflammation in producing changes which, in their turn, lead to dropsy, but we shall do well not to confound those collections of serum mixed with blood or with coagulable lymph, which are distinctly events or products of inflammation, with other collections of serum which resemble the former in that respect only, but differ entirely from them in every other particular. To the class denominated active, which occur suddenly, from defect of some one or more of the usual channels of aqueous excretion, and which are usually attended with much disturbance of the whole system, the epithet *febrile* would not be inappropriate. There may be some few cases in which it is impossible to determine whether the effusion be inflammatory in its origin or not. If the serum be turbid, if we can discover in it the smallest admixture of pus, or of flakes of lymph, or if the disease have been marked by the ordinary signs of internal inflammation, we need not hesitate in our opinion. One of the latest systematic writers on dropsy in this country holds that all dropsies are more or less inflammatory. We can see one reason for this mistake (for a mistake it surely is) in the relief and amendment which often ensue upon the employment of blood-letting in dropsy.

The general *prognosis* in this disease may be readily gathered from what I have said of its causes and conditions. The anasarca which occurs in chlorotic young women is the least perilous, and the most curable. Of the rest, febrile dropsies are more obedient to treatment, and oftener admit of complete recovery, than the passive or chronic. Local dropsies are to be regarded with hope, in proportion as the obstruction on which they depend is capable of being removed, or of being compensated by the development of fresh channels for the delayed blood. As far as the mere water is concerned in the chronic forms of the disease, cardiac dropsies are more readily dispersed for a time, but more likely also to return, than dropsies which are complicated with renal disease. It is obvious also that the immediate danger of dropsical accumulations will depend much upon the place the liquid may occupy. The difference in this respect is immense between the tunica vaginalis, and the pericardium; between the areolar tissue of a limb, and that which lies beneath the mucous membrane of the glottis.

It remains that I should offer a very few final remarks concerning the principles upon which dropsies are to be treated.

The first object is to get rid of the preternatural accumulation of watery fluid: the second is to prevent its collecting again; in other words, to remedy the diseased conditions which gave rise to the dropsy. Indeed, if we can accomplish this second object without delay, the dropsy will generally disappear of its own accord. Now *venæsection* will often sensibly reduce the dropsical swelling. In what has been called active or febrile anasarca, general blood-letting is advantageous in several ways. It helps to relieve the congestion, akin to inflammation, upon which the effusion depends: it tends to abate the undue action of the heart: and by emptying the blood-vessels, it facilitates the re-absorption of the effused liquid, and its ultimate ejection from the system.

But although blood-letting is the most direct and certain way of unburdening the loaded veins, and therefore, in many instances, the most effectual remedy for the dropsy, it is by no means adapted to all, nor even to many, forms of the malady. It will always indeed remove a portion of the aqueous ingredient of the blood, but it expends at the same time its fibrin and its red particles. It impoverishes the circulating fluid, and thus enfeebles the patient more than would the indirect measures, to be mentioned presently, for evacuating the collected liquid. Perhaps, by rendering the blood more watery, *venæsection* may indirectly favour the transuding of its serum outwards whenever the venous current happens to be retarded. It certainly weakens the central organ of the circulation; and to muscular debility of the heart we have

already seen that certain forms of general dropsy may owe their origin; and thus it is that ill-timed or excessive bleeding may be the *cause* of dropsy. In these forms of anasarca, instead of robbing the veins of their blood, we seek to repair the quality and richness of that fluid, and so to restore the deficient tone and vigour of all the muscles, and of the heart among the rest.

In most cases then it is inexpedient to let blood; and we endeavour to empty the vessels indirectly, and in such a manner as to withdraw from them the more watery parts only of their contents. In other words, it becomes our object to augment the discharge of watery fluid from one or more of the secreting surfaces of the body: but it must not be the inner surface of a shut sac.

I noticed before the close analogy that obtains between dropsies and fluxes. Dropsy is a flux into a closed cavity. Fluxes would be dropsies if the fluid poured forth did not escape. And you are to observe that we frequently try to cure a dropsy by producing a flux.

By what surface or channel this artificial drain shall be attempted, is often a matter of great nicety and importance. In some cases we strive to promote the discharge of the superabundant water by the way of the kidneys: in others by the mucous lining of the alimentary canal: in others by the external skin. The circumstances by which our choice must be determined will come under review hereafter.

Passive dropsies are much more difficult of cure than active, and will often baffle our best-directed efforts. You are not, however, to regard those passive dropsies which depend upon the obliteration of a large vein as necessarily incurable; for if a collateral venous circulation be accomplished, the dropsy will permanently disappear. But we must give nature the credit of the cure in such cases. Time is the best remedy; and all that we can sometimes do is to alleviate in the meanwhile the most distressing or threatening of the symptoms.

I mentioned, in the outset of the lecture, that the presence of the dropsical fluid may constitute nearly all the suffering of the patient, as well as much of his danger. Now, when we cannot get rid of the water by bleeding, or by internal remedies which excite serous discharges, we may often afford great present comfort to our patient, and prolong his days, by letting the water out by a slight mechanical operation. *Paracentesis* is the scientific, and *tapping* the vulgar name for this proceeding. It has been performed successfully, by means of a small trocar, to evacuate the water from the brain in chronic hydrocephalus; it is often resorted to for the purpose of emptying the peritoneal cavity, and the tunica vaginalis testis; and it is not seldom practised to let out the fluid of anasarca; for *acupuncture* of the legs and thighs and scrotum is only another form of tapping.

In the local variety of dropsy that is called *hydrocele*, the re-accumulation of the liquid is sometimes prevented by exciting just so much inflammation of the membrane as may cause its opposite surfaces to cohere; whereby the cavity itself being abolished, any return of the disease is rendered impossible.

This is an expedient which we scarcely dare to employ in other species of dropsy; in ascites, for example; first, because the inflammation itself would place the patient's existence in imminent peril; and secondly, because if it could be safely conducted, the adhesion and obliteration might seriously embarrass and impede the functions of important organs.

The circumstances which require and justify this mechanical remedy; the rules and precautions to be observed in its performance; and the measures to be adopted for preventing the recurrence of the accumulation, by the removal of its efficient cause, will all be considered in detail when we come to treat of the special forms of dropsy.

LECTURE XVII.

Diseases of the eye. Catarrhal Ophthalmia. Purulent Ophthalmia of Adults.

HAVING brought my observations on general pathology to a close, I next proceed to the consideration of individual diseases; and I shall take them up one by one, in that anatomical order to which I adverted in the introductory lecture of this course. That is to say, I shall go *a capite ad calcem*: interpolating those disorders which, although they have a name, have as yet no ascertained local habitation, wherever it may seem most convenient to introduce them. I mentioned before one advantage, as it seems to me, of bringing together, in juxtaposition, all the diseased conditions to which the same part, or the same neighbourhood, of the body is liable — namely, the facility thus afforded of comparing the phenomena by which they are characterized, and of discriminating one disorder from another. In taking the parts in succession from the head downwards, we adopt a sort of order, definite enough for the purpose of aiding the memory, and yet free from the trammels which belong to all attempts at arranging diseases according to their essential nature and affinities.

I propose then to speak, in the first place, of certain diseases of the organ of vision. Diseases of the eye occupy a sort of neutral ground, upon which the surgeon and the physician may both lawfully enter. For some of them there are no means of relief, but in manual operations of the most delicate kind. On the other hand, many of the internal parts of the eye require, when diseased, exactly the same species of general treatment which the physician adopts in diseases of *other internal* parts. We seek to change the condition of a small portion of the body, by remedies which act upon and through the system at large. My real and only motive, however, for beginning with a few of the numerous morbid states to which this little part is liable, is this: — that we find, in the eye, more satisfactory and plain illustrations of the *general* facts and doctrines of pathology, as I have been endeavouring to set them before you, than in any other single organ of the body. “Here” (to use the words of Dr. Latham, whose published Lectures on *Clinic & Medicine* I strongly recommend you to study)—“here you see almost all diseases in miniature: and from the peculiar structure of the eye, you see them as through a glass; and you learn many of the little wonderful details in the nature of morbid processes, which but for the observation of them in the eye would not have been known at all.”

“Within the small compass of the visual apparatus,” says Mr. Lawrence, “we meet with a greater variety of *structures* than in any other parts of the body. Indeed the eye, with its appendages, exhibits specimens of every one of the animal tissues. We find in it bone, cellular and adipous substance, and blood-vessels: mucous, fibrous, and serous membranes; the conjunctiva exemplifying the first; the sclerótica, the sheath of the optic nerve, and the lining of the orbit, the second; the surfaces containing the aqueous humour, the third: muscular, nervous, and glandular parts: common integument, and hairs. Besides these, it contains several tissues of peculiar nature, to which there is nothing strictly analogous in other parts.”

The eye itself, taking it apart from its appendages, the spheroidal *eyeball* itself, is scarcely an inch in its longest diameter. Yet it seldom happens that disease, of any kind, occupies the whole, even of this small space, at once. Inflammation, for example, is often confined to one of the tunics of the eye, external or internal; and when it affects more, it is usually in consequence of the extension of the inflammatory process, from some one texture in which it took its rise. You will not expect me to treat of the vast number of disorders to which the several parts of the eye are liable. I shall bring, I repeat, a few of them only under your notice; and I shall select those concerning which the physician is most frequently consulted; which every one, whatever branch of the profession he may follow, ought to be competent to treat; and, more particularly, which are calculated to elucidate other diseases, and above all, other internal diseases, that are usually assigned to the care of the physician. With the

anatomy and physiology of the organ, I may take for granted that you are already acquainted.

I will first briefly inquire into the inflammatory affections of what may be considered the mucous membrane of the eye. Like other mucous membranes, it forms a surface communicating with the external air. Some of these affections are very trifling: some are very severe.

There is a mild form of inflammation of the conjunctiva, which constitutes the most common disease of the eye to which adults are subject. It results, in most cases, from vicissitudes of temperature; or from certain conditions, or sudden variations, of the atmosphere. It is very apt to be excited by exposure to a stream or draft of air, especially in the night and during sleep. It has a strong analogy—indeed it is the *same disease*, except in *situation*—with that moderate degree of inflammation, produced by the action of the same causes, in the mucous membrane of the nasal cavities, the throat, and the bronchi, which in common parlance we style a *cold* in the *head*, or in the *chest*, as the case may be: and accordingly that inflammation of the conjunctiva of which I speak is often called by the unlearned, a *cold in the eye*; and the same analogy is expressed in its technical appellation: the cold in the head or chest is termed by nosologists a *catarrh*; and the *cold in the eye* of the vulgar is, with them, *catarrhal ophthalmia*. The suddenness (sometimes) of its accession has procured for it also the denomination of a *blight* in the eye.

The term *ophthalmia* is at present used to denote inflammation of the eye *generally*; it conveniently expresses in one word what would otherwise require more. Formerly, when the diseases of the eye were not so well understood in this country as they are at present, almost all the inflammatory conditions to which that organ is subject were lumped together under the common appellation of *ophthalmia*, or *the ophthalmia*. That word now requires some epithet to distinguish the seat or the kind of inflammation that is meant.

It can scarcely be otherwise than interesting to mark the phenomena which occur in catarrhal ophthalmia, when we reflect that in its cause and nature it is the same with inflammation of a similar surface, in parts which we cannot so well inspect as we can the conjunctiva. This membrane, as you know, lines the eyelids, and covers about a third part of the globe of the eye anteriorly. The inflammation, in catarrhal ophthalmia, is confined to the conjunctiva and the meibomian follicles. Its leading symptoms are *redness* of the surface of the eye; *some pain* and uneasiness there; an increased *discharge* from the affected membrane and the follicles; and a *sticking together* of the *eyelashes and lids*.

The redness is worth notice, both in respect to its tint, and to the arrangement of the vessels in which it appears. It is *superficial*; and of a *bright scarlet* colour; and usually *irregular*, or diffused in patches, some fasciculi of vessels being more distended than others. When, however, the inflammation is more intense, the whole surface, except that of the cornea, becomes of a scarlet red. The vessels of the conjunctiva, thus rendered visible by inflammation, anastomose continually with each other, and form a net-work, which can be slipped and dragged about over the subjacent surface by moving the eyelids with the finger. Frequently some of the meshes of this network are filled up with little patches of extravasated blood; the eye is what is called blood-shot, or, to speak learnedly, there is *ecchymosis*; and sometimes all distinction of separate vessels is nearly lost. In the commencement of the complaint the redness is confined to that part of the conjunctiva which lines the lids; and it afterwards advances gradually, from the angle where it is reflected over the eyeball, towards the cornea.

Now all these particulars are of consequence, since they are diagnostic of the seat of the disease; and to show this I must mention by anticipation, the appearance, and the arrangement, of vessels that are observed when inflammation affects some of the textures which lie deeper than the conjunctiva, and especially the sclerótica. The sclerotic redness is seen *through* the conjunctiva. It is of quite a different *tint* from that of the conjunctiva. Instead of showing a *bright scarlet* colour, it is *pink*, or sometimes of a slight *violet* hue. The vessels are much *smaller* and *finer* than those belonging to the conjunctiva, like hairs. They are *straight* also, and arranged regularly, after the manner of radii in a circle. They lie in the sclerotic, round the cornea, like what is called by painters a *glory*; or like a *halo*, or *zone* surrounding the

central cornea; and they *cannot be made to shift their place* by any dragging of the lids. These are very important distinctions. They are such as are easily recognised when two eyes are examined in which the two membranes in question are separately inflamed and vascular; and they are still more palpable perhaps when both membranes are simultaneously inflamed, as they often are, in the same eye. Then, unless the conjunctiva is so universally red as to prevent our seeing the sclerotica through it, the contrast between the larger, more tortuous, scarlet, and reticular vessels of the conjunctiva, and the fine, straight, rose-coloured, radiating vessels of the sclerotic, is exceedingly striking; and those of the conjunctiva which lie naked on the loose mucous membrane, admit of being slipped about over the fixed zone of vascularity which is presented by those of the fibrous tunic.

The pain which attends catarrhal ophthalmia is slight and trifling. At the outset there is generally some uneasiness when the eye is exposed to the light; but there is no intolerance of light when the disease is fairly developed. The patient complains rather of a sensation of stiffness and dryness, and feels as though there were some foreign substance in the eye, between the globe and the lids, especially when the eye is moved; a grain of sand, or of gravel, or a little fly. So exact is the resemblance of this feeling, that you can with difficulty persuade the patient that there is nothing of that sort in his eye. No doubt this sensation is produced by the inequality and roughness of the surface, consequent upon the irregular distension of the vessels of the inflamed membrane: irritating the organ mechanically, just as a piece of dust might irritate it.

Now in this respect again, there is a marked difference between conjunctivitis and sclerotitis. In the latter disease the pain is much more severe, of a dull aching character, with a sense of tightness: the part inflamed is denser, and less yielding than the conjunctiva. The pain is attended, also, frequently, by throbbing, and it is felt in the surrounding parts more severely perhaps than in the eye itself; in the brow, temples, and head. It is a very remarkable circumstance, too, that the pain is distinctly aggravated towards night; increasing in violence from the evening till after midnight, abating towards morning, and ceasing in a great measure during the day, to be again renewed in the evening. I am speaking now particularly of inflammation of the *sclerotica* produced by the same *causes* as give rise to catarrhal ophthalmia; of what is generally called *rheumatic ophthalmia*.

The increased discharge that takes place from the eye in catarrhal ophthalmia is *not* a discharge of *tears*. In the beginning of the complaint there is sometimes a slight degree of lachrymation. But this soon ceases, and the mucous secretion from the surface of the membrane is augmented in quantity, and changed in quality. At first it is somewhat thin, but it soon becomes thicker, and it is often puriform; *i. e.*, opaque and yellow: sometimes it retains more exactly the characters of mucus, is transparent and viscid; so that the eye looks moist to a bystander, while to the patient it feels gummy. The puriform secretion is not, in general, in any great abundance. You may see it lying in the angle between the eye and the lower lid, upon pulling them apart; or it makes itself visible at the corner of the eye, or between the eyelashes along the edges of the lids, which it glues together at night. Sometimes, however, the discharge is more copious, so as to approximate to what is observed in the less severe forms of another disease I shall presently mention; *viz.*, *purulent ophthalmia*.

There is seldom much swelling of the conjunctiva. If there be any, it results from an effusion of serous fluid into the meshes of the areolar tissue that connects the membrane with the subjacent sclerotica; by which effusion the conjunctiva is partially raised and separated. This kind of effusion often goes to a very great extent in purulent ophthalmia, or in violent inflammation of the external membranes, as I shall show you by and by.

So much, then, for the symptoms, and causes, of catarrhal ophthalmia. It is necessary that you should be familiarly acquainted with them; not so much because the complaint is very *serious* in its nature, but because it is *common*; because you are sure to be again and again consulted about it, and because it is of great importance to distinguish it from other forms of ophthalmia, in order to adopt the proper treatment. A mistake of diagnosis might lead to mischievous activity on the one hand; or to still more pernicious inertness on the other.

When the inflammation does not extend beyond the mucous membrane, it will run a certain course, and then, under favourable circumstances, subside. But if it be improperly treated, or if the patient cannot guard himself against a repetition of its exciting causes, it may continue for weeks, and harass him a good deal, and even produce such a change in the inflamed lids as may prove a source of permanent irritation, and of chronic disease, of the cornea over which they sweep.

Remedies of an active kind, such as influence the whole economy, are scarcely ever necessary. The patient should observe the main particulars of the antiphlogistic regimen, and avoid exposure to drafts or currents of air, and to cold and moisture generally. When the external weather is inclement, he should remain in rooms of a uniform temperature. It will be right to purge him in the outset with calomel and jalap, or with calomel followed by a black dose.

If the system at large sympathize with the local disease, it may become necessary to draw blood from the arm, or to apply leeches; but neither of these measures is requisite, unless the inflammation is unusually severe, or the disease has been neglected or mismanaged.

After the bowels have been thoroughly cleared by an active purgative or two, remedies which encourage moderate perspiration will be likely to forward the cure: such as warm diluent drinks; five grains of Dover's powder, and immersion of the feet in warm water, at bed-time; and saline draughts containing two or three drachms of the *liquor ammoniæ acetatis*, taken at intervals during the day.

But in this complaint *local* measures are of greater importance than those which are addressed to the general system: stimulating or astringent applications to the affected membrane itself. Almost all modern writers on diseases of the eye agree in this. Dr. Mackenzie, of Glasgow, states it as the result of his observations on Beer's practice in Vienna, and of his own subsequent experience, upon an extensive scale, at the Glasgow Eye Infirmary, that "general remedies in this disease are inferior to local ones; that *violent* general remedies are worse than useless; and that a local stimulant treatment may almost entirely be relied on." Mr. Melin, in a report of ocular diseases at the General Hospital, Fort Pitt, states that he had treated nearly 300 cases, some of them severe, upon the same principle, without either local or general bleeding: and that he had satisfied himself of the efficacy of this plan of management. And Mr. Lawrence, who for ten years was one of the surgeons to the Ophthalmic Infirmary, in Moorfields, and who during that period had ample opportunities of studying this disease of the eye as well as others, says that it is one to which the use of powerful astringents is more particularly applicable. In disorders which manifest a strong natural tendency to terminate in recovery, it is only by taking advantage of the conclusions derived from extensive observation that we can be quite sure of our ground; and when the same result is reached by different and independent observers, we may safely place confidence in their concurrent testimony.

Dr. Mackenzie and Mr. Melin both employ, and recommend, the same application; viz., a solution of the nitrate of silver in distilled water, in the proportion of four grains to the ounce. A large drop of this solution is to be applied to the membrane once or twice, or three times, in the course of the day. If the patient recline his head backwards, and the drop be placed in the hollow formed in the internal angle of the eye, it will be diffused over the globe upon the separation and subsequent winking of the lids. After a minute or two this causes a pricking or smarting sensation, which subsides in from ten to twenty minutes, and the eye then feels much easier than it did before the drop was applied. Dr. Mackenzie says that the feeling as if of sand in the eye, is uniformly relieved, and the inflammation abated, by the use of this solution, which he speaks of as a remedy of sovereign utility in the purmucous inflammations of the conjunctiva. The eye continues easy, after its application, for five or six hours perhaps; and when the symptoms return, they are again to be met by the introduction of another drop. As the disease subsides the remedy gives less and less pain, till at last it is scarcely felt. He tells us that "he has sometimes alarmed other practitioners by proposing to drop upon the surface of an eye highly vascular, affected with a feeling as if broken pieces of glass were rolling under the eyelids, and evidently secreting puriform matter, a solution of lunar caustic; and that he has been not a little pleased and amused at their surprise when, next day, they have found all the symptoms much abated by the use of the application." He

declares also that the acetate of lead, and the sulphate of zinc, substances which are much used in what are called *collyria*, or *eyewashes*, are greatly inferior, as local applications, to the nitrate of silver, in this disease.

There is another expedient that requires to be attended to in these cases. When the eyelids are gummed together by the viscid discharge, much hurtful irritation is often produced by the hasty attempts which the patient makes to separate them. Now all this may be obviated by smearing their tarsal edges at bed-time with any mild ointment; the spermaceti ointment, or a bit of lard. There is no necessity, as I believe, in this form of disease, to use medicated or stimulating salves: the object is to prevent the mutual adhesion of the lids; and this is accomplished by simple grease.

Purulent Ophthalmia—is another disease of the conjunctiva; differing from catarrhal ophthalmia in degree, in the severity of its symptoms, in the danger which it implies to the sense of vision, and in its exciting causes. It takes its name from the profuse discharge of pus that pours from the inflamed surface. There are three remarkable varieties of purulent ophthalmia; called respectively—1, purulent ophthalmia of adults, or Egyptian ophthalmia, or contagious ophthalmia; 2, gonorrhœal ophthalmia; and 3, purulent ophthalmia of newly-born children.

The symptoms of the two first-mentioned varieties, especially in their severer forms, are so much the same, that it would involve us in mere repetition if I did not take them together. In truth it appears to me much the simpler and better mode to look upon purulent ophthalmia as one disease; and to specify, as we go on, the differences by which its several forms are characterized: and not to split it into three different diseases, and to give a separate description of each.

Although purulent ophthalmia is inflammation of the very same part that is inflamed in catarrhal ophthalmia, from which it differs chiefly in degree, it is a hideous complaint, either to suffer or to treat; on account of the rapid progress it frequently makes, and its destructive tendency. The inflammation is *greatly* more intense; the surface becomes, in the worst cases, highly vascular throughout. A copious discharge of thick, yellow, puriform matter is speedily established; this flows out from between the swollen lids, and runs over the cheek, which it often excoriates. At the same time considerable effusion takes place into the areolar tissue that connects the sclerótica and the conjunctiva. You are aware that the conjunctiva extends over the whole anterior face of the globe; adhering, however, so much more closely to the cornea than to the sclerótica, that we might doubt at first whether it did not stop at its margin. This close and firm adhesion over the cornea, and the looser attachment to the sclerótica, give rise to a very singular phenomenon. The conjunctiva is raised to some distance from the subjacent sclerótica by the effusion that takes place between them; and it projects around the cornea in the shape of a large thick ring, leaving the cornea buried, as it were, in a pit; nay, sometimes the swollen and prominent membrane will lap over, so as nearly to exclude the cornea from our sight. The same kind of effusion takes place also, sometimes very rapidly, into the areolar tissue that connects the conjunctiva with the palpebræ, producing great external tumefaction, and a livid red appearance of the eyelids, which project forwards in large convex masses, and often prevent our seeing the globe of the eye at all: the upper lid especially becoming hard and stiff, and completely overhanging the lower. This swelling from effusion into the subconjunctival tissue is of a pale red or flesh-colour, sometimes marked here and there with patches of extravasated blood. The appearance is called *chemosis*: not *ecchymosis*, as the similarity of the sound has led some erroneously to suppose, but *chemosis*. *Εκχυμωσις*, from *εκχεω*, effundo, signifies an effusion, and by common consent among medical writers, an effusion of blood. *Χημωσις*, the root of which is *χημα*, hiatus, means a gap or hollow.

Now this puriform or purulent inflammation, so long as it is confined to that part of the membrane which lines the eyelid, is not of any serious importance; but it is prone to extend itself to the cornea, and the whole anterior surface of the eye, and to produce ulceration or sloughing of the cornea, either in consequence of the actual inflammation of that part, or in consequence of the pressure made upon and around it by the swelling of chemosis. Frequently, when the cornea remains visible, a furrow or trench of ulceration may be seen at its margin; sometimes forming a complete

circle, sometimes portions of a circle, sometimes going quite through; and when this happens, or when the cornea bursts from the effects of deeper-seated inflammation, the aqueous humour is evacuated, and the iris protrudes through the aperture. Even when these horrible results do not take place, the eye is often as effectually spoiled for the purposes of vision by an interstitial deposit between the laminæ of the cornea, rendering it opaque, and permanently precluding the passage of light towards the retina.

And when neither of these lamentable effects of the inflammation is produced, it is apt to leave behind it a chronic and very troublesome condition of the membrane. The conjunctiva that lines the lids remains thickened, granular, hard, and rough, instead of regaining its natural smoothness, softness, and polish. One consequence of this is a perpetual irritation of the surface of the cornea, by the mechanical friction of the rough and hard lid in opening and closing the eye, and in the various motions of the eyeball. The continuance of this irritation leads at length to haziness or opacity of the cornea, which becomes traversed also by visible red vessels. Chronic inflammation of its investing membrane is produced, and kept up.

The most severe forms of this disease are attended, at length, with a good deal of pain; doubtless because the inflammation penetrates to the deeper-seated textures of the organ. The pain then presents those characters which I mentioned before as belonging to certain inflammations of the sclerotica: *i. e.*, it is pulsative; and sometimes sharp and lacerating, sometimes dull and aching; and it is intermittent, or if constant, it is aggravated by paroxysms; the paroxysms coming on at night, and abating towards morning: and it is not confined to the eye itself, but extends to the parts around it. The circumorbital pain is characteristic of inflammation of the sclerotica and cornea, and of the internal tunics, the choroid and iris. When the eye is not visible, from the swelling, we may conclude that the inflammation is as yet confined to the *conjunctiva*, if the pain be only scalding or "sandy;" and that it has extended to the sclerotica and cornea if the pain be severe, throbbing and paroxysmal. In the cases in which the latter kind of pain is felt, the cornea generally gives way. Sometimes this event brings relief to the pain, and sometimes the pain continues to return after the bursting of the cornea. It is curious that with all this, there is seldom much intolerance of light.

In the earlier stages of this malady, it is entirely local: the system at large is scarcely disturbed at all. But the constitution begins to sympathize and suffer when the local symptoms increase in severity; the pulse becomes frequent, and the tongue white, but there is seldom much thirst or fever; and when blood is drawn from a vein, it does not, in general, exhibit the buffy coat. A good deal of variety in these respects has been noticed however in different cases. Children manifest more constitutional disturbance when labouring under purulent ophthalmia than adults. If there be not much fever, there is always much uneasiness and irritation, and the sleep is broken by the nocturnal accessions of pain.

Such being the general features and course of the disease, at least as it occurs in adults, or in patients beyond the period of infancy, we may next inquire into the circumstances under which it has been observed to arise.

Purulent ophthalmia has been ascertained to be a common disease in hot climates: in India, Persia, and Egypt. It was brought into England, from the latter country, by our troops in the beginning of the present century, after the well-known contest which there took place between the French army and our own under Sir Ralph Abercromby. In this way it got the name of the *Egyptian* ophthalmia. It naturally excited very great attention at that time, and it does not appear to have been accurately described before.

To give you some notion of its prevalence in certain places and at certain periods, and of its serious nature, I may state that, according to returns made from the Military Hospitals at Chelsea and Kilmainham, there were, on the 1st of December, 1810, no fewer than 2317 soldiers a burden upon the public from blindness in consequence of ophthalmia; and in this number those soldiers who had lost the sight of one eye only were not included.

Again, in the year 1804, within nine months, *i. e.*, from April to December, nearly 400 cases of purulent ophthalmia occurred at the Royal Military Asylum; and within

six years from that time, without including relapses, upwards of 900 cases had taken place in the same establishment.

You will find these statements in a paper in the third volume of the *Transactions of a Society for the Improvement of Medical and Chirurgical Knowledge*, by the late Sir Patrick Macgregor. Many of our best regiments were for a time crippled and rendered unfit for service by this disease; which they carried from Egypt to other foreign stations as well as to this country, especially to Sicily, Malta, and Gibraltar. Nor were the French troops affected by it in less number. Assalini, who wrote an account of the ophthalmia of Egypt, states that two-thirds of the French army were labouring under it at one time. It occurs also, but fortunately not to such an extent, in civil life. It broke out, some years ago, in a large boys' school in Yorkshire; and blindness in one or both eyes, or serious injury to sight, from opacity of the cornea, and other consequences, took place in nearly twenty cases.

You perceive, therefore, that this formidable complaint has been ascertained, within the last fifty years, to have prevailed as an epidemic; attacking great numbers of persons living under the same circumstances, and having constant communication with each other. And one of the first questions that naturally arises in one's mind is, whether it is capable of being propagated from one person to another by *contagion*. Much difference of opinion has existed on this subject. For my own part I cannot imagine how any one can doubt its contagious properties.

I will give you a case or two, as related by Sir Patrick Macgregor, proving two very important facts; first, that the disease is capable of being excited in the eye of a person, previously healthy, by the direct application of the puriform discharge from an eye affected with this ophthalmia; and secondly, the very rapid operation of the poison so applied.

One of the nurses employed at the Military Asylum, while syringing the eye of a boy who had much purulent discharge, found that a considerable quantity of the matter had spurted into her own right eye. This was at four o'clock in the afternoon. She felt little or no smarting at the time; but towards nine o'clock the same evening her right eye became red and somewhat painful, and when she awoke the next morning, the eyelids were swelled, *there was purulent discharge*, and she complained of pain in the eyeball. The usual remedies were begun in the morning, and she recovered in the space of three weeks or a month. The left eye, into which none of the matter had gone, remained free from disease.

On another occasion a precisely similar mischance befel another of the nurses, except that the matter spurted into her *left* eye, about nine in the morning. Sir P. Macgregor happened to be in the hospital at the time when the accident occurred. He desired the nurse to bathe her eye immediately with lukewarm water, and she did so for several minutes; but notwithstanding this early precaution, about seven o'clock in the evening the left eye began to itch to such a degree that she could not refrain from rubbing it. When she awoke next morning the eye was considerably inflamed, the lids were swelled, and upon moving the eyeball she had a sensation as if some sand were lodged beneath them. In the course of the same day purulent fluid issued from the eye, and other symptoms followed, which were similar to those of the children under her care. The disease subsided, under the usual treatment, in fourteen days. In this case also the other eye remained sound.

A third nurse in the same institution did not come off so well. She was sponging, with warm water, the eyes of a boy suffering severely from purulent ophthalmia; and she inadvertently applied the sponge she was using to her right eye. This happened at eight o'clock in the morning. She mentioned the circumstance to the other nurses, but she took no means to prevent infection. Between three and four o'clock of the afternoon of the same day, itching of the right eye came on; and before she went to bed it was considerably inflamed. Next morning her eyelids were swollen, she complained of pain in moving them, the whole anterior surface of the eyeball was in a state of high inflammation, and a purulent discharge began to trickle down the cheek from the inner canthus. The symptoms increased in severity in spite of all the means employed to check them, and on the fourth day the eyeball burst. The sight of the eye was irrecoverably lost, and the inflammation continued for upwards of three months. The left eye did not suffer.

These were cases in which the poisonous matter was *accidentally* applied. But a

similar application has been made *intentionally* and by way of *experiment*, and with *the same results*. Dr. Guilié, of Paris, introduced the puriform secretion furnished by some children affected with purulent ophthalmia, under the eyelids of four other children belonging to a separate institution for the blind. These four children were amaurotic, but the external surface of their eyes was healthy and entire. In each instance a regular attack of purulent ophthalmia followed the introduction of the matter.

Facts of this kind prove, I say, beyond the possibility of question, that the disease may be propagated from a diseased to a healthy eye by actual contact of the puriform matter. Here we have not one case (which might be considered as an accidental coincidence), but several: the morbid secretion is applied to one eye only; the symptoms of inflammation commence, and the regular form of the disease is fully developed within a few hours after the first application of the pus; and that eye only is affected. It is impossible to get over evidence of this kind.

The only questions, therefore, that can be raised respecting the sources of the disease are these:—whether the malady can be communicated through the medium of an atmosphere impregnated with the effluvia that proceed from the diseased part, without any actual contact of the pus in substance?—whether the disorder is ever produced in *any other* way than by contagion?—and if so, *how* it is then excited?

I ought to observe, that independently of such isolated examples of the direct communication of the complaint, by contact with the diseased matter, as I have just laid before you, the history and progress of ophthalmia, since it has been noticed in Europe, are very strongly indicative of its contagious nature. I have already stated that it was not known in Europe till the commencement of the present century—till after the Egyptian campaign in fact. It is not alluded to by any of the authors on disorders of the eye who wrote previously to that period; although some of the Italian physicians and surgeons, and many of the Germans, had paid great attention to ophthalmic diseases. It spread from Egypt both to France and to this country, and to other places in which detachments of the Egyptian force were subsequently stationed: in Sicily, to wit, and in Gibraltar and Malta. Whenever it has prevailed among our troops at home, *this* circumstance has been *uniformly* observed: that it first broke out in soldiers who had come from Egypt, or had communicated with regiments which had been in Egypt. In all cases its origin could be traced to the introduction of fresh troops into the regiment or the barracks.

Again, the manner in which it spreads is exceedingly instructive on this point. It diffuses itself rapidly, when once introduced, in places where a considerable number of persons are collected together; especially under circumstances favourable to the propagation of contagious maladies; as among soldiers assembled in barracks, where many of the men live in the same apartments, and use the same towels: while the officers, who live in larger and better ventilated rooms, and apart from each other, generally escape. And the good effect, in checking the further extension of the disease, of separating the healthy from the sick, and of restricting every one to his own washing utensils, and clothes, and towels and sponges, leads to the same conclusion. Rust, a German author, mentions this striking fact in corroboration of what I have just been saying. The disease broke out in the town of Mayence. This place was garrisoned by Prussian and Austrian troops. The ophthalmia began and spread extensively among the Prussian soldiery; while the Austrians, who were stationed in separate barracks from the Prussians, in another quarter of the town, remained quite free from it.

Those persons who deny, or who doubt, the contagious nature of purulent ophthalmia, rest their opinions upon some such considerations as these. They hold, in the first place, that the peculiarities of the atmosphere, in Egypt, where the disease has been found so common, are *sufficient* in themselves to account for it. That the inhabitants of that country never dream of its being caused by contagion. Assalini, who saw the complaint raging in the French army, professes his belief that it did not arise or spread by contagion. He remarks that the atmospheric conditions which are known to occasion *catarrhal* affections, are very frequent and powerful in Egypt: the days are very hot, the nights chilly, and attended with heavy dews; and men's eyes are perpetually exposed, in the day time, to a dazzling glare of light from the white and arid surface, while the air is full of floating particles of hot sand, which are

raised from the ground by the slightest breeze. His opinion, therefore, and the opinion of others who saw the disease as it prevailed in that country, was, that it consisted simply of acute catarrhal inflammation of the conjunctiva; and that it affected those persons most who were most exposed to the exciting causes of such inflammation; the common soldiers, therefore, more frequently than the officers.

Other circumstances adduced by the disbelievers, or sceptics in respect to contagion, are that many who have intercourse with the sick escape the disease; and that when bodies of men, among whom purulent ophthalmia has been prevailing to a great extent, are broken up and dispersed, the complaint is not thereby disseminated, as they say it ought to be, supposing it to be communicable from one person to another; that, in fact, this dispersion, the disbanding of troops for instance, and sending them to their friends and families all over the country, is the surest way of stopping the disorder.

Again, many ineffectual attempts have been made to inoculate the eyes of animals with the matter of purulent ophthalmia. Müller, a German, with that pains-taking industry for which the Germans are so remarkable, collected on a camel's hair pencil matter from the eyes of patients labouring under purulent ophthalmia, early in the morning, before they had washed them, and inserted it under both the lids of each eye, in a great number of animals, leaving the pencil there for a few seconds, and then pressing it so as to squeeze the matter out. He also smeared the pus copiously and repeatedly along the edges of the lids. He served in this way five cats, ten dogs, two rabbits, two squirrels, two blackbirds, a starling, a yellowhammer, and a cock. And in none of them did the inoculation produce the slightest effect.

It is a sufficient answer to these *negative* experiments, however, that other persons were more successful in producing the disease in this manner. Vasani and Grafe have both excited it repeatedly in dogs and cats, by the application to their eyes of matter taken from human patients. And I have already informed you of many instances in which the disease was generated in men by accidental, and even by intentional, inoculation. No amount of negative evidence can do away with positive testimony so often repeated.

And with respect to the other objections, and especially the *main* objection, that persons may associate and hold close intercourse with individuals labouring under purulent ophthalmia without contracting the disease; I would have you remark that this is no more than what continually happens in regard to diseases which are acknowledged on all hands to be contagious, and to have no other source at present, however they may have originated at first, *but* contagion: the small-pox for example. I think there is some reason for believing, from the facts which I have been relating, that purulent ophthalmia, like the small-pox, is capable of being communicated from one person to another, not only by positive contact, but by transmission of the specific poison somehow for a short distance through the air. But many persons exposed to the contagion of small-pox escape it altogether: and more persons still, perhaps, fail to be affected, though fully exposed, at one time, and yet readily accept the disease at another time, even when the exposure may seem much more slight than on previous occasions. Now what is true of the small-pox may be presumed to be likely, although perhaps in a different degree, of purulent ophthalmia.

As to the circumstance that the disbanding of a regiment infected with the disease prevents instead of favouring its dissemination, that circumstance is really no argument at all against our belief in its contagious nature. We shall see hereafter, that when fever patients are collected in numbers in distinct wards, or in fever hospitals, *that* disease is very apt to be communicated to the nurses and medical attendants of the sick; whereas when such patients are distributed here and there among others, in a *general* hospital, the disease is scarcely ever known to spread. In the one case the poison is *concentrated* and *effective*, in the other it is *diluted* and *harmless*.¹

¹ [That diseases, of the contagious character of which there can be no doubt, are rendered more virulent when a number of patients affected with them are crowded together in ill-ventilated apartments, is unquestionably true, but these diseases do not cease to be contagious, when those labouring under them are distributed here and there among the non-affected. We know of no more effectual means of disseminating small-pox than by introducing single cases of it, here and there, among persons predisposed to its attack. The fact is, that the perfect safety attendant upon the dispersion of fever patients among unaffected communities, and the stop put to the

Dr. Mackenzie indeed has come to the conclusion, from what he has himself observed, that the discharge in *catarrhal* ophthalmia, especially when it is distinctly puriform, if conveyed from the eyes of the patient to those of others by the fingers, or by towels, and so forth, is capable of exciting inflammation of the conjunctiva, still more severe, more distinctly puriform, and more dangerous, than was the original ophthalmia. And with respect to the disease which I have been speaking of as purulent ophthalmia, or Egyptian ophthalmia, this author calls it *contagious* ophthalmia. He holds that the inflammation of the conjunctiva, whether in the mild or the more severe form, may and often does originate from common atmospheric influences; but that when so caused it may be communicated from person to person, especially when it is attended with a puriform discharge.

And this is an opinion which, I think, is fully warranted by the facts of which we are in possession upon this subject. There is a strange reluctance, which I have never been able to account for, in some medical men, to admit of the operation of contagion, as a cause of disease. Undoubtedly there are some difficulties belonging to the doctrine of contagion, and I hope in the progress of the course, and especially when I come to speak of fevers, to give that subject the careful attention which its great importance demands; and to enable you to make up your minds respecting it. At present I will only remark, that there is nothing absurd, nor unlikely, in the supposition that diseases may first arise from some other source, and then become capable of spreading by contagion; and that in all cases, even when the contagious principle is most manifest, there seems to be something else required besides the presence of contagious matter; there must be a readiness to receive it, a susceptibility of its influence, on the part of the person exposed to it: a predisposition which is less common in regard to some diseases than to others; but without which there is scarcely any complaint that can be so propagated.

At any rate I would desire to impress upon you the expedience and propriety of *acting*, whatever your doubts or your belief may be, *upon the safe side*. We are bound to proceed, in all questionable cases of this kind, upon the most unfavourable supposition. Very great discredit and loss of reputation have fallen upon practitioners who, having themselves no belief that a given complaint was contagious, have neglected those precautions which, under a contrary impression, they would have thought necessary. Perhaps they may have sometimes suffered unjustly: but you had better not commit yourselves, especially while you are young in years and in experience, by strong assertions of the non-contagiousness of any disease, the mode of propagation of which is at all equivocal. And as for the disease that we are now concerned with, you will do well to act as though it were certainly contagious; whether you meet with it as a sporadic or as an epidemic complaint, whether it be severe in its symptoms, or mild. You should forbid the use of your patient's towels and washing vessels by other members of the family; you should avoid employing the same instruments or sponges to any sound eye, which you have been using for one that is affected with this complaint; and you should take care to wash your own hands, after touching a diseased eye, before you apply your fingers to another that is yet, in this respect, healthy.

occurrence of new cases so soon as a removal is effected of all the inmates of the houses or the inhabitants of the localities where it prevailed, is an almost positive proof that the fever originated solely in local causes, and cannot be communicated by contagion.—C.]

LECTURE XVIII.

Purulent Ophthalmia, continued. Gonorrhœal Ophthalmia. Purulent Ophthalmia of Infants. Strumous Ophthalmia.

WHEN we last met, I spoke of catarrhal ophthalmia, *i. e.*, a mild and common form of inflammation of the conjunctiva, resulting from atmospheric influences. I described its characteristic symptoms, and explained the treatment that has been found most successful for its cure: consisting chiefly in local stimulating or astringent applications.

I began also to speak of the severer forms of inflammation affecting the same part, and included under the head of *purulent ophthalmia*. The symptoms and course and consequences of the two varieties of this complaint, as it occurs in adults, are so essentially the same, that one description of its phenomena is enough. There are certain differences, however, that require to be noticed, in respect to its exciting causes. I laid before you the reasons which satisfy me, that what is called the Egyptian ophthalmia is a contagious disease; and which make it probable that the complaint is capable of being propagated from person to person, through the medium of the air, without the necessity for any substantial application of the morbid secretion from a diseased to a sound eye. These reasons, briefly stated, are as follows: that the disease was unknown to Europe till after the war in Egypt; that, arising among our own and the French troops in that country, it was conveyed by them to various places, and extended itself to soldiers who had intercourse with those troops; that when once introduced it spreads rapidly wherever men are crowded together within a small compass, pay insufficient attention to cleanliness, and use the same towels and utensils; that it has been propagated again and again by the direct application of the morbid secretion; and that its progress is checked by measures which provide against such accidental application, and by separating the diseased from the healthy.

On the other hand, it has been contended that the disease is nothing more than an extreme degree of catarrhal ophthalmia; that the peculiar conditions of the atmosphere in Egypt and other hot countries, where it is prevalent, are enough to account for it; and that when troops are disbanded, they do not give the disease to their friends and families all over the country, but, on the contrary, the dispersion of the sick in this way is the most effectual mode of stopping the disease.

To these arguments the proper answer is, that the same difficulties meet us in respect to some other diseases which are confessed by all persons to be strictly contagious.

My own creed upon the matter is this—that the disease may, and often does arise, independently of contagion, from the agency of ordinary causes of inflammation; and that having so originated, it acquires contagious properties, which develop themselves only under circumstances that favour the propagation of most of the contagious complaints.

I shall next advert to purulent ophthalmia as it is observed to occur, in the adult, in connexion with *gonorrhœa*. If we look to the mere phenomena presented by the inflamed eye, we find nothing to distinguish the gonorrhœal from the Egyptian ophthalmia. Taking the average of a large number of cases, the gonorrhœal is the severer form of the two, and runs th more rapid course. It is said, also, that the inflammation usually commences on the lids in the Egyptian variety, while it attacks the whole conjunctiva at once in the gonorrhœal. But, comparing individual instances, these mere differences, and slight differences too, in degree and situation, will not help our diagnosis.

But other circumstances may guide us. If a patient present himself with severe purulent ophthalmia, who has not been exposed to any of the known atmospheric causes of that disease, and at a time when purulent ophthalmia is not prevailing as an epidemic, and if this patient have a clap, we may conclude that we have to deal with a case of *gonorrhœal* ophthalmia; and this conclusion will be further strengthened if

the disease affect one eye only. For what, through the lack of any better nomenclature, I am constrained to call *Egyptian* ophthalmia, seldom restricts itself to a single eye. Dr. Vetch says, "there is but one case in a thousand in which one eye only becomes affected." Walther observes that contagious ophthalmia almost always appears in both eyes together, but not in the same degree; and Eble (another German author) states that the contagious ophthalmia has not confined itself to one eye in any instance. These round assertions require, however, some qualification; the nurses, whose cases I quoted in the last lecture from Sir Patrick Macgregor's paper, suffered each in one eye only. On the other hand, gonorrhœal ophthalmia mostly, but by no means always, is limited to one eye. In Mr. Lawrence's instructive book *On the Venereal Diseases of the Eye*, he mentions fourteen cases of gonorrhœal ophthalmia. In nine of these, one eye only was inflamed.

It is always a matter of some interest to make out whether the disease has or has not any connexion with gonorrhœa; even though we may gain nothing, in respect to the treatment, by the distinction.

Purulent ophthalmia has been said to be connected with gonorrhœa in three several ways: 1st, by direct contact of the gonorrhœal discharge from the urethra with the conjunctiva; 2d, by metastasis of the inflammation from the urethra to the eye, without any such contact of matter; and 3d, independently of either of these ways; *i. e.*, purulent ophthalmia has been supposed to occur in connexion with clap, just as ulceration of the throat is apt to occur in venereal diseases.

Now the last two of these three modes of origin are more or less questionable; the first is certain.

Very odd speculative opinions are apt to possess themselves of the minds of medical as well as of other philosophers. Some who believe that the disease is communicable to the eye by direct contact of gonorrhœal matter, yet hold that it must come from the urethra of another person; that the Hudibrastic aphorism is true, "No man of himself doth catch." Dr. Vetch seems to have fallen into this opinion through the very common mistake of drawing positive conclusions from negative experiments. He had known a hospital assistant, who "with more faith than prudence," conveyed the matter of gonorrhœa from his urethra to his eyes, with impunity. He states also the converse experiment: a soldier in a very advanced stage of *Egyptian* ophthalmia, attempted to divert the disease from his eyes by applying some of the matter they were discharging to the orifice of his urethra: no effect followed this trial. But in another case the matter taken from the eye of one man labouring under purulent ophthalmia, was applied to the urethra of another man; and inflammation commenced there in thirty-six hours, and he had a very severe attack of gonorrhœa. Some persons, judging from such cases as this, and from the similarity of the discharge in the two diseases, "have gone the length of concluding (according to Dr. Mackenzie) that gonorrhœa has been *originally* an inoculation of the urethra by the matter derived from the eye in the *Egyptian* ophthalmia; whilst others are of opinion that this last disease is nothing else than the effect of an inoculation of the conjunctiva with matter from the urethra in gonorrhœa."

To satisfy you that a person may "catch" the complaint from himself or from others, it is right that I should bring before you one or two well-marked examples.

It is a common persuasion, among the lower classes, that to bathe the eyes in human urine is good for the sight. This piece of practice has cost several persons their vision. A gentleman belonging to the class mentioned to me the other day two cases of purulent ophthalmia so produced, which he had seen among Mr. Guthrie's patients at the Ophthalmic Hospital. In the one, a young woman, not so healthy as she ought to have been, used her own water; in the other, an older woman, for what reason it did not appear, preferred her husband's to her own. Mr. Lawrence alludes to several similar cases. He details an instance also, in which partial sloughing of one cornea occurred; the disease having been caused by the patient's wiping his eyes with a towel soiled with the gonorrhœal discharge from his own urethra. But one of the neatest and most conclusive instances of the production of the disease in this way has been furnished by Dr. Mackenzie. A patient was brought to him from the country with his left eye violently inflamed and chemosed, and discharging a large quantity of purulent fluid; the lower lid everted, and the cornea totally opaque. Thirteen days before, this man, who had then a profuse gonorrhœa, but whose eyes were per-

fectly well, while stooping down and shaking away the discharge from his penis, flung a drop of it fairly into his left eye. Violent inflammation immediately set in, was confined to the eye that was thus inoculated, and produced the results I have mentioned: the gonorrhœa going on just as before.

Numerous authentic cases have been recorded of gonorrhœal ophthalmia produced by the application to the eye of gonorrhœal matter from another individual. Mr. Wardrop met with the following example. An old lady went into the dressing-room of her son, who had gonorrhœa, and washed her face with a towel which he had recently been making use of. Purulent ophthalmia quickly supervened, and destroyed the eye in a few days. Delpech mentions the instance of a young and healthy woman, who bathed her eyes with goulard water, by means of a sponge which had been used by a young man who had a clap: violent inflammation soon arose, and the sight of one eye was lost. Several cases of purulent ophthalmia have been observed in laundresses, who had been employed in washing linen, foul with the discharge of gonorrhœa.

Mr. Lawrence seems to be of opinion that purulent ophthalmia is not a very frequent consequence of the application of the urethral discharge to the eye of the same person. "When we consider," he says, "how this matter is diffused over the linen of patients, both male and female, how often the fingers must be smeared with it, and how inattentive to cleanliness the lower classes are, we cannot help concluding that the gonorrhœal discharge must be *often* applied to the eyes of the same individual; yet gonorrhœal ophthalmia is comparatively rare." Dr. Mackenzie, on the other hand, thinks that the application of the matter to the eye is seldom made. "The instinctive closure of the eyelids," he observes, "when the finger approaches the eye, making it actually difficult for a person to touch his own conjunctiva, unless with one finger he draws down the lower lid, and intentionally applies another finger to the eye, will serve in some measure to explain the rarity of this kind of inoculation."

It has been noticed that women are much less frequently the subjects of gonorrhœal ophthalmia than men.

Does gonorrhœal ophthalmia ever occur by metastasis? This question does not admit of a positive answer. Practical men are divided in opinion on the subject. In the majority of cases of gonorrhœal ophthalmia, we are unable to trace any application of the urethral discharge to the eye, either from the same or from another individual. Yet it does not follow that no such application took place. The German and Italian writers believe in metastasis. "In all the instances," says Beer, "which I have seen, this ophthalmia has occurred in young, plethoric, robust, and truly athletic men; and it has always taken place in a very short time, generally in a few hours, after the suppression of gonorrhœal discharge from the urethra." Mr. Lawrence never knew the urethral discharge stop upon the coming on of the ophthalmia; it has generally diminished, but in some instances has continued as copious as before. He seems to regard the occurrence of the ophthalmia as analogous to those successive attacks of distant parts that are common in gout and rheumatism. Dr. Mackenzie evidently doubts the occurrence of metastasis at all in this disease, and is inclined to refer all the cases in which it has been alleged, to inoculation, or to an accidental concurrence of purulent ophthalmia and gonorrhœa in the same person.

The supervention of purulent ophthalmia as a part of the gonorrhœal malady, independently of inoculation and metastasis, seems to me extremely problematical. The eye is well known to suffer, as well as other organs, in the secondary forms of syphilitic disease, but the conjunctiva is not the part that is attacked. I have never seen nor heard of any satisfactory example of purulent ophthalmia alternating with gonorrhœa, where the possibility of inoculation was excluded. And, upon the whole, my own opinion — (you will take it for whatever it may seem worth) — is against the existence of this alleged form of purulent ophthalmia. Whether it exists or not is of very little consequence in regard to the main question; namely, what is the proper mode of treating the *purulent ophthalmia of adults*?

Now the two chief points to consider, so far as respects the treatment, are — first, blood-letting; and secondly, the application of strong astringents to the inflamed membrane.

Blood-letting has been carried to a very great extent in this disease, or in *these diseases*, if you choose to consider the Egyptian purulent ophthalmia and the gonorrhœal

purulent ophthalmia as two different inflammations. Its effects have not been very decisive or satisfactory; indeed, we could hardly expect that they would. In the first place, the inflammation is so rapidly destructive, that, in many of the worst cases, irreparable mischief is done before the patient applies for medical assistance. In forty-eight hours, or a little more, Mr. Lawrence tells us, the affection may have proceeded to such an extent as to be beyond our control. Of course this reason for the want of success is equally applicable to every remedy that has been, or could be, proposed. But independently of this, even when the disease is seen and submitted to treatment in its very beginning, we should have the less confidence in the power of general blood-letting to control it, for these two reasons; that the part affected is a *mucous membrane*; and that there is *so little constitutional sympathy* with the local inflammation. Free venæsection tells most upon inflammation, when it is attended with high fever and a hard pulse, *i. e.*, with increased action of the heart; which the abstraction of blood tends to abate. It is also a matter of experience, that general bleeding has more influence over the inflammation of serous and fibrous membranes than over that of the mucous tissues. Accordingly, though bleeding has been even lavishly employed in purulent ophthalmia, it has too often disappointed the practitioner. There is one lesson, however, to be learned from copious blood-letting in this disease, even when it fails of its object. It clearly demonstrates what may be hoped for, from an early recourse to that measure in internal inflammations. "You see a person," says Mr. Lawrence, who has both had, and used freely, very numerous opportunities of putting this remedy to the test, "you see a person with his eye bright red, and very painful; he cannot face the light, and tears gush out, with great suffering, if he attempt to do so. You bleed to fainting, and immediately the capillaries are emptied, so that the organ resumes its natural paleness; the pain is gone, the eye is opened without difficulty, and the full influx of light can be borne without an uneasy sensation. For the time the part has passed from violent inflammation to a nearly natural state. With the restoration of the circulation the inflammation will recur after this temporary suspension; but its violence is diminished, and it often gradually abates." Mr. Lawrence is here speaking of acute inflammation affecting the textures of the eye generally, and not of purulent ophthalmia in particular; but I am desirous that, in passing, you should take notice of this direct effect of bleeding to syncope, upon the capillaries of the eye, because it teaches us what the same expedient may do for the capillaries of any other *internal* part, which we cannot see, when that part is attacked with inflammation. In purulent ophthalmia, however, if you trusted to bleeding alone, you would often reduce your patient to a very dangerous state of weakness, and after all fail of your mark. Dr. Vetch bears strong testimony to the usefulness of blood-letting when freely employed in the early stages of Egyptian ophthalmia. In young and robust persons, and at the very outset of the disease, it may aid the local expedients which I shall presently mention; and if the patient be not seen till the globe of the eye is invisible for the swelling, the propriety of abstracting blood will be still further indicated by the occurrence of throbbing and circumorbital pain, returning in nocturnal paroxysms; for this symptom denotes that the inflammation has descended deeper than the conjunctiva. The bleeding, when performed at all, should be performed in the way I formerly spoke of as being required in serious inflammations: the patient should be bled from the arm, in the upright position, till fainting is about to ensue, or the pulse begins to falter. You will do more towards obtaining safety for your patient's vision in this way, and at less expense of his strength, than by bleeding him many times to a smaller amount. When the patient begins to rally from his faintness, from twelve to twenty-four leeches may often be applied with advantage; *round* the eye, and not *upon* the tumid lids, where their bites are apt to add to the existing irritation, and to fester. You had better bleed your patient from the arm, and not from the jugular vein, or the temporal artery, for reasons which, as I have fully stated them already, I need not now repeat.

But of late years more reliance has been placed by most practitioners upon local stimulants, for checking this horrible malady, than upon general or topical bleeding. Dr. Vetch strongly recommended the insertion of *undiluted liquor plumbi acetatis*, and Mr. Briggs, in his translation of a work of Scarpa's on the eye, advised the introduction of a very minute quantity of the oil of turpentine, between the eyelids. But Mr. Guthrie has the merit of having applied, in its full extent, this principle of curing

conjunctival inflammation, even in its severest forms, by stimulant and astringent substances. I told you, when speaking of catarrhal ophthalmia, that Mr. Melin and Dr. Mackenzie treat that complaint with a wash, made by dissolving four grains of lunar caustic in an ounce of distilled water. I might have added other authorities in favour of the same kind of practice. Now Mr. Guthrie treats purulent ophthalmia on the same principle, but with a much larger dose of the nitrate of silver. The greater intensity of the disorder is met by increasing the strength of the remedy. He considers it to be a local disease of a peculiar character; and, acting upon the aphorism of John Hunter (an aphorism, however, which requires some qualification) that two diseases or actions cannot go on in a part at the same time, he proposes to set up in the inflamed conjunctiva a new action, which shall supersede the original disease, and create another that is more manageable. In this point of view Mr. Guthrie's *ratio medendi* agrees with that of Hahnemann, about which there has been so absurd a noise made of late years. I have never had the advantage of seeing Mr. Guthrie's plan tried, but, from all that I have heard of it, I believe it to be a valuable discovery. *A priori*, we should expect that the caustic application would add to the existing mischief, and destroy all chance of saving the inflamed eye. But it is not so. Even Mr. Lawrence, who was, I have reason to think, formerly very sceptical on this point, appears to be so no longer. In his treatise *On the Venereal Diseases of the Eye*, he uses this cautious language:—"Destructive or injurious consequences have so frequently resulted under the usual management of this disease"—he is speaking of gonorrhœal ophthalmia—"that I should certainly employ the local astringent, if I met with a case favourable for the trial; *i. e.*, where the affection had not extended beyond the conjunctiva. Blood-letting might be resorted to at the same time; in most cases, however, our aid is not sought until the cornea has become affected, and it is therefore too late for the astringent plan." But he subsequently added a note, to the effect, that after the statement I have just quoted was written, he had employed the caustic solution in two cases of conjunctival inflammation with the best results.

Mr. Guthrie's plan, therefore, you ought to be acquainted with. After many trials, he has arrived at the conclusion that the best appliance, in this formidable complaint, is an ointment, made by mixing ten grains of the nitrate of silver, reduced to an impalpable powder, with a drachm of hog's lard. This is what he calls his *ten-grain ointment*.

Before applying it to the diseased eye, the discharge must be well cleansed away by a solution of alum; then the ointment having been inserted beneath the lids, they are to be moved freely up and down, so that the whole conjunctiva may get its due share of the remedy; and that it has done so is shown by its turning white. If the surface do not turn white, the ointment has not been sufficiently applied, and will not answer the purpose. If we wish to be quite sure, he says, we turn out the eyelids, and rub the ointment on them. This application gives pain, which lasts for half an hour, or an hour, or more. "Warm narcotic fomentations may be applied to relieve uneasiness, and opium given to allay pain, and to obtain sleep; while a solution of alum, in the proportion of a drachm to a pint, should be injected from time to time into the eye, to clear it; but should the patient sleep, he must not be disturbed. A mild ointment may be applied to the edges of the lids at night, to prevent their sticking together. The next morning the discharge is again to be removed, and the ointment to be reapplied; for on no account should the action we are desirous of exciting be allowed to cease." Of course Mr. Guthrie means it is not to be suffered to cease prematurely. This, with a free but not excessive venesection, is the substance of his peculiar mode of treating purulent ophthalmia; and it appears to have been eminently prosperous in his hands. I have been informed, by one of yourselves, that purulent ophthalmia has been successfully treated, on a large scale, in Manchester, by applying the nitrate of silver, in substance, to the surface of the conjunctiva; that this gives less pain than the ten-grain ointment, though perhaps it may require to be oftener repeated.

I say I have never seen this method of Mr. Guthrie's carried into effect; but after what I have myself witnessed of the intractable and destructive nature of the disease, under the treatment ordinarily adopted before his ointment was devised, I will say also, that were I so unfortunate as to be attacked with severe purulent ophthalmia, I should desire to have the caustic applied without delay.

There are some minor points in the treatment that require a cursory notice only.

Some persons, and Mr. Guthrie among the rest, recommend the exhibition of *mercury*, so as to affect the gums. Now I believe that mercury is quite useless in this complaint; and if useless, mischievous. The disease is too rapid to be overtaken by the mercury, and if you could obtain the specific influence of that mineral in time, *i. e.*, before any of the destructive effects of the inflammation were accomplished, you would do no good thereby. This is not the kind of inflammation over which mercury exercises any useful control. Mr. Lawrence tells us that he has seen both the ordinary purulent, and gonorrhœal ophthalmia, proceeding apparently unchecked, under the full mercurial action.

Practical men are not agreed about the propriety of scarifying the conjunctiva when it is swelled and elevated by chemosis. Mr. Lawrence objects to it, as likely to increase the local irritation; a disadvantage not compensated by the quantity of blood discharged from the divided vessels. Dr. Mackenzie recommends it, stating that the incisions will bleed copiously, and greatly allay the symptoms. Who shall decide in this puzzling discrepancy of opinion? Mr. Guthrie's caustic ointment would, I presume, supersede any other meddling with the inflamed surface. But when the question happens to lie between scarification and no scarification, I should give my vote *for* scarifying; not because I think any useful depletion of the *blood-vessels* could be brought about by that measure, but because, if properly performed, it would evacuate the serous effusion from the areolar tissue between the conjunctiva and the sclerotica, which effusion constitutes the chemosis, and hastens, if it do not cause, the sloughing of the cornea, by the mechanical pressure that it exerts around it.

Are *blisters* of any use? Hear Dr. Mackenzie. "Counter-irritants are *highly serviceable* in this disease, and ought *always* to be employed. There is generally a *marked* change in the quantity and appearance of the discharge from the eye, as soon as a counter-discharge is established by blisters on the temples, nape of the neck, or behind the ears." But listen to Mr. Lawrence. "Experience does not warrant us in ascribing much efficacy to *blisters*." Now the truth is, I believe, that during the active stage of the disease, blisters are not of any use; but that in the more advanced and chronic periods, they are. Indeed Mr. Lawrence admits that they may be regarded as auxiliary measures, and resorted to after antiphlogistic means.

I agree with the same gentleman in thinking that no reliance is to be placed, in gonorrhœal ophthalmia, upon any attempts to reproduce the urethral discharge; indeed, in most cases it is not suspended.

Although I have not mentioned purgatives, you will conclude that they form a very proper and necessary part of the treatment during the activity of the complaint.

After what has already been said of purulent ophthalmia in the adult, and of gonorrhœal ophthalmia, it will not be necessary for me to take up very much of your time in speaking of *purulent ophthalmia as it occurs in newly-born children*.

This is a *very common* disease: it is *very serious* when neglected: it is *very easily managed* when it is seen and treated in time. These are all reasons why you should make yourselves familiar with the complaint, and with the mode of curing it. You may perhaps never have occasion to treat a case of purulent ophthalmia in the adult: you are sure to be consulted about the purulent eye of infants, the *ophthalmia neonatorum*.

The importance of the disorder is apt to be overlooked by mothers and nurses; they say the baby has a cold in the eye, which will go off; and they wash it perhaps with a little of the mother's milk, or some such insignificant fluid. Meanwhile the eyelids swell, the mischief that is going on beneath them is concealed from sight, and when at last a medical man is consulted, he too often finds that one of the eyes has perished, or both: the cornea has sloughed; or become opaque; or protrudes, and constitutes what is called staphyloma; prolapse of the iris has taken place; or the coats of the organ have shrunk up.

The inflammation usually comes on about three days after the child is born, although it may commence later. It is confined, at first, to that part of the membrane which lines the lids. Their edges are observed to stick together when the infant wakes: there is more intolerance of light, apparently, than is suffered in the analogous diseases of adults. The little patients cannot indeed tell us their sensa

tions by words, but they express them significantly enough by keeping their eyes shut, by knitting their small brows, and by turning their heads away from the light. At length the inflammation extends to the conjunctiva that covers the eyeball, the eyelids swell, sometimes enormously: and an astonishingly copious discharge of pus takes place. By the adhesion of the edges of the lids the puriform matter is sometimes pent up, causing them to protrude; and when they are separated it escapes in a profuse hot gush. The eyelids are sometimes everted during the cries and struggling of the little sufferer, and their mucous surface is then seen to be villous and shaggy, and of as bright a scarlet as you ever saw the injected mucous membrane of a foetal stomach. At last those destructive consequences to the eye take place which I have already mentioned. The disease, however, may continue for eight or ten days without any affection of the transparent parts; and so long as these remain uninjured, the eye is safe, provided that proper treatment be adopted.

This disease is probably much the most fertile source of blindness with which we are acquainted. It is believed to originate most commonly, if not always, in contagion. We might, perhaps, expect this, from the analogy of the severe inflammation of the same parts in adults. And it is matter of fact, that in a very large number of cases the mother has been affected, at the time of her confinement, with some kind of vaginal discharge—leucorrhœa, or gonorrhœa; and the eyes of the children are exposed to these morbid secretions, as they are brought into the world. The circumstance of the disease commencing so regularly on the third day, is greatly in favour of the supposition that it results from inoculation of the eyes by the unhealthy fluids of the mother. The discharge from the infant's eyes has been ascertained to be highly contagious. Dr. Mackenzie mentions a lamentable illustration of this fact, which fell under his observation at the Eye Infirmary, in Glasgow. An infant and its grandfather became his patients there at the same time; the latter having been inoculated from the former. Both were so severely affected that the infant had one eye left in a state of total, and the other of partial staphyloma: while in each eye of the old man, the greater part of the cornea remained opaque, and adherent to the iris.

However, the disease certainly occurs in the infants of mothers who seem to be healthy, and who deny that they have any unnatural discharge. It may probably be brought on, sometimes, by bad management on the part of the nurse: by exposure soon after birth to draughts of cold air, or to the injurious influence of a hot and bright fire; or by the introduction of soap into the eye in the primary ablutions, or of gin, wherewith the lower classes, in some absurd persuasion of its strengthening virtues, are wont to bathe the unlucky infant's head. The disorder is observed to be most common in damp and cold weather; in low crowded places; and among the children of the poor.

[Contaminated air, with its often associated morbid causes, neglect of cleanliness, defective nourishment, or improper food, and want of sufficient exercise, is liable to produce a general tendency to disease in the mucous tissues of children. The mouth, the anus, and the vulva, under such circumstances, being all alike liable to affections terminating rapidly in gangrenous ulceration. It is asserted, upon good authority, that in cases of purulent ophthalmia in children, the vagina is liable to be affected with a purulent discharge, precisely similar in appearance to that from the conjunctiva. See Condie on Diseases of Children, 4th Ed. Dr. Mildner, speaking of the disease as observed in the Foundling Hospital of the city of Prague, says that most of the children affected with it manifested an "albuminous crisis of the blood," characterized by a catarrhal condition of the mucous membranes generally, with marasmus and debility. The influence of atmospheric causes in the production of the disease were, according to Dr. Mildner, very evident. Often, when the wards were crowded with puerperal women, especially if the air was cold and damp, from six to ten children would be seized in one day, and usually in both eyes. When, he remarks, the catarrhal cases assumed a *septic* character, numerous cases of umbilical phlebitis, purulent infection, gangrenous erysipelas, croup, &c., were observed.]

Dr. Mildner describes a croupal form of the disease, characterized by an exudation of various thickness, which may assume a membranous form with newly developed vessels. It is often accompanied with the occurrence of membraniform exudations in the mouth and pharynx. When in this form of the disease a *septic* tendency was

manifested, loss of vision might be predicted, even when but a small portion of the cornea was affected. *Annales d'Oculistique*; ser. 4, tom. ii, p. 140. — C.]

One striking difference between the disease as it exists in adults and in newly-born children I have already adverted to; viz., its rapid and often uncontrollable progress in the former; and the facility with which it yields to suitable and timely treatment in the latter. If a child be brought to you with purulent ophthalmia, and you are able to separate the lids sufficiently to obtain a glimpse of the cornea, and perceive that it is still brilliant and uninjured, you may confidently tell the anxious mother that, with due care on her part, her child's eye is safe. If the cornea have lost its transparency, it is still within the reach of recovery, but the chances are against it; if you cannot get a sight of the cornea at all, you will do wisely to give a doubtful prognosis, or even an *unfavourable* prognosis; for such is the ignorance of the vulgar (and I include both rich and poor under this phrase) that if they are not forewarned of the danger, they are very apt to attribute the blindness that ensues to your *stuff*, as they call it.

In the severer forms and stages of the complaint, if the lids are very much swelled, and red externally, and especially if you are unable to obtain any satisfactory view of the cornea without using a degree of violence that might be hurtful, it will be right to apply a leech. In this case it may be placed upon the centre of the tumid upper lid; and you should, whenever that is possible, stay by the little patient until the animal drops off, and the bleeding ceases; for sometimes the bleeding is difficult to stop, and it must not be trusted to the care of the nurse; and the loss of blood occasioned by the bite of a single leech will often blanch the infant's skin, and make you fear that the depletion, slight as it is in actual amount, has yet been too much. The child's bowels should be emptied by a little castor-oil; and a lotion, made by dissolving two grains of acetate of lead in an ounce of water, may be applied to the inflamed organ.

In less severe cases, and I believe in all cases in which you can see the uninjured cornea gleaming through the pus that bathes it, it will be quite sufficient to keep the infant's bowels open with magnesia; to apply a little lard along the edges of the lids, that they may not stick together; and to inject carefully into the eye, *beneath* and *between* the lids, a solution of alum; in the ratio of four grains to one ounce of water. Such, Mr. Lawrence tells us, was the treatment in forty-nine cases out of fifty at the London Ophthalmic Infirmary when he was surgeon to it: no other means being used than magnesia internally, and the solution of alum locally: and out of many hundred instances he scarcely recollected one that suffered in any respect, if the cornea were clear when the infant was first seen. I had, for a considerable period the advantage of watching Mr. Lawrence's patients under that treatment; and the result of it was so entirely and uniformly satisfactory, that I should never think of employing any other. If the eye became at length insensible to the stimulus of the alum, a solution of the nitrate of silver, (from one to four grains in the ounce of water,) was substituted with advantage. Mr. Guthrie uses, I fancy, his caustic ointment; but I am sure that the simple and less severe plan I have been describing is quite sufficient.

There is just one more disease belonging to the conjunctiva, that I wish to bring before you; and then I shall have done with the morbid affections of this external membrane of the eye. It has received several names. Sometimes it is called *pustular* ophthalmia, from the appearance of little pustules upon the surface of the organ. Dr. Mackenzie, who looks upon it as an eruptive disease, affecting the conjunctiva not so much as a *mucous membrane*, but rather as a continuation of the skin, names it *phlyctemular* ophthalmia. It has also acquired the title of *scrofulous* or *strumous* ophthalmia, from its continual occurrence in children of a scrofulous *habit*, and its very frequent association with scrofulous *disease* in other parts. It is a disorder of childhood, and it is so common a form of disorder, that, of ten cases of inflammation of the eyes in young persons, nine will be of this kind. I shall call it *strumous* ophthalmia. It is a form of ophthalmia that differs in many striking points from those which we have been considering.

In the first place, it is intimately connected with the scrofulous constitution; the

peculiarities of which I formerly explained. Although a disease of children, it is not a disease of infants at the breast. It is most prevalent from the time of weaning to about the age of eight. I mentioned to you, in a previous lecture, the remarkable fact — showing the strong influence of unsuitable or insufficient nourishment in developing scrofulous disease — that when asked to prescribe for children having bad eyes, you will find, in nineteen cases out of twenty, that you have to deal with purulent ophthalmia if the child be still at the breast, and with strumous ophthalmia if it have been weaned.

The leading symptoms of this disease are, *slight* redness; great intolerance of light; the formation of little prominences or pustules on the surface of the conjunctiva; and specks which are the result of these. The complaint sometimes occurs in one eye alone, oftener in both; but then one eye is generally worse than the other. Mere catarrhal ophthalmia is apt to degenerate into this affection in scrofulous children. After seeing two or three cases of strumous ophthalmia, you cannot fail to recognise it whenever you meet with it again.

The redness has this peculiarity, that it is slight and partial. Sometimes it is altogether confined to that part of the membrane which lines the eyelids: generally a few vessels, collected into little bundles, are seen proceeding from some point of the circumference — more commonly from the angles of the eye than from any other point — towards the cornea: the vessels are evidently superficial, often prominent. These scattered bundles of vessels (sometimes there is but one) stop when they reach the cornea, or occasionally encroach a little upon it; and where they stop, the small elevations of the membrane may be observed, which are called pustules. This is the most common situation of these elevated points, just at the line of junction between the sclerotica and the cornea, or near that line. Sometimes, however, you may see one or two near the centre of the cornea. They are smaller in size when they appear on the cornea, than when they are situated near its edge.

These pimples may be absorbed, and leave behind them a temporary white spot; more frequently they break and form little ulcers. When these ulcers are beyond the cornea they are of less consequence: when they are situated upon it, they become sources of danger in two ways; they may penetrate the cornea, and let out the aqueous humour, and cause prolapsus iridis and various other mischief: or they may leave, after healing, a permanent opaque white speck, (called *leucoma*,) which, according to its size and its exact place, will interfere more or less with the patient's vision.

The intolerance of light is a very prominent symptom of this disease, and sometimes it really is the only symptom that manifests itself. It is curious that this inability to endure a bright light bears no regular or definite proportion to the intensity of the other symptoms. It is not that the eye is painful when protected from the light; but that the access of the ordinary light of day occasions extreme suffering; the eyelids being spasmodically closed and the orbicular muscle in such strong, and apparently involuntary action, as effectually to resist all attempts at opening them. Children that are affected with this disease carry it legibly written in their physiognomy. Although you cannot tell what is the actual condition of the eye without examining it, you *can* tell, as soon as you look at the patient, what is the *nature* of the inflammation under which he is suffering. The child's brow is knit and contracted, while his *alæ nasi* and his upper lip are drawn upwards: those muscles of the face (they happen to be also muscles of expression) are instinctively put in action, which tend to exclude the light without quite shutting out the perception of external objects; producing a peculiar and distinctive grin. In the severer cases the child will skulk all day in dark corners; or if in bed, will lie upon his face, or under the clothes; and while the light is thus kept off, he does not appear to suffer. If brought towards a window, he holds his head down, and presses his hands or arms over his eyes. When you attempt to open his eye to examine it, a profuse discharge of scalding tears takes place: these pass partly into the nose, and excite fits of sneezing, and partly over the skin, which they sometimes inflame and excoriate; and then, frequently, pustules arise, and produce a discharge that crusts over the cheek and extends to the forehead and temples. This is called *crusta lactea*, and is very characteristic of the scrofulous habit; it occasionally spreads over the whole body.

You might suppose, from this extreme intolerance of light, that the retina was inflamed or in danger. But it is not so. The affection of the retina is purely sym-

pathetic, and need not of itself excite any fears about the vision. Towards dusk indeed, in the twilight, the child can generally open his eyes, and then is quite as able to see as if he were well. Dr. Mackenzie endeavours to explain the connexion of intolerance of light, spasmodic contraction of the lids, and lachrymation, even when there is but little visible redness, by the distribution of the lacrymal nerve; which, after supplying the lacrymal gland, goes to the conjunctiva, and to the orbicularis palpebrarum. Doubtless they are all *reflex* phenomena. Of this epithet I shall have much to say hereafter. We have the same set of symptoms when a bit of dirt gets into the eye, and fixes itself beneath the upper lid. When little or no redness exists, this extreme intolerance of light has been called *photophobia scrofulosa*.

With this strumous affection of the eye there are usually present other evidences also of scrofulous disease. Swelling and redness of the *alæ nasi* and upper lip; enlargement of the absorbent glands about the neck; eruptions upon the head; sore ears; a large and hard belly; disordered bowels; offensive breath; grinding of the teeth; and general debility. And the ophthalmia will alternate sometimes in severity with some of these other local scrofulous complaints; getting better as they get worse, and *vice versâ*.

LECTURE XIX.

Strumous Ophthalmia, continued. Recapitulation. Treatment of Strumous Ophthalmia. General Remarks on Conjunctival Inflammations. Iritis: its Symptoms and Treatment. Causes of Iritis.

WHEN we separated yesterday, I was about to describe the treatment which has been found by experience to be the best for relieving strumous or phlyctenular ophthalmia. Before I take up the subject where it was then dropped, let me briefly remind you of the character and principal symptoms of the disorder. It is a form of inflammation of the conjunctiva, to which *scrofulous* children, from the time when they are weaned to about the age of eight, are extremely liable. It *may* occur considerably later. Sometimes it is the first and only token of the existence of the scrofulous diathesis; generally it is observed in children who bear other marks of the strumous *habit*, and are afflicted with other forms of strumous disease.

Its symptoms are—first, slight vascularity; the redness being partial, and proceeding from one or more fasciculi of superficial vessels, which advance from the circumference of the visible part of the eye towards the cornea, where they usually stop: sometimes, however, they pass a little beyond its edge. At the extremities of these fasciculi, upon or near the line of separation between the cornea and the sclerotica, small prominences appear, which are sometimes absorbed, sometimes break and form ulcers. Less frequently the phlyctenæ are situate towards the central part of the cornea. Secondly, with this partial vascularity and these pimples, and sometimes even without them, there is *extreme* intolerance of light. The pain produced by exposing the eye to the influence of light imparts a characteristic expression to the countenance of the suffering child. Tears flow over the cheek, and inflame it often, and give rise to the eruptive appearance termed *crusta lactea*: or, from its sometimes covering the cheek like a mask, *porrigo larvalis*.

I may add to this summary of what was stated in the last lecture, that sometimes the vessels which pass along the conjunctiva and over the cornea, instead of leading to pustules, extend laterally: so that several bundles of vessels unite by their mutual ramifications; and that part of the conjunctiva which covers the cornea becomes thick, as if it were darned; and more or less opaque. Indeed, the greater portion or the whole of the corneal covering may thus be rendered patchy and vascular. The appearance presented by the eye under these circumstances is called *pannus*.

You will readily believe, from what has been said of this complaint, that it is an

obstinate and troublesome one. Even when it has been cured it is very apt to recur. The scrofulous *habit* on which it depends we cannot get rid of; and whenever the exciting causes of scrofulous *disease* come into action, this form of scrofula is very prone to declare itself, at the period of life which I have already mentioned.

More good is to be done by general treatment, applied to the system at large, in this form of ophthalmia, than in those we were occupied with before; and this is one strong point of difference between them.

In the first place we must endeavour to correct that unnatural condition of the whole system, and especially of the digestive organs, which is commonly so striking a concomitant of the local disease. It will be proper to clear out the bowels in the outset, and occasionally, by a mercurial purge; and to regulate them at other times by laxatives, such as rhubarb, or the *confectio sennæ*, or castor oil. The recovery will be greatly promoted also by those measures which are found to benefit the general health in such constitutions; warm clothing, frequent ablution of the body, nourishing though plain food, the respiration of a pure atmosphere, *change* of air, and regular exercise.

In addition to these measures, tonic medicines should be administered; the preparations of iron, for example, or the dilute mineral acids: but the best remedy of this kind is, undoubtedly, the sulphate of quina. This may be given to a child in grain doses, three times a day, dissolved in water, with a drop of the dilute sulphuric acid, and some syrup of orange-peel. Dr. Mackenzie, in particular, has put this medicine fairly to the test, having employed it in a very large number of cases with the happiest results. In most of his patients he declares that it acted *like a charm*, "abating, commonly in a few days, the excessive intolerance of light and profuse epiphora; promoting the absorption of phlyctenulæ, and hastening the cicatrization of ulcers of the cornea." And Mr. Lawrence adds his testimony to the same effect; and his experience in this disease, like Dr. Mackenzie's, has been large enough to make it highly valuable.

A few words will suffice to explain the kind of local treatment that has been found useful. You may feel tempted to apply leeches round the eye. This is seldom requisite, except when there is more redness and pain than common, and the tongue becomes white, and the skin hot. Certainly you must not take the intolerance of light as a fit indication for the use of leeches. Abstraction of blood rather aggravates that symptom; apparently by increasing the irritability of the retina. Warm fomentations are generally very comfortable to the patient's feelings.

When the general disorder of the system has been somewhat rectified, local stimulants and astringents are of great service. The *vinum opii*, and the solution of lunar caustic, are the best. These are often tedious cases, and therefore it is necessary that you should be aware of one great objection to the long-continued employment of the nitrate of silver wash, which objection has been pointed out by Dr. Mackenzie. It is apt (but only when frequently repeated for a long time together), to stain the conjunctiva of an *indelible* olive colour. For this reason the *vinum opii* is to be preferred in slow cases, and in cases where frequent relapses happen. The good effects of either of those preparations are very striking; they diminish the irritability of the eye, and promote the healing of the ulcers. The red precipitate ointment, and the citrine ointment of the *Pharmacopœia*, diluted, are also found beneficial.

Counter-irritation is another local measure, which is of undoubted utility in this complaint. A great change for the better in the state of the organ often occurs, almost suddenly, upon the rising of a blister placed behind the ear, or at the back of the neck. And issues in the arms are not only serviceable in promoting the cure, but have a marked effect in many children, in preventing relapses. Mr. Welbank, in his notes to Frick's *Treatise on Diseases of the Eye*, states that he has seen chronic strumous ophthalmia, of seven years' duration, quickly and effectually relieved by an issue in the arm. "Having once (says he) in the case of a boy in Christ's Hospital, directed the healing of an issue which had been made above twelve months, I found the immediate consequence to be a relapse of strumous inflammation and ulceration of the cornea, resisting every measure but the renewal of the issue."

He suggests also (what parents are sometimes more willing to assent to) the advantage of making counter-irritation by piercing the lobe of the ear, and inserting a ring, or silk; and "a very convenient form of vesication will be found in the appli-

cation of a strong thread, smeared with the *emplastrum cantharidis*, and firmly tied behind the ear at the angle of its reflection."

When ulceration is going on in the cornea, and threatening to penetrate it, the progress of the ulcer may be checked by touching its surface once in two or three days with a pencil of lunar caustic which has been scraped to a fine point.

When the more urgent symptoms have abated, and the discharge of hot and irritating tears has ceased, the crusta lactea may very easily be got rid of. The crusts are to be removed by a light poultice, or by warm water; and then the part must be bathed from time to time with a lotion made by mixing the oxide of zinc with water; a drachm to four ounces is the proportion I am in the habit of prescribing. If rose-water be used instead of common pump-water, the prescription will be thought the more *elegant*. This lotion will speedily dry up the discharge, and in a short time no vestige of the ugly-looking crust will remain. Parents are highly delighted and very thankful when you thus accomplish the removal of a large, disfiguring, and disgusting scab, which they naturally enough felt apprehensive might leave behind it a corresponding scar. But it is quite superficial.

I have now done with the exterior membrane of the fore part of the eye—with its *mucous* membrane. In examining some of its diseases, we have had the opportunity of noticing several things which illustrate the pathology of the mucous tissues generally, and which exemplify the influence of other circumstances also, as well as of peculiarities of tissue, upon the morbid processes to which these membranes are obnoxious.

We have seen that the mucous surface of the eye readily enough takes on inflammation, under vicissitudes of external temperature, and from the agency of other atmospheric conditions; that the inflammation is apt to spread, often rapidly, over the whole surface of the membrane; and that, in some cases, it may be strictly limited for a long time together, or entirely, to the mucous tissue in which it began; but that when intense, or under special circumstances, it may dip through and extend to the subjacent textures: that, on the other hand, the inflammation sometimes occupies separate specks only of the membrane, and then is more likely to penetrate to the deeper seated tunics: that although the membrane is folded upon itself, so that different portions of it are mutually in apposition and contact, these opposing surfaces do not become adherent to each other under inflammation; on the contrary, that they readily pour forth pus. This tendency to the formation of pus I formerly showed you to be commonly observable, whenever the air finds free access to the inflamed part. The pus thus poured out possesses the remarkable property of exciting the same kind of inflammation when placed in contact with a healthy mucous membrane of the same or of another individual: whether it be the conjunctiva of the eye, or the internal lining of the urethra. The pus, in short, acts locally, upon certain parts at least, as a poison. And we perceive, in this fact, how a disorder that originates in common and accidental causes may become capable of propagating itself indefinitely—may become, in one word, *contagious*. We have seen also that the most intense inflammation may occur in this membrane, without exciting much or any constitutional disturbance; an illustration of the fact that the inflammation of mucous membranes is not so prone to light up fever, is not in general attended with so much pyrexia, as inflammation of some other tissues, and especially of the serous and fibrous tissues: and in proportion as this constitutional sympathy with the local disease is small or absent, so the influence of general bleeding upon the inflamed part is slight or ineffectual. The effect of a new and strong local irritation, in altering or superseding the original inflammation in some cases, has been illustrated in the treatment of purulent ophthalmia as it occurs in the adult subject. The influence of age in modifying the phenomena, and in qualifying the plan of treatment, has been made perceptible in the differences noticed in these respects between purulent ophthalmia in infants and in grown-up persons. We have witnessed, too, the remarkable characters impressed upon inflammation of the very same part, by the presence of the scrofulous diathesis. We shall hereafter meet with numerous examples of chronic inflammation, and the deposition of tubercular matter, and the formation of ulcers in consequence of the elimination of that matter, in other mucous membranes. Whether the phlyctenæ, or pustules, which appear upon the surface of the eye in strumous

ophthalmia, result from a similar separation of tubercular matter from the blood-vessels near the extremities of which these prominences are placed, has not been clearly ascertained. One other lesson we have learned from this review of conjunctival inflammation, viz., that general bleeding, carried so far as to produce syncope, will sometimes completely empty the capillaries of an inflamed part of the red blood wherewith they were, just before, so turgid.

I shall next request your attention to a part of the organ which is strictly internal—to the iris: that thin curtain, with a circular aperture nearly in its centre, which hangs between the cornea and the crystalline lens, and is bathed on both sides by the aqueous humour. This little part, the office of which is to regulate the quantity of light admitted to the retina, is of exceeding interest in respect to its morbid as well as its healthy conditions. It is frequently the seat of inflammation; and, small as it is, the inflammation seems to be entirely confined to it, or to the surfaces immediately before and behind it. No doubt, with inflammation of the iris, there is in many cases inflammation of the choroid and retina also, and of the sclerotica. But the inflammation seems to make the iris its point of departure, and there it works its most striking changes. We cannot see so well what is the actual condition of the choroid and retina; but we have this proof, either that they do not always participate in the disease, or that they often suffer less than the iris; viz., that when the natural pupil has been closed up by lymph, and a new or artificial one is formed, vision is frequently restored.

The little cavity across which the iris is vertically stretched, is lined by a smooth membrane, the source of the watery fluid always contained in the cavity. This membrane is analogous in its smoothness, in its forming a shut sac, and in the nature of its secretion, to the serous membranes met with in other parts of the body: it is analogous also to the serous membranes, in its behaviour under inflammation. It is, in fact, the serous membrane of the eye. Now we have the means of inspecting a portion at least of several of the *mucous* surfaces of the body; but this serous cavity, constituting the anterior chambers of the eye, is the only *serous* cavity into which we have the privilege of looking, and of noting what is going on, when the membrane that forms its boundary is inflamed; and this it is that makes iritis, to me, one of the most interesting of all diseases. There is no single part of the body from which you can derive so much instruction concerning some of the minuter processes of inflammation, and concerning the power of certain medicines over those processes, as you may by watching a few examples of inflammation of the iris.

All the changes which occur in iritis depend upon the circumstance that the inflammation, like that of the serous membranes generally, is of the adhesive kind; i. e., is attended with the effusion of coagulable lymph. By means of this lymph the form and the colour of the part are changed: the size and figure of the pupil undergo alterations, or that aperture is completely closed up; the motions of the iris are limited, or entirely put an end to.

The symptoms which characterize inflammation of the iris are very obvious. To be perceived and understood, they require only to be looked at. Yet they long escaped notice, and even now are not always so carefully studied as they deserve to be. Not a great while ago I had to convince a surgeon of some pretensions, that he did not know this disease when he saw it. And English surgeons and physicians were all of them ignorant even of its existence as a distinct disease, until a most excellent account of it was published by a German, Schmidt, in the first year of the present century.

What are these plain and obvious symptoms that were so long overlooked, or that were not understood when seen? They are the following. I will first enumerate them, and then speak of each rather more particularly. Redness of the sclerotica; a change in the colour of the iris itself, and in its general appearance; irregularity of the pupil, produced by adhesion of the iris to the neighbouring parts; immobility sometimes of the pupil from such adhesion; a visible deposition of coagulable lymph. All these changes are apparent and conspicuous. Scientific writers term them *objective* symptoms. Then there are also the *subjective* symptoms, of which the patient alone is conscious—impaired sight; pain in the eye, and around it.

The redness is such as I formerly described as resulting from the vascularity of the

sclerotic. The cornea is surrounded by a zone of fine straight converging pink lines, very different in appearance from the tortuous, anastomosing, scarlet blood-vessels of the inflamed conjunctiva. These hair-like converging lines stop abruptly at the edge, or just before they reach the edge of the cornea; they dip through the sclerotic, in fact, to go to the iris. The vascular zone therefore is well defined in front, while it becomes fainter from before backwards, and is gradually shaded off; the posterior portion of the sclerotic being generally pale. As the disease advances, and in violent cases, the more superficial conjunctival vessels also sometimes enlarge, and mingle their tint of redness with that of the sclerotic, and more or less confuse or conceal it. Now this red zone or halo continues as long as the inflammation of the iris continues, and disappears when that ceases. It is an important symptom therefore.

The change in the *colour of the iris* itself is also a remarkable circumstance. You know that what is called the colour of the *eye* is simply the colour of the *iris*. When lymph begins to be effused into the texture of this coloured part, it deepens, and at the same time alters, its tints. A gray or blue eye is thus rendered yellowish or greenish. A dark eye presents a reddish tinge. The change is such as would be produced by a mixture of the colour of the lymph with that which is natural to the iris. But besides a variation of colour, the peculiar brilliancy of the surface is spoiled. It becomes dull and tarnished as it were, and the fibrous arrangement, which is usually so evident, is confused or gone. The change commences at the inner or pupillary margin of the iris, and extends gradually towards the outer or ciliary edge. This is a symptom which you can scarcely overlook. It is rendered certain and unequivocal by comparing the sound eye with that which is inflamed.

The change of colour which I have been describing is occasioned by the *effusion of lymph*. But the same event of inflammation leads to various other changes, not less striking, and more important, in so far as the functions of the organ are concerned. The lymph becomes visible upon the surface of the iris. Its precise appearance varies considerably in different cases. Sometimes it presents little spots like freckles, or specks of rust: or a thin stratum of the same colour is deposited. Sometimes it exhibits the appearance of drops, or (as they have improperly been called) tubercles, embossing the surface, and projecting from its pupillary edge. These are commonly of a yellowish or reddish brown colour, and they vary in magnitude from the size of a small pin's head, to that of a large shot. There are seldom more than two or three of these masses. The lymph thus effused upon, or thrusting forward the surface, is confined almost always to that part of the iris which is nearest to the pupil, to the annulus minor; while its ciliary portion, or annulus major, is dull and clouded. Sometimes, when the inflammation is very violent, or the disease has been neglected, actual suppuration takes place. A reddish yellow prominence arises from the surface of the iris, and at length breaks, and discharges matter which sinks down to the bottom of the anterior chamber, and presents the appearance that has been called *hypopyon*. All these changes, I say, become perceptible near the margin of the iris; its free edge, which in the natural state is clear and sharp, becomes rounded and blunt: and at the same time the pupil often begins to lose its jet-black colour.

Another very common consequence of the effusion of lymph from and upon the surface of the iris (from its hinder surface, that is, which is called the uvea, or from its pupillary edge), is its adhesion to the capsule of the crystalline lens, which lies, you know, behind the iris and very near it. And the pupil itself is apt to become blocked up by lymph.

The motions of the iris are seriously impeded by the mere effusion of lymph into its texture. At first it moves sluggishly under variations of the light; gradually the pupil contracts, and becomes fixed and motionless. The adhesion of the iris to the capsule of the lens still more decidedly restrains the action of the part. When it adheres at one or more points of the margin, and remains free elsewhere, the pupil is deformed; loses its circular shape; becomes angular; and this deformity is the most marked when the eye is examined either under a weak light, which allows the pupil to dilate, except at the points where the iris is tied down to the lens; or under a very strong light, which forces the free portions of the margin, and those only, to approach the centre. Still more palpable does the alteration of figure become when the pupil is artificially dilated.

Vision is always impaired in this complaint: partly because the posterior tunics of the eye are liable to be implicated in the inflammatory process; partly by the detriment done to the proper function of the iris, which should duly measure the quantity of light admitted to the retina; partly by the presence of more or less lymph, filling up the pupil; and partly by a change, not yet mentioned, which is apt to take place, especially in severe cases, in the cornea, and perhaps in the aqueous humour. The cornea becomes hazy and dull, and loses its bright polish. It looks like a piece of glass that has just been breathed upon. It has been thought (on the ground of analogy chiefly) that the aqueous humour grows turbid under the inflammation of the membrane that secretes it: just as serous effusion into the pleura is often found to be troubled and thick. But there is no sure evidence that this is the case. While the cornea remains transparent, the aqueous humour is seen to be clear: when the cornea is dim and semi-opaque, we cannot distinguish the state of the aqueous humour.

Acute iritis is attended with pain and intolerance of light. To the latter circumstance is probably owing the contraction of the pupil during the progress of the inflammation: and then the lymph *fixes* the pupil in that state of smallness and contraction. There is pain in the eyeball itself, and in the parts about the eye, the brow and temple, most severe at night. There is much variety, however, in regard to the pain. Sometimes it is constant and severe, but still more aggravated in nocturnal paroxysms. Sometimes, even when the quantity of mischief that is visible is very great, scarcely any pain at all is experienced.

The same remark applies to the constitutional symptoms. In some instances these are but slightly pronounced; but in most cases, particularly in acute cases (for iritis, as I have hinted before, is sometimes a chronic disease), there is a good deal of fever and headache, the pulse is full and hard, and the tongue white, and the sleep is broken.

If the progress of the inflammation be not checked, it extends itself beyond its original seat. It creeps from the pupillary margin to the ciliary; and thence it passes on to the ciliary body, to the choroid coat, and to the retina; and as this takes place, the pain and the pyrexia increase, and blindness is usually the result. The delicate texture of the retina is spoiled for ever.

I have thus described the phenomena of iritis generally: and I will next consider, in the same manner, the *treatment* which it requires. It will afterwards be necessary for me to mention certain modifications of the disease, in respect to its rate of progress, its causes, and the circumstances under which it occurs. I say it will be necessary to mention these modifications, because they require a corresponding adjustment of the plan of treatment.

When we have to deal with iritis alone—that is, when the inflammation and the changes to which it may have led, are confined to the iris—the disease is always, I believe, manageable; and affords a beautiful instance of the power of well-directed remedial measures. We cannot always tell whether the inflammation has been restricted to the iris or not.

We have three powerful weapons wherewith to combat iritis: *blood-letting*; *mercury*; and a remedy that hitherto has not been mentioned in these lectures, *belladonna*.

If I were restricted to the use of one of these means, I should choose mercury; if to two, mercury and belladonna; but the combined employment of the three has the most powerful effect in curing the disease; and cases that have seemed almost desperate, have been retrieved and rescued by these remedies.

With respect to *blood-letting*, I shall not run the risk of fatiguing you by dwelling at any length upon the mode in which it should be employed, or the indications for its adoption. I shall content myself with saying that the intensity of the local symptoms, especially of the pain,—and the degree in which the general symptoms, the fever, and the hardness of pulse, are present,—offer the best measure, both of the necessity for bleeding, and of the amount to which it ought to be carried. Both must be estimated also in reference to the strength and constitution of the patient. Bleeding from the arm till some decided impression is made upon the circulation; cupping from the temples; or both these modes of taking blood, together or in succession, may often be required. At the same time active purgatives should be exhibited; and the whole of the antiphlogistic regimen strictly enforced.

But bleeding, assisted by purgatives, and the antiphlogistic regimen, will not cure

the disease; or it will not cure one case in a hundred. It may *stop the inflammation* probably, but not till the organ has been spoiled. Such a termination cannot with any propriety be called a cure. We want not only to put an end to the inflammatory process, but to repair the mischief which may have been already done.

Yet bleeding is not to be held cheap, or neglected, because it is unequal to the cure of iritis. It is often productive of direct benefit by abating the force of the circulation, and by checking the progress of the local inflammation: and it is productive of great indirect benefit by preparing the system to submit itself more readily and rapidly than it otherwise would, to the specific influence of mercury. Mercury is our sheet-anchor in this disease.

After free blood-letting, then, or after such abstraction of blood from the system, or from the part, as the circumstances of the case may dictate, you must administer *mercury* in the manner that I formerly recommended. The object is, in acute cases, to affect the gums as speedily as possible; the soreness of the gums, and the peculiar fœtor of the breath, being the tokens that the whole capillary system feels the specific influence of the remedy. Calomel with opium is, in most cases, the best form in which mercury can be introduced into the system; the purpose of the opium being to prevent the calomel from running off by the bowels. Two, three, or four grains of calomel, with one-fourth, one-third, or one-half of a grain of opium, should be given every four, or six, or eight hours. Equal doses at equal intervals.

Some persons prefer giving the calomel still more frequently; one grain, for instance, with one-tenth or one-eighth of a grain of opium, every hour. If the gums do not rise in the course of thirty-six or forty-eight hours, and a speedy effect be desirable, inunction of the mercurial ointment should be added. And in some cases mercurial frictions alone may be sufficient, and the most expedient. Or the *hydrargyrum cum cretâ*, in five or ten grain doses.

You may have bled your patient sufficiently, and purged him well, and yet, on looking into his eye, you perceive the mischief to be still going on, and the deposition of lymph increasing. But the instant that his gums and breath acknowledge the specific agency of mercury upon his system, a welcome change becomes apparent: the red zone surrounding the cornea begins to fade; the drops of lymph to lessen; the iris to resume its proper tint; and the puckered and irregular pupil once more to approach to the perfect circle; till, at length, the eye is restored to its original integrity, and beauty, and usefulness.

I speak now of favourable cases. The changes for the better that I have been describing are sometimes rapidly accomplished, sometimes slowly. If the disorder have been long neglected, irreparable damage may have been done; the effused lymph may have become organized; or firm adhesions may have been already contracted between the iris and the lens. But even in cases of some standing, when the inflammatory action has in a measure subsided, the use of mercury will sometimes greatly improve, sometimes altogether restore, the impaired vision.

With the mercury, both before and after its specific influence is ascertained, we combine the use of *belladonna*.

Doubtless you are all aware of the singular effect of this vegetable poison upon the iris. It dilates the pupil. Now it is of great importance in iritis, to prevent the tendency to contraction which the pupil manifests. If we can artificially dilate the pupil, we may prevent the iris from forming adhesions with the capsule of the crystalline lens; and if it have recently contracted such adhesions, we may, while the lymph is yet soft, stretch or break them. And this power of artificially dilating the pupil we possess in the agency of *belladonna*, and of certain other narcotic vegetables. This remarkable power of the *belladonna* was first discovered, accidentally, by our countryman, the celebrated Ray. He tells us that a noble lady of his acquaintance applied a leaf of the plant to a small ulcer, suspected to be cancerous, just below one of her eyes. The pupil of that eye became greatly dilated, and the membrane remained motionless under the strongest light. This effect gradually subsided when the leaf was removed. But it took place on three several occasions. and was witnessed by Ray himself. Other vegetables have the same property; henbane, for example, stramonium, and the cherry laurel. And there are others which have it not, although we might have expected that they would possess it, from the analogy they bear to the

former in other respects. It has been ascertained that neither hemlock, nor aconite, nor foxglove, nor opium, have any such power.

Preparations of *belladonna* are chiefly, if not exclusively, employed in ophthalmic disorders in this country. The extract is used in two ways. After being made soft and semifluid by admixture with distilled water, it is smeared freely around the eye, upon the lids, and brow, and forehead. This is washed off after remaining an hour: generally it produces a marked effect upon the pupil.

A more efficacious and speedy mode of dilating the pupil is to drop a solution of the extract *into* the eye itself. The solution is to be made by rubbing down a scruple of the extract in an ounce of distilled water, and filtering the fluid through linen. Two or three drops of this solution are to be introduced between the eyelids.

Some very interesting experiments have been made in Germany by Dr. Reisinger upon this power of *belladonna* and of *hyoscyamus* to contract the iris—in other words, to dilate the pupil. The result of these experiments is given in the 24th volume of the *Edinburgh Medical and Surgical Journal*. Dr. Reisinger procured *atropine* and *hyoscyamine*, the active principles of the two plants, and made comparative experiments with these principles, and with the coarser extracts; and he concludes that the former are much to be preferred to the latter. Thus, he dissolved a grain of *hyoscyamine* in ten minims of water, and introduced a small drop of the solution into the eyes of several dogs and cats. No irritation whatever of the eye was produced in any instance, but the pupil was so much widened by the application, that in an hour's time only a small ring of the iris could be seen beyond the edge of the cornea; and after three hours, the pupil seemed as large as the cornea itself. The dilatation did not begin to diminish till after three days; and the pupil did not recover its natural dimensions until the sixth day. Then he applied a solution of the *extract* of *hyoscyamus*, made by mixing five grains with ten minims of water. This evidently caused irritation of the organ, which lasted from five to eight minutes, and was evinced by a discharge of tears, by the animal's shutting its eyes, and rubbing its eyebrows with its paws. Much less dilatation of the pupil followed, and continued not more than six or eight hours in dogs, and about twenty-four hours in cats. As soon as Dr. Reisinger had satisfied himself that the *hyoscyamine* had no injurious influence either upon the conjunctiva, or upon the deeper seated textures of the organ, he applied it to the human eye. He dissolved a grain of *hyoscyamine* in a drachm of distilled water, and inserted a drop of the solution into the eye of an old lady of seventy-one, who had cataract. So great was the consequent dilatation of the pupil, that only a narrow ring of the iris remained visible. No irritation whatever of any part of the eye was produced; and the dilatation continued for seven days.

As chemistry is now furnishing to us every day, in greater abundance, and with more ease, the active principles of various of our medicinal vegetable substances, we shall soon, in all probability, adopt *hyoscyamine* or *atropine*, for artificially dilating the pupil, instead of the preparations now in use. Till that time arrives, you had better smear the surrounding skin with the moistened extract of *belladonna* whenever the eye is painful or much inflamed. But under other circumstances, the solution dropped into the eye is to be preferred for its readier action, and its greater power. The use of this curious virtue, possessed by certain plants, is not confined to the cure of *iritis*. It enables the surgeon to introduce instruments through the pupil with greater facility and safety. It affords us also the means of examining the deeper seated textures of the eye: and it is of great service to many persons who are partially blind; to such, for example, as have central specks on the cornea, or central opacities of the crystalline lens. To such persons it gives the power of enlarging the window of the eye; of admitting more light; and of having painted upon the retina, and represented to the mind, the images of objects which, but for the mysterious agency of these poisonous vegetables, they could never hope to see at all. Fortunately, this power of *belladonna* over the iris does not diminish by repetition. Mr. Lawrence mentions two patients of his, one of whom had used it habitually for four or five years, and the other for fourteen or fifteen; and it dilated the pupil just as

¹ The study of the textures lying deep within the living eye has been wonderfully aided by the modern invention of the ophthalmoscope; an instrument which Mr. Bowman assures me is not less instructive and valuable in respect to ophthalmic diseases, than the stethoscope in respect to the diseases of the heart and lungs.

well at the end of these periods as at the beginning. By carefully examining an eye in which lymph has recently been effused, you may distinctly see the good effects of the artificial dilatation of the pupil. Little strings of adhesion are often visible, connecting the edge of the iris with the surface of the lens; and these are stretched, and not unfrequently broken, under the influence of the belladonna: and minute black spots may sometimes be seen upon the capsule, marking the points where the uvea had stuck, and where it left behind it, when it was detached by the belladonna, a portion of its peculiar pigment. These black points are indelible. There is one case recorded in which the pupil, after being dilated by belladonna, became fixed in that condition; probably by lymph subsequently effused into its texture, and binding together its fibres. Even this is better than that the pupil should be *contracted* and fixed.

These three remedies, then—bleeding, mercury, and belladonna—are the means by which we may hope to subdue inflammation of the iris, and to repair the ravages it has occasioned. With respect to the most important of the three, mercury, there are some points that require to be further noticed.

You may ask to what extent the mercury should be pushed, and how long it should be continued?

Why we have, in iritis, an illustration of what I have more than once mentioned before, viz., that the rapidity of a disease will require a corresponding haste in the use of its remedy. In acute and violent cases, the mouth should be made decidedly sore, as quickly as possible; and when that has been done, the further administration of the mercury may be suspended. "Full salivation," says Mr. Lawrence, "quickly produced, cuts short recent disease, as if by a charm." In cases of longer standing, or of slower progress, we must be slower in the introduction of the remedy: it will be enough to obtain any, the smallest certain evidence of its action, in the gums and breath; and we must keep up that moderate influence for some time. For what precise time it is impossible to say; but till the redness is gone, and the natural colour of the iris returns, and all the visible lymph has disappeared, and the sight is perfectly restored; and this *may* require a month or two.

When you look from day to day into the aqueous chamber of an eye in which iritis has recently produced its peculiar changes, and after the due effect of mercury upon the gums has been achieved, you will be surprised as well as delighted to see large masses of lymph rapidly disappear, melt away, as it were, from the surface of the iris, while that which had been deposited in its intimate texture, rendering it confused and discoloured, as quickly clears off. And you will be inclined to believe, as many have done, that mercury has a vast influence in promoting and accelerating absorption. It may have such a power: I am not disposed to deny it; but that it really has so we cannot safely infer from such circumstances. It clearly has the power of arresting the deposition of lymph; of putting an end to the adhesive inflammation. Whether it does anything more towards completing the cure, we have these reasons for doubting. When blood chances to be effused into the anterior chamber; or pus; or when, as frequently happens, pieces of a cataract that has been broken up pass through the pupil, and show themselves between the iris and cornea; they (the blood, the pus, the fragments of the lens) disappear, *i. e.*, are absorbed, just as rapidly as the lymph in iritis, although not a particle of mercury is taken. Mr. Lawrence even gives a case of syphilitic iritis, which got well without any affection of the gums by mercury, and which had been marked by the deposition of a large mass of lymph on the iris; and he says that the lymph was immediately absorbed, as soon as the inflammation ceased; and that he never saw it disappear more quickly under any circumstances.

There is one local use of mercury which I must not omit to mention, because though it probably has no share in *curing* the complaint, it is productive of great comfort and relief to the sufferings of the patient. It is adapted to those cases in which severe pain is felt round and over the orbit of the eye at night. Ten grains of the strong mercurial ointment, intimately mixed with two grains of finely powdered opium, and well rubbed into the temple a little while before the nocturnal pain is accustomed to recur, will in many cases completely prevent it. We owe this piece of practice to the Germans.

Iritis is apt to occur from different *causes*, and in connexion with different diseased states of the system. It is no uncommon accident from surgical operations performed

upon the eye, the iris suffering mechanical injury. The inflammation thus excited is usually violent and acute, and requires that the whole plan of treatment that I have been sketching out should be actively prosecuted.

But inflammation of the iris sometimes arises slowly and insidiously, without vascularity enough to call attention to the eye, and without pain. This generally happens when the eye has been strained by over-use; in women who occupy themselves with fine needle-work; in engravers, and such as are accustomed to look at *minute* objects, or at *bright* objects. A more common effect of continued exertion of the eyes in this way, is a diseased state of the *retina*; but (however the fact may be explained) the iris is sometimes the part that suffers. In this form of the disease mercury will often be found a successful remedy; but its influence must be gradually brought about; and it is not so certainly productive of benefit as when it is employed in acute iritis:—probably because the chronic inflammation has involved the posterior tunics also.

But most frequently iritis is met with in combination with syphilitic, or with rheumatic disease, which manifests itself at the same time in other parts of the body. Syphilitic iritis is more common than any other. It is one of the *secondary* symptoms of syphilis; and accordingly it is commonly associated with other *secondary* symptoms; with syphilitic eruptions, nodes, pains in the limbs, and ulceration of the throat. It is also one of the *earlier* of these secondary affections, and therefore is sometimes the only one to be seen; and occasionally it declares itself before the primary disease is well. The pain that attends this species of iritis is chiefly felt at night, but at that time it is apt to be very severe and distressing, so as entirely to prevent sleep until it takes its departure in the morning. We cannot, I believe, distinguish syphilitic iritis with any certainty from other acute varieties of the same complaint, by mere inspection of the eye. However, there are some points worth remembering in respect of the local phenomena which it most commonly presents.

Syphilitic iritis is never attended (according to Mr. Lawrence) with *abscess* of the iris, and hypopyon; the lymph is usually deposited in *distinct masses*; and the pupil becomes angular, and is not unfrequently displaced towards the root of the nose, by the adhesions which the iris has contracted with the parts behind it. In another variety of inflammation of the iris (which I shall mention to-morrow, *arthritic* iritis) lymph is equally effused from the margin of the iris, but it is not usually deposited in a distinct drop-like form. We ascertain the variety of iritis with which we have to do by these peculiarities; by the co-existence of other tokens of syphilis; by the periodical character of the nightly pain; by taking into our account the age, the constitutional habit, and the probable state of morals of our patient. Syphilis, you know, is not uncommon in children; it is sometimes even congenital; but it very seldom affects the iris at that early period of life. Among a large number of syphilitic children brought to Mr. Lawrence, he never witnessed iritis but once.

It was in *syphilitic* iritis that the curative power of mercury over adhesive inflammation was first distinctly recognised. But you must not fall into the error of supposing that the success of the remedy depended upon the specific character of the disorder; upon its connexion, I mean, with the venereal virus. Mercury is fully as serviceable, and as sure, in common acute inflammation of the iris. Upon this point all men of experience are agreed. “Its influence (says Mr. Lawrence) is not confined to the syphilitic form of the disease, but extends *equally* to the idiopathic.” And Dr. Farre bears testimony to the same effect.

LECTURE XX.

Iritis, concluded. Rheumatic Ophthalmia. Amaurosis.

THE principal theme of the last lecture was that most interesting disease, inflammation of the iris.

The symptoms of iritis are these : a radiating zone of vascular redness situated in the sclerotica, and surrounding the cornea ; a change in the colour of the iris, from gray or blue to a yellow or greenish tint, from brown or hazel to a dusky reddish hue ; a visible deposit of lymph upon the anterior and innermost portion of the iris ; a thickening of its free edge ; contraction, irregularity, and immobility of the pupil ; *closure* of the pupil by lymph ; adhesion of the uvea to the membrane of the crystalline lens. All these we can see and ascertain for ourselves. We can ascertain also the presence of fever, which attends the acute forms of the disease. And we learn from the testimony of our patient that his sight is impaired ; that the influx of light into the eye hurts him ; and that he experiences pain in and around the organ, especially at night.

The grand remedies in iritis are three :

1. *Blood-letting* : of which the objects are to abate the force of the heart's action ; to moderate the febrile disturbance ; and to facilitate the operation of the second remedy : which is

2. *Mercury*. This is to be given so as to produce soreness of the gums, and the peculiar fœtor of the breath : and these effects are to be sought for rapidly or gradually, according as the inflammation of the iris is recent and acute, or moderate and chronic. The object of this remedy is to arrest the effusion of coagulable lymph ; to put a stop to the adhesive inflammation.

3. The application of the extract of *belladonna*, or of a solution of atropine, to the conjunctiva, or to the skin around the eye, so as to dilate the pupil. The objects of this measure are to prevent the adhesion of the iris to the parts in its neighbourhood ; to detach it from the lens when it has already been glued thereto by soft lymph ; and to stretch and elongate the bands of adhesion when they cannot be broken : and thus to obviate any impairment of the free movements of the iris, and any deformity of the pupil, and the inflammation shall have ceased.

I began to speak of the causes of iritis.

I say it may be occasioned by mechanical injury ; as during the operation for the extraction of a cataract. A clean cut, however, is frequently followed by no bad consequences ; a portion of the iris has been shaved off by the knife in making the section of the cornea, without any injurious results. When iritis is excited by mechanical violence, it is acute.

2dly. A chronic form of iritis is sometimes brought on by excessive employment of the eye, in looking at minute or bright objects.

3dly. The most common species of iritis is that which arises in connexion with syphilitic disease. It is one of the early secondary symptoms of syphilis. It is marked by the co-existence of other secondary consequences of the introduction into the system of the syphilitic poison, and by the periodical character of the nightly pain ; it is never attended with abscess of the iris and hypopyon ; the lymph that is effused is deposited in separate masses ; and the pupil is often displaced towards the root of the nose, as well as rendered irregular, by the adhesion of the iris to the capsule behind it.

4thly. It is curious enough that iritis has actually been ascribed to *mercury*, as a cause. This notion can only have arisen from that loose kind of logic, and hasty generalization, for which, I am sorry to say, medical reasoners are too often distinguished. Mercury is perpetually exhibited for the cure of syphilis ; and people who have been treated for syphilis are very liable to iritis. This seems to be the only foundation for the opinion in question. When we come to appeal to facts, we find no ground for believing that this mineral is thus, both bane and antidote. If it were

so, Benvolio's advice to the slighted Romeo might be very pertinently offered to the *patient* in such a case :

"Take thou some *new* infection to thine *eye*,
And the rank poison of the old will die."

Mr. Lawrence has seen no instance of iritis, of whatever kind, in which there has appeared to him any reason for attributing the occurrence of the complaint to this cause. I have never heard it alleged that persons who have taken large quantities of mercury for other diseases, as for affections of the liver in India, are particularly subject to inflammation of the iris. On the other hand, iritis has come on, in hundreds of cases, in connexion with syphilis, though not a particle of mercury had been swallowed by the patients.

Lastly, there is a peculiar form or variety of iritis, called the *arthritic* or *rheumatic*.

This affection is characterized by the following *general* features. It occurs in persons who are subject to gout or to rheumatism, and often forms a part of the attack of the one or the other of those diseases. Like them it is liable to return again and again ; and this circumstance it is which makes arthritic iritis a serious disorder. It is seldom that much or permanent damage to vision is effected by a single attack ; but adhesions readily form under it, and lymph is effused ; and in each successive attack fresh effusion takes place : the pupil becomes more and more contracted ; and it may be filled up, at last, by an opaque plug of lymph. Some patients, however, will suffer ten or a dozen recurrences of the disease, and recover almost completely, and enjoy perfect vision in the intervals, before the sight becomes much impaired.

Some of the *local* appearances are more or less characteristic of this variety of iritis. It is seldom attended than the syphilitic variety by a deposition of lymph in distinct masses ; the contracted pupil keeps its central position, and is not displaced towards the root of the nose, as it is apt to be in syphilitic iritis. The adhesions that bind the iris to the neighbouring parts are said to be *whiter* in this variety of iritis than in others. It is also a very remarkable circumstance that the zone of red vessels encircling the conjunctiva does not approach so close to the cornea as in other species of iritis ; but a white ring is left between the cornea and the anterior margin of the zone. Sometimes the circular white stripe is partial, being most marked towards the angles of the eye ; sometimes, on the other hand, it is as perfect as if it had been described with a pair of compasses. I believe, with Mr. Welbank, that the appearance of this bluish ring depends upon the less intense degree of the sclerotic inflammation. He says that he has noticed its coming on, when syphilitic inflammation of the iris was beginning to yield to the action of mercury ; although there had been no such interval during the height of the inflammation. Again, the colour of the zone is not so bright as in other forms of iritis ; it is of a somewhat livid, or slightly purplish tint ; and the larger vessels at the back part of the eye, belonging to the conjunctiva, are apt to become tortuous and varicose.

Rheumatic *iritis* is often met with in combination with what is called rheumatic *ophthalmia* : a disease which I have not before mentioned. But each may exist alone. And as rheumatic iritis, though frequently an independent disease, does also in many instances grow (as it were) out of rheumatic ophthalmia, I will take this opportunity of shortly describing the latter complaint.

What is called *rheumatic ophthalmia*, then, is inflammation affecting the fibrous coat of the eye, the sclerotica. We know that the fibrous tissues throughout the body are frequently the seat of rheumatic inflammation. Some persons are more liable to rheumatism than others—are more readily affected by its external exciting causes, which are vicissitudes of temperature, and exposure to and cold wet. In such persons there seems a tendency to take on inflammatory action in all the structures of the same kind : and most particularly in the fibrous membranes, and tendons, that help to form the various joints ; and as the sclerotica partakes of this fibrous texture, so it is apt to suffer, in its turn, from rheumatic inflammation. The connexion of the moveable eyeball with the head may be considered as a sort of *joint*. The local symptoms are not in general of a violent kind ; and, as in other parts, the rheumatism seldom leads to any permanent alteration of structure ; seldom, at least, when the ophthalmia is confined, as it often is, to the sclerotia alone. Perhaps the best way to put you in possession of the features that belong to rheumatic ophthalmia will be to

describe an actual instance of it. I will take a well-marked example, related by Mr. Lawrence. He was sent for to see a gentleman who was suffering from what is commonly called rheumatic gout: swelling, some redness, and severe pain of one foot and knee, and one hand; aching of the back; and great constitutional excitement. He got well under the treatment adopted. After a short interval, upon Mr. Lawrence's calling to inquire how he was, he said there was something the matter with his eyes; and asked to have them examined. "I looked at them hastily," says Mr. Lawrence: "the room was dark, and the day dull; and I saw no appearance of disease. When I called again, after a few days, as the complaint was repeated, I examined more attentively. On bringing him towards the window, he obviously felt the light troublesome; he drew down the eyebrows, and half closed the lids, to avoid it. The conjunctiva was natural; but the whole of the sclerotica had a livid red, and mottled appearance, which might have been called dull, or almost dirty, in comparison with the red colour of common active inflammation. The sclerotic vessels were partially distended; the redness terminated short of the cornea, so that there was a distinct white rim round the latter. Vision was perfect; there was no pain so long as the eye remained at rest; but exertion of the organ, particularly under strong light, brought on uneasiness. The nature of this gentleman's occupations, and of his tastes, which were literary, prevented him from giving his eye the necessary repose; and the condition of the sclerotica just described lasted for three or four months:" so that Mr. Lawrence was apprehensive that some serious mischief would ensue to the organ. The affection remained confined, however, to its original seat, evincing only that obstinate character which belongs to disorders of such structures; and at last, it disappeared completely, leaving the eyes with their organization and powers unimpaired.

The treatment that appears to answer best in simple rheumatic ophthalmia of this kind, consists in moderate topical bleedings, and counter-irritation: with such other measures as conduce to improve the general health; and among these change of air and scene have sometimes a decided effect. Those remedies also are to be given which have been found by experience to be beneficial in rheumatic inflammation, although we cannot always *depend* upon finding them useful: colchicum, I mean; bark; sarsaparilla; the iodide of potassium. In these abiding or frequently recurring forms of disease, you will often be obliged to try the so-called specific remedies one after the other.

Now when the rheumatic inflammation is not confined to the sclerotic, but creeps inward, as by their vascular connexions it easily may, to the *iris* also, we name the disease according to the most important part that it occupies — *arthritic iritis*. On the other hand, when, with that affection of the sclerotic which I have been describing, there is combined a moderate degree of inflammation of the *conjunctiva*, this complex disorder receives a compound denomination: it is called *catarrho-rheumatic ophthalmia*.

Dr. Mackenzie states it as the result of his experience, that arthritic iritis seldom occurs in connexion with the earlier appearance of gout, while the patients still retain strong powers of digestion, and have the means of indulging their appetites; but rather with the asthenic and irregular forms of gout and rheumatism: when repeated attacks have been followed by mental depression, indigestion, flatulence, and languor. He has generally met with the disease in subjects beyond the age of fifty, very frequently in tobacco-smokers, and whisky-drinkers, who have often suffered rheumatic affections, who are teased by headaches, acidity of stomach, bad gums and teeth, and lowness of spirits: in persons, that is, whose health has been impaired and broken by intemperate habits. I believe you will find this to be a very correct statement; although arthritic iritis *may* also take place in those who are more robust.

After what has now been stated you will be prepared to believe that arthritic iritis neither requires nor bears those free emissions of blood, and that liberal use of mercury, which are necessary for the cure of other varieties of the complaint. Mercury, pushed to salivation, is sometimes found to do more harm to the system than good to the eye; and in a disease which is so apt to recur, we must not be continually salivating our patient. I can only say that the treatment must be conducted on the *principles* already laid down, and adapted to circumstances. If there be much fever, and a hard pulse, and a white tongue, you should bleed and purge your patient, and afterwards give him from twenty minims to half a drachm of the wine of colchicum

two or three times a day. When the symptoms are less active, you must be less active too: strive to set the disordered digestive organs right, and to correct the bad habits of the patient: give small doses of mercury (such as five grains of Plummer's pill) three or four times a week; excite counter-irritation by blisters, or by the croton-oil liniment. After the use of bleeding or leeches, and the regulation of the bowels, preparations of iron, the sulphate of quina—tonics, in short,—have been found, in not a few cases, extremely beneficial.

I should have mentioned another remedy, which of late years has been recommended in iritis, and especially in syphilitic iritis, by Mr. Carmichael of Dublin: not as being a better remedy in itself than mercury, or so good, but as having considerable power over the disease, and as affording, therefore, a valuable resource when from any cause the exhibition of mercury is forbidden. This remedy is the *oil of turpentine*. He gives it in drachm doses, three times a day. He relates cases of syphilitic iritis in which the pain, redness, and other symptoms, were quickly removed, and effused lymph was absorbed, and vision restored, under the use of this medicine. It is necessary to its beneficial action that the bowels should not be confined. In other instances of the same disease Mr. Carmichael was not so successful. Mr. Guthrie, who has also tried this remedy, reports of it that "in some cases it succeeded admirably, in others it has been of little service, and in some unequal to the cure of the complaint." I do not know that it has been fairly put to the test in arthritic iritis.

I proceed next to quite a different kind of ophthalmic disease from any that we have yet considered. I have spoken of inflammation of the exterior membrane of the eye occurring separately; and of inflammation of certain internal parts, and particularly of the iris, occurring separately. Between these exterior and interior tunics, the sclerotica forms a sort of natural barrier or shield, the chief point of connexion between them being near the edge of the cornea, where the sclerotic vessels dive through to reach the iris. Inflammation of the sclerotic itself has also been described. When vision is impaired or destroyed in consequence of any of the complaints which have hitherto engaged our attention, that effect results from the partial or total exclusion of light from the retina. The cornea is left opaque, or it bursts; the pupil, or aperture in the iris, is shut up by a web of lymph; or the capsule of the lens to which the iris adheres has undergone a change, and lost its transparency. In each case the retina suffers an eclipse.

But light may be freely admitted, and yet no vision ensue. The transparent parts of the eye, the several media, so skilfully and exquisitely adjusted for the due refraction and collection of the rays of light into an image of the object from which they flow, may all be perfect and in order; but the beautiful apparatus is useless; the patient cannot see with it. The fault is in the *nervous* matter that should receive and transmit the impression, and render it an object of perception to the mind.

Now persons in this condition are said to have *amaurosis*. The term is derived from the Greek word *αμαυρος*, which signifies obscure or dark. It expresses various degrees of imperfect vision, from defective nervous function. The words *gutta serena* are applied to that form of amaurosis in which vision is totally lost. It was formerly supposed that this sort of blindness was caused by the effusion of some humour or fluid behind the pupil: and this was held to be a clear fluid, because the natural blackness of the pupil is sometimes not troubled in amaurosis. Milton has literally translated this term when, speaking of his own eyes, he says,

"So thick a *drop serene* hath quenched their orbs."

Amaurosis is a very *obscure* disease. It is capable of being caused by various changes, the exact seat and nature of which we often have no means of determining during life: and which frequently leave no traces behind them in the dead body. It would take a much larger space than I can possibly devote to it in these lectures, thoroughly to discuss this difficult but interesting subject. I shall endeavour to give you such a sketch of it as you may fill up and complete by future observation, and by reading, for yourselves. It will be something to learn the direction and objects of our inquiries into what is yet unknown in the pathology of this affection.

There is one division of the disorder which immediately suggests itself. The cause

of defect may exist in the brain, at or beyond the origin of the optic nerve; or it may be situated in any part of the course of that nerve, from its commencement at the base of the brain to its termination in the retina; or it may be confined to the retina itself.

There is reason to believe that the functions of the retina may be impaired or suspended by deviations from the natural quantity of blood sent to it; by disturbances of its circulation. Various degrees of amaurosis are common among persons who employ the sense of vision overmuch, and strain the eye. This over-use is likely to produce congestion, or chronic inflammation, in the vessels of the retina; and very slight changes of that kind may seriously affect the function of a part so delicate and tender. I say we frequently meet with amaurosis among those whose occupations oblige them to look attentively at small or bright objects during many hours of the day; or what is still more pernicious, during many hours of lamp or candle light; so as habitually to fatigue the eye;—engravers, printers, watchmakers, tailors and milliners, mathematical instrument makers, persons who gain their bread by writing, miniature painters, cooks who are exposed to the heat and glare of large fires, men who have the charge of forges or furnaces, and so on. Here a continual stimulus leads to a chronic disorder, which increasing in intensity may terminate in total blindness. We call these cases of *amaurosis*, but they may be justly considered to be instances of *chronic inflammation* of the retina. We cannot indeed see the suffering part during life; and the complaint is not a fatal one, and, therefore, we have few opportunities, or none, of examining after death the condition of the retina while the amaurosis is yet recent. But judging from the nature of the *causes* that precede the defect of vision, and from the nature of the *remedies* that are often found to remove it, we are warranted in regarding the essence of the disease to be *retinitis*. The same condition, apparently, may be suddenly produced by the transient operation of some more powerful cause of congestion; such as intense light. I will illustrate this form of amaurosis—amaurosis, that is, dependent upon congestion which perhaps amounts to inflammation, sometimes slowly established, and sometimes very suddenly—by the narration of a few cases. I may as well premise, however, that the treatment which promises most, or I should rather say, which has performed most, in this form and kind of amaurosis, is very nearly the same (excepting the use of belladonna) that I have already recommended for chronic and acute iritis: *blood-letting*, general or topical, according as there are more or less pain, and fever, and fulness of the system, and according as the amaurosis is more or less recent; and above all *mercury*, so administered as to affect the gums, and rapidly introduced into the system in the acuter cases; more slowly in proportion as the disease has crept on more gradually and lasted longer. This treatment is very often quite successful; the mercury is the most important part of it; and we have in this fact a strong corroboration of the inference drawn from the nature of the exciting causes, viz., that the complaint is essentially inflammatory. And again, supposing it inflammatory, we need not be surprised that a remedy, the curative effect of which we can see in inflammation of the iris, should be equally serviceable when the same diseased process is set up in the retina, which we cannot see. Purgatives, counter-irritation, and perfect repose of the eye, are necessary parts of the treatment in both forms of disease.

Mr. Allan gives the following account of the master of a printing office who became blind. He had corrected the press, and was otherwise engaged in reading, for eighteen hours daily out of the twenty-four. He continued this practice for twelve months, notwithstanding an evident failure of his sight. At the end of that time the amaurosis was so complete that he could not distinguish one object from another, but was merely capable of just perceiving the light, so as to grope his way along the streets. He continued in this state for several years, but ultimately recovered his vision.

The next instance that I shall cite is recorded, in these words, by Mr. Lawrence. "A young woman, of florid complexion and full habit, came to the London Ophthalmic Infirmary complaining that she had lost the sight of one eye. She was cook in a family, and occupied for several hours daily before large fires, supporting her strength by free living. The pupil was slightly dilated; the iris motionless. A faint and scarcely perceptible pink tint was observed in the sclerotica near the cornea. Vision was dim, and had been so for three days. There was headache, flushed coun-

tenance, heat of skin, whitish tongue, and thirst. I considered the case to be pure retinitis; and to afford a favourable opportunity for showing whether the affection could be arrested by antiphlogistic treatment. At that time (now many years ago) I did not possess the knowledge of the power of mercury in inflammation of the retina, which subsequent experience has given me. I directed a full bleeding from the arm, free purging, low diet, repose of the organ, and general rest. At the end of two days the sight was worse: cupping and a blister were now ordered; but there was no improvement at the end of two days more. I now determined on trying mercury, and ordered two grains of calomel every four hours. Before the remedy had affected the system, vision was quite lost, or at least reduced to the mere power of distinguishing light from darkness. Full salivation, which took place in about a week from the first application of this patient at the infirmary, suspended all the symptoms; the sight immediately improved, and was soon completely restored."

A soldier, unacquainted with the proper method of observing an eclipse of the sun, employed for that purpose a piece of opaque glass, with a transparent point in its centre. Notwithstanding the vivid and painful impression he experienced from the rays that passed through the lucid part of the glass, he continued to look at the sun till the eclipse was over, using his right eye. He was soon after seized with vertigo, and pain in the right side of the head, and found himself almost entirely deprived of the sight of the right eye. Some weeks afterwards, the pain in the head continuing, he came under the care of Baron Larrey, who observed that the vessels of the eye were injected; the pupil somewhat smaller than that of the other eye, retaining, however, its natural freedom of motion; the vision very obscure or almost gone. This man recovered his sight completely after two bleedings, one from the temporal artery, the other from the jugular vein; blisters to the temple and nape of the neck; ice to the head, and moxas.—(MACKENZIE, from the *Mémoires de Chirurgie*.)

In the year 1832, a young man standing in a door-way, by a lamp-iron, in a thunder-storm, was struck by the lightning, fell backwards, and was convulsed. He said afterwards that the lightning appeared to enter his eye with a scorching sensation. During the night vision was quite lost. The next morning there was no redness, nor any unusual appearance of the eye. The iris was motionless, however, and the patient could not see even the sun. He was treated with calomel, and his sight returned; but the retina remained extremely irritable, and unable to bear the light. A month afterwards, when this account was written, he could see distinctly enough, but he could not use his eyes without the protection of blue glasses.—(LAWRENCE.)

In these cases the nervous apparatus that ministers to vision is not, I believe, in general, the only part of the nervous system that is injured. In August, 1839, Phoebe Judge, a delicate-looking girl, eleven years old, became my patient in the Middlesex Hospital. She had lost, in a great degree, the power of using her legs: when she attempted to stand they separated, and she sank down. She had not perfect control over her bladder. The desire to make water was frequent, and if not immediately attended to, the urine escaped in spite of her efforts to retain it. The same urgency, and inability to wait, occurred whenever her bowels were about to act. Sensibility in the legs and thighs was impaired, but not extinct.

Her parents informed me that some time previously, while stooping to raise up a sister in a room at Hampstead, she had been struck by lightning, fell backwards, became blind, and remained so for ten days. She did not lose her consciousness, but complained immediately that the lightning had hurt her eyes. They presented no visible injury or defect, but the upper lids fell, and she was unable to raise them. It was soon found, however, that when pressure was made on the right eyelid she could open the other eye. The palsy of her limbs commenced, by degrees, two or three days afterwards. The power of vision returned suddenly, and at the same moment the power of moving her limbs was restored: but it gradually went again. When she lay down her limbs were still; but they began to tremble and to be agitated as soon as she sat up. Even when lying in bed, she had, occasionally, a sensation and dread, as if she were falling down. She had been in this state nearly three weeks.

She was put upon steel, and a tonic plan of treatment, and in ten days she could walk, dragging her left leg a little after her. In ten days more she was dismissed quite well, and able to run from one end of a long ward to the other.

The greater number of the cases of amaurosis depending upon a morbid condition of the retina itself, belong to the class that I have now been mentioning; there is congestion of the vessels of the retina; or inflammation, chronic or acute. In a few instances a totally opposite condition of the blood-vessels is presumed to exist. I say presumed to exist, because our judgment of this matter is founded, as before, upon the nature of the circumstances that have caused the affection, and upon the nature of the treatment that removes it. On these grounds some cases of amaurosis (few in number, speaking comparatively) may fairly be ascribed to a deficient supply of blood to the vessels of the retina. We know that a temporary defect of sight may be produced by a diminished circulation through the retina, as in approaching syncope under hæmorrhage; and we can therefore the more readily believe that more permanent amaurosis may be occasioned by causes that gradually lessen the quantity of blood circulating in the body, and debilitate the whole system. "It is well known (writes the late Dr. Gooch) that large losses of blood enfeeble vision. I saw a striking instance of this in a lady who flooded to death. When I entered the chamber she had no pulse, and she was tossing about in that restless state which is so fatal a sign in these terrific cases. She could still speak; asking whether I was come? (she knew I had been sent for) and said, 'Am I in any danger? — How dark the room is! I can't see.' The shutters were open, the blind up, and the light from the window facing the bed fell strong on her face. I had the curiosity to lift the lid, and to observe the state of the eye. The pupil was completely dilated, and perfectly motionless, though the light fell full upon it. Who can doubt that here the insensibility of the retina depended on the deficiency of its circulation?"

One might ask, also, who can doubt that the retina *may* become insensible from a similar state of the circulation in it, brought on by some *long-continued* drain upon the system? Amaurosis of this kind, proceeding from too profuse and protracted a secretion (which may be considered a sort of hæmorrhage), is sometimes noticed in nurses. Mr. Lawrence describes the case of a young mother of slender make, who suckled her first child, which was strong, and took the breast very often: her milk was abundant. After two or three months she began to feel very weak, could not lift a weight, and cried frequently, without having any moral reason for grief. She became totally blind, and was led to his house by a friend. He found her pallid, with a small feeble pulse. The pupils were of middle size, and the irides moved slightly. The retina was completely insensible. She could not discern the situation of the window, nor see a lighted candle held close to her. After weaning the child, and using generous diet, she got perfectly well. Some counter-irritation was employed in this instance, but I question whether it had anything to do with the recovery. Such cases are not uncommon, and their well-known occurrence has probably tended to encourage the notion — too prevalent among both patients and practitioners — that amaurosis is always and essentially a disease of debility, and requires tonic and stimulant remedies; bark, and high feeding, and strychnia, and electricity. "Our eyes are *weak*," say they, "and we require strengthening medicines." You must perceive from what has already been said, how necessary it is to *discriminate* in such cases: to look closely into all the circumstances under which the disease has occurred.

When amaurosis is the result of pressure or of disease, in the course of the optic nerve, or in the sensorium, the complaint is generally less within the power of remedial measures. We cannot say, indeed, in many instances, where the cause of defect lies: and in obscure cases, I should always advise a trial of the mercurial plan. I have again and again seen slight palsy of some of the voluntary muscles, evidently depending upon some morbid condition of the brain, clear away rapidly upon the affection of the gums by mercury; and the lost power of the retina will sometimes return under similar treatment.

There is something very peculiar in the expression of countenance, and in the gait, of an amaurotic person, by attending to which alone, you may almost recognise his disease. He comes into a room with an air of uncertainty in his movements; the eyes are not directed towards the surrounding objects; the eyelids are wide open; to use a strange but common and intelligible phrase, the patient seems gazing upon vacancy — has an unmeaning stare; and there is a want of that harmony of movement and expression which results in a great measure from the information obtained by the exercise of vision. This seeming stare at nothing at all, is not observed in patients

who are blind in consequence of opacity of the crystalline lens or its capsule, *i. e.*, in consequence of cataract. They, on the contrary, while they cannot see, still seem to look about them, as if they were conscious that the power of sight remained to the retina, although light was shut out from it.

When the amaurosis is incomplete, the motions of the iris are sluggish, and the pupil is larger than ordinary. When the blindness is total, the commonest condition of the eye is that of great dilatation of the pupil, with complete immobility of the iris. A mere ring of iris is all that is visible, and no change takes place in the diameter of the pupil, under the greatest variation of the light that falls upon it.

Sometimes, on the other hand, though the amaurosis be total, the iris is as active as ever; and this is a very interesting circumstance, and may help us, in some degree, to conjecture the actual seat of the malady. When the amaurosis is confined to one eye, *this* may happen. You examine the diseased eye, and you find that the pupil enlarges or contracts, as you diminish or increase the light. But the other eye is open. Shut the sound eye, and try the amaurotic eye again, and you find the pupil fixed, although you vary the light. The motion you formerly noticed was sympathetic with the motion of the iris in the healthy eye. We express this otherwise by saying that the *associated* movements of the iris were natural and lively, but its *independent* movements were lost. But sometimes the independent movement is unaffected: nay, the motions of both irides may be perfect, although both eyes are completely amaurotic. I may state, by the way, that *cateris paribus*, when both eyes are affected, that is a ground for supposing the cause of the disease to be situated within the cranium. And I should come to the same conclusion if, in the case where one eye alone was amaurotic, I found the independent motion of the iris of that eye unimpaired. We know that in the healthy condition of the parts, the brightness of the light admitted to the retina determines the size of the pupil; but the motions of the iris do not depend solely or directly upon the retina. It has been ascertained, by experiments made upon animals, that the pupil may be made to contract either by mechanical irritation of the *optic* nerve within the cranium, or by irritation of the *third* nerve; a motor nerve which sends filaments to the ophthalmic ganglion, whence the ciliary nerves, passing to the iris, are derived. Now the optic and the third nerves have some link of connexion within the brain; and if the morbid condition upon which the amaurosis depends is situate deeper than that point of connexion, we may understand, I think, how disease so placed may destroy the power of vision, and yet leave the connexion between the retina and the third pair unaffected: and then the influence of light falling on the retina, though it fails to create a perception in the mind, will be reflected back upon the third pair of nerves, and so continue to govern the motions of the pupil. In conformity with these views, M. Andral relates cases in which amaurosis connected with disease in the cerebellum was attended with brisk movements of the iris.

There are other causes of amaurosis besides those that I have already adverted to. It is sometimes produced by the presence of worms in the alimentary canal.

[That amaurosis is frequently dependent upon irritations seated within the stomach and bowels, and upon derangements of the digestive organs generally, there can be little doubt. We have met with many cases of this kind, and they are repeatedly referred to, more especially by the German writers on the disease. Children confined in ill-ventilated and ill-lighted apartments, and supplied with coarse and indigestible food, are often affected with a certain degree of amaurotic blindness, which is readily removed by a proper hygienic treatment, and such remedies as are adapted to restore the regular functions of the stomach and alimentary canal. We have observed the disease, also, in children who have been, at too early an age, confined in crowded school-rooms for many hours of the day, while their minds were compelled to the performance of tasks beyond their powers. Complete blindness, we have known suddenly to occur in consequence of the presence of indigestible food in the stomach, and to be as quickly removed upon its expulsion. For further information on this subject, the reader is referred to the chapter on amaurosis by Dr. Taylor, in the 2d volume of Tweedie's Library, Philadelphia edition, page 515, and to the very able paper of Dr. Jacob on the same subject in the Cyclopædia of Practical Medicine, Philadelphia edition, vol. i., page 78. — C.]

It has some obscure connexion with teething, probably through irritation of the facial branches of the fifth pair. A physician of my acquaintance, residing in London, had a young son, who on two or three occasions caused him great uneasiness, by becoming blind in one eye without any obvious cause, and with no visible change in the organ; but the blindness on each occasion went off again, apparently in consequence of the extraction of some teeth which had grown irregularly. I am assured by Dr. Ashburner that such cases are common. Mr. Lawrence relates a very singular instance of *dental irritation* giving rise to amaurosis. A man, thirty years old, was suddenly attacked with violent pain in the left temple near the eye, and in that side of the face generally. The pain continued to recur from time to time, and at length he discovered that he was blind in the left eye. By and by the cheek swelled, and some spoonfuls of bloody matter were discharged by a spontaneous opening in the lower eyelid, and then the pain subsided; but after some months it returned with great severity. The patient then went to Wilna, with the intention of having his eye extirpated, and consulted Professor Galenzowski, who found the left eye totally insensible to light, with the pupil dilated, and no other visible alteration. He ascertained, however, that the first molar tooth on that side was carious: it had never caused the patient much uneasiness; and the toothache which he *had* occasionally suffered had not been coincident, in point of time, with the pains in the head and eye. Dr. Galenzowski thought fit to extract this tooth, and was greatly surprised at seeing a small substance protruding from the extremity of its fang. This proved to be a little splinter of wood about three lines in length, which had perforated the centre of the tooth, and had probably been introduced in using a wooden toothpick. A probe passed from the socket into the antrum, from which a few drops of a thin purulent fluid escaped. The pain ceased almost entirely, and on the same evening the eye began to be sensible to light. The vision gradually improved, and on the ninth day from that time, after thirteen months' blindness in that eye, he was able to see with it as perfectly as with the other. M. Galenzowski has since been in England, and he showed Mr. Lawrence the tooth, and the splinter of wood. Doubtless he felt some pride in exhibiting these trophies of his exploit.

Amaurosis is said also to occur as an *hysterical* affection; and I am certain that I have seen this myself. An unmarried lady, of a very nervous and susceptible habit, came to town in great apprehension about her eyes, the sight of one of them being quite gone. I could perceive no defect in the eye itself. I saw her in consultation with Mr. Travers, who took an unfavourable view of the case, and thought the chance of recovery was very slender. I had one reason for hoping a better result, in the knowledge of some facts which Mr. Travers was not aware of till I mentioned them to him. I had been acquainted with this lady for some years, and during that period she had several times almost entirely lost, and again recovered, the use of her lower extremities. On two occasions she had been affected with aphonia, and unable to speak, except in a whisper, for months together; and then, on a sudden, without any apparent cause, her voice returned. I trusted, therefore, that this suspension of the power of vision in one eye might be a similar freak; and so it turned out. A few weeks subsequently, the sight returned, she knew not how; and she afterwards lost it a second time, and a second time regained it.

Certain poisons will produce temporary amaurosis; and the suppression of certain natural evacuations, as of the perspiration, of the menstrual fluid, and of the bleeding from piles, and the repulsion of certain eruptions, have been charged, by authors, with producing the same complaint.

In those cases in which amaurosis creeps on slowly and insidiously, as it is apt to do from various causes; and more particularly when it depends upon a low and chronic inflammation, engrafted upon habitual congestion of the vessels of the internal tunics of the eye; its approach is marked by sundry curious affections of the vision. The eye feels full or stiff, and sometimes there is pain of the head in its neighbourhood. The patient complains that he sees things through a fog or mist, or as if a thick piece of gauze were interposed between his eye and the object he is looking at. In the daylight, the gauze or fog seems dull and murky, but in the dark it often appears shining, reddish, and fiery; the flame of a candle is seen surrounded with a halo of prismatic colours. That amaurosis of this kind is often really dependent upon local congestion we are taught by the *ludentia*, by the circumstances that aggravate

it: thus *straining* of any kind, which augments for the time the fulness of the vessels about the head, will make the mist appear more dense; the same effect may be produced by tying the neckcloth tight, or even by stooping. Boerhaave relates the case of a man who, whenever he was intoxicated, laboured under complete amaurosis. It came on by degrees, increasing according to the quantity of wine he drank; and after the drunkenness went off, his vision returned. Surely these phenomena are very illustrative of the way in which nervous disorders may arise, or be made worse, from mere local plethora, in almost any part of the body.

Sometimes the perfect amaurosis is preceded by a remarkable diminution of the apparent size of the objects looked at. A patient told Dr. Farre that a carriage, which happened to pass the window, seemed to him as small as a wheelbarrow, and the horses no bigger than dogs. More commonly ocular spectra become visible: that is, parts of the retina lose their power, or perhaps are eclipsed by turgid vessels: the patient sees flies in the air, *muscæ volitantes*, particles of soot, *blacks*, as we, who live in London, call them, which always float before his eyes, and seem to follow their motions; and which are especially plain and troublesome when he is looking upon a white surface. They multiply in number till the whole becomes dark.

Do not, however, suppose that the appearance of these *muscæ volitantes*, even when they are permanent, necessarily implies the approach of amaurosis. I should be sorry if it were so, for I see two of them every morning, when my eyes are directed towards a white basin, while I am washing my face. I can find them at other times if I look for them; else I am not sensible of their presence. They bode no further evil, if they are associated with no other defect, in function or in appearance, of the instrument of vision.

You will infer from what I have been saying that there are two kinds of *muscæ volitantes*,—the one a harmless kind, the other suggestive of further mischief. And it is so. And as patients will often be coming to you in a fright upon first perceiving these objects, you ought to be able to distinguish the innocent sort from the dangerous, so as to allay your client's alarm, or to direct his course of proceeding, as the case may be. The distinction is, in general, easy to be made; and it is well set forth in an interesting article in the *North British Review* for November, 1856. The paper is anonymous, but it bears internal evidence of having been written by Sir David Brewster.

Whoever will look through a minute pinhole in a card at the clear sky, by daylight—or through a lens of short focal length at a candle, by night—may see floating before his sight a number of translucent tubes or fibres, and many little beads, of which some are separate, some attached to the tubes, some apparently within them. The tubes or fibres are some straight, others looped or twisted, others again forked. All these objects are bright in the middle, and bounded by fine black lines. Beyond and parallel to the black lines which belong to the larger fibres, may be seen an appearance of coloured lines or fringes. The doublings and crossings of the knots or loops in the twisted fibres represent black points. Though the eye be fixed, these bodies are observed to change their position; some of them moving faster, some farther than others.

Now, in ordinary light and vision, all these objects are imperceptible, except in certain cases the knots, and perhaps some of the fibres, when they happen to be large; and these knots and fibres, thus seen, constitute the harmless kind of *muscæ volitantes*. They are described by several writers as resembling the knots in a deal board, with long fillets extending from them of irregular shape, and edged with black parallel threads. If the eye be fixed on a white surface after a sudden shake of the head, they appear to sink gradually downwards. Those among you who are versed in the science of optics will recognise the black lines and fringes that I have spoken of, as being phenomena of the inflexion or diffraction of light which are never seen but in divergent rays. All *muscæ volitantes* having such fringes must be situated at a greater or less distance from the retina. In fact, there are conclusive reasons for believing that they occupy the vitreous humour. They cannot, therefore, portend either amaurosis or cataract. Whereas those black specks which have no fringes, and which do not move, or which move only in correspondence with the motions of the eyeball, are flaws in the retina, insensible or eclipsed spots, and are therefore to be dreaded as probable harbingers of amaurosis.

The simple and easy criterion, then, is this. The *muscæ* which are motionless when the eye is at rest, and move with it when it is in motion, are signs of danger to vision. Those which sink gently downwards when the eye is fixed, are innocent.

Let me add, however, that although these harmless *muscæ* seem to descend, they must in reality be ascending; floating upwards in the vitreous humour as far as the partitions made by the hyaloid membrane will let them.

It is obvious that no particular rules, no rules, that is, which will fit all cases, can be laid down for the treatment of so multiform a complaint as amaurosis. When it manifestly results from disease of the brain, as when it accompanies hydrocephalus, or remains after a stroke of apoplexy, our attention must be directed to the disease from which it has sprung. When there is any reason to suppose that congestion or chronic inflammation of the internal tunics of the eye itself is concerned in the production of the amaurosis, we must adopt the measures that I have already described, as the most likely to remove the congestion; and especially the mercurial plan. When there is ground for suspecting that the blindness takes its rise in vascular exhaustion, or nervous debility, we must have recourse to tonics; bark, preparations of iron, nourishing diet, the cold bath.

After all, you will find too many cases, which will baffle your best-directed attempts, and in which you will be required and warranted to try other expedients. When what I may call rational measures have been expended in vain, you may have recourse to such as are empirical and tentative. There are various *stimulants* which have occasionally been found serviceable; but most of them, I believe, fail much oftener than they succeed. Electricity is one of these: it is applied by taking small sparks from the eyelids, and from the integuments round the orbit. The object of this is to rouse the dormant energies of the impassive nerve; and it appears sometimes to do this for the retina, as well as for the nerves supplying voluntary muscles. The same or a very similar agent may be conveniently administered by help of the electromagnetic apparatus. Mr. Ware tells us that electricity is most beneficial in those cases in which amaurosis has succeeded a stroke of lightning. You must take great care not to apply this remedy when there is any inflammatory action at the bottom of the complaint: it should seldom be tried therefore when the affection is recent.

Strychnia has, of late years, been used for the cure of amaurosis. I shall hereafter take an opportunity of telling you the ordinary effects of that substance upon the body, when given in a certain dose—what is its poisonous operation, and what may sometimes be hoped from it as a remedy. In amaurosis it does good, when it is useful at all, by stimulating the exhausted or atonic nerve into action. With respect to this remedy also I may say—first endeavour to ascertain that it is not likely to do harm; as it will be if the blindness depend upon any condition akin to inflammation. Mr. Middlemore, of Birmingham, has probably given this remedy an ampler trial than any other person, and he speaks very favourably of its effects in certain cases: in others he found it to produce so much pain, and spasm, and distress, that he was obliged to discontinue its use. It is not given, in these cases, by the mouth, but applied locally, and Mr. Middlemore considers that it is most efficient when placed over the supra-orbital nerve. He puts a narrow blister above the eyebrow; when it has risen he cuts off the cuticle, and applies a piece of linen, for half an hour, to absorb the serum that continues to ooze forth; then he sprinkles the strychnia, finely powdered, upon the raw part, and covers it with linen smeared with the *unguentum cetacei*. He repeats this every twenty-four hours, cautiously increasing the dose till the vision improves, or some sensible evidence of the agency of the strychnia becomes apparent. He commences with the sixth part of a grain.

I must here leave the subject of diseases of the eye.

In addition to the lessons which I pointed out before as capable of being learned by attending to the disorders of this small organ, I may now mention a few others, of no little moment, since we shall meet with their application again and again, as we proceed to investigate the morbid conditions of other parts. We have seen enough to convince us that mercury, properly administered, has the invaluable power of stopping adhesive inflammation; of arresting the effusion of coagulable lymph from the blood-vessels: that inflammation of a given part may be sensibly modified by the simultaneous agency of some specific poison upon the system, as that of syphilis; or

by the presence of constitutional tendencies to disease, such as are observable in gouty and rheumatic people. And we have seen that the functions of a nerve may be perverted, suspended, or abolished, in various ways: by *pressure* made upon it; by a *plethoric* state of its blood-vessels, or by an *empty* state of them; by *inflammation* of its texture, chronic or acute; and even, in some mysterious, or hitherto unexplained manner, by mere irritation of a distant part; by worms, for example, in the alimentary canal; by poisonous substances introduced into the stomach; and by what, in our ignorance, we denominate the freaks and caprices of hysterical disorder. All these lessons we shall find repeated, as the course advances.

LECTURE XXI.

Diseases of the Brain and Nervous System. Difficulties of the subject. Short Review of some points in the Physiology of the Brain and Nerves. Peculiarity of the Cerebral Circulation. Pressure.

HAVING considered some of the most important disorders of the eye, because they afforded me the means of illustrating many of the doctrines and principles, which I had previously endeavoured to lay before you, of *general pathology*, I go next to the diseases of that portion of the body, which, though it includes many distinct parts, is called, collectively, the head. I pass over the maladies to which the integuments of the head are liable, because they will fall more naturally and conveniently into the class of cutaneous disorders; and I come at once upon one of the most interesting, and at the same time most difficult and obscure subjects of special pathology — that which embraces the *diseases of the brain and nerves*. Though it will be a slight departure from the plan I have proposed of taking diseases as they affect different parts of the body from the head downwards in succession, I shall speak of the diseases of the spinal cord, and of the nervous system generally, in connexion with those of the brain. To disunite them would neither be easy nor useful.

The study of the maladies and disordered conditions of the brain and nervous system, is surrounded with peculiar difficulties: and, accordingly, our knowledge of these diseases is less precise than of the diseases of most other parts of the body.

1. One source of difficulty lies in the circumstance, that the structure of the nervous system has no perceptible or understood subservience to its functions. We do not discover in the mechanism of this system that adaptation of means to an end which is so conspicuous in many other parts of the body: and consequently, though such adaptation doubtless exists, we are not able to trace the reason or the manner of its interruption. We find in the lungs an apparatus of tubes and cells fitted for the reception of air, upon the expansion of the chest by the contraction of certain muscles; of which muscles also we can see and understand the action. If we meet with any obstruction of those tubes, or any obvious impediment to the play of those muscles, we perceive at once how and why the function of respiration is deranged. But no alterations that become visible, after death, in the brain or spinal marrow, afford us any such explanation of the interruption of *their* proper functions; which are, in three words, *sensation, thought, and motion*. An apoplectic cell has no relation, direct or inverse, that we are capable of appreciating, with a sentiment: nor a distended lateral ventricle with the exercise of the will. The morbid anatomy does not in any degree elucidate the disorder, simply because the natural structure throws no light upon the healthy office of the parts concerned.

2. It is a further source of difficulty, that physiologists have not yet been able to determine, with anything like precision or certainty, what share the several parts of the brain and spinal cord have in regulating, respectively, the functions which all physiologists acknowledge to belong to the nervous system in the aggregate. There

are many and convincing reasons, for believing that the brain is a complex organ; but we can seldom put our finger upon this or that portion of the nervous matter which composes it, and say, *here* resides the influence that governs this or that particular function.

3. Again, the brain and cranio-spinal axis are so encased by their bony coverings, that, in the living body, we are unable to ascertain their physical conditions by means of any of our senses. Of many parts of the frame we ascertain the state by the sense of sight; and of many parts which we cannot see, we still may recognise the changes by the faculty of touch, or by the ear. The brain and spinal cord we can neither see, nor hear, nor handle.

4. Besides these obstacles to the acquisition of information by the exercise of our own senses, concerning the organs affected, the very disturbance of the functions of his brain cuts us off, in many cases, from that kind of information which we might otherwise derive from the statements of the patient himself.

5. There is a still greater cause of perplexity, with which we have to contend. The very same symptoms accompany alterations of the brain apparently of a very different, nay of the most opposite kind: and on the other hand, changes of structure, which, as far as we can perceive, are absolutely identical in their nature, are associated, in different cases, with totally different symptoms: and more frequently than not, nervous diseases are attended with *no* alterations of structure, appreciable by our senses.

6. And lastly, we are perpetually asking ourselves, when we find the proper functions of the nervous system disordered,—is this disorder the result of disease in the nervous matter itself? or is it merely sympathetic of disease in other parts? for there are few diseases of any kind which do not, in some degree, modify or disturb the due exercise of the offices of the brain and nerves: and it is very difficult often, and sometimes it is impossible, to determine whether, and how far, the disturbance is primary or secondary.

With all its difficulties, however, the pathology of the brain and nerves is always full of interest. How can it be otherwise when we reflect that the nervous system is the medium through which we hold communion with the world around us; the stage upon which all the phenomena of animal life are transacted; the instrument of the mind?

And with all its difficulties, there is also a good deal, in the pathology of the brain and nerves, that is fairly made out, and well understood; and we are at present in the right way for advancing our knowledge of this intricate and mysterious subject, by that careful collection of facts, and rigid induction of particulars, which will lead, at length, to a safe and useful generalization.

I shall endeavour to point out to you what is *known* of the morbid conditions of the nervous system; I shall also state the conjectures and probabilities by which our judgment and practice must be guided, when absolute certainty is unattainable. With mere speculative questions, that have no practical bearing, I shall meddle as little as I can.

Our knowledge, I say, of the exact functions of the different parts of the nervous apparatus is scanty and imperfect. Some certainties, however, we possess: and some strong probabilities which almost amount to certainties. Without first expounding my creed upon these matters, it would be impossible for me to explain, as it would be for you to understand, the notions I entertain respecting many of the diseases of the brain and nerves.

Omitting the sympathetic nerve and its ramifications, (for we know but little of its office, and still less of its disorders,) the nervous system is made up of certain masses of nervous matter, called the *nervous centres*; and of *nerves* therewith connected.

The nervous centres consist of the cerebrum and cerebellum, the medulla oblongata and the medulla spinalis. I shall include the cerebral hemispheres, and the lobes of the cerebellum, under the common term, the *brain*. So I shall speak of the oblong and of the spinal marrow, in the single phrase, the *spinal cord*, or the *cranio-spinal axis*; their endowments appearing to differ more in relation and degree than in kind.

I adopt the belief that the grey (which are much the more vascular) portions of these nervous centres, form the part in which their peculiar powers reside, or are

generated: and that their white or fibrous portions are, like the white and fibrous nerves, mere conductors of the nervous influence.

I incline also to the opinion (recollect, if you please, that I do not press these opinions of mine upon you as being necessarily correct), that the influence which originates in the grey matter, and is transmitted by the white, will at last be found analogous, if not identical, with some modification of electricity. We know that some of the effects of this influence may be very exactly imitated, in animals recently dead, by galvanism.

The functions of the brain and nerves are sensation, thought, volition, and the power of originating motion. Other functions indeed there are; but these four are all that we need, at present, concern ourselves with.

Now it is a part of my creed that the faculties of sensation, of thought, and of the will, belong to the brain: in all probability to the cerebrum alone. The precise office of the cerebellum is involved in some obscurity and dispute. Of the principal opinions that have been formed respecting it, I shall say something hereafter.

The chief grounds for believing that the brain proper is, exclusively, the instrument of the mind, are these:—

1. Because this portion of the nervous centres is superadded to the cranio-spinal axis, in the greatest bulk and most complicated form, in man: and after him, in those of the inferior animals which show the largest share of reason.

2. Because injury or disease of the human brain does so often impair or abolish the mental powers.

3. Because in inferior animals which evince a certain amount of mental endowment, all manifestation of intellect ceases upon the gentle and gradual removal of the cerebrum and cerebellum: the animals continuing to live, for a long time, notwithstanding this mutilation.

Again, it forms part of my creed on these subjects that the motive power resides in the spinal cord.

The muscles furnish the instruments of motion.

Now there is a certain class of muscles which contract without our willing their contraction; and generally without our being conscious that they are contracting. Such are the heart, the muscular fibres of the alimentary canal, and of the bladder. These are, therefore, called involuntary muscles.

There is another large class of muscles, which obey the bidding of the will, and serve the purposes of prehension, locomotion, and bodily effort. These are considered and called voluntary muscles.

There is still another distinct set of muscles, of which the habitual action is involuntary, yet which submit also to the interposing control of the will. You will call to mind at once the muscles of respiration, which act while we are asleep, or otherwise unconscious; and the sphincters, which regulate the entrances and outlets of the body. Here, I say, the habit is involuntary, but the occasional action is prompted and governed by volition. But sometimes the involuntary action rebels against the willed action, and overcomes it. The muscle contracts in spite of the will.

Nay, those muscles which, ordinarily, move only in obedience to volition, do sometimes, under the influence of strong emotion, or of disease, contract independently of any effort of the will, and even in opposition to, and defiance of the voluntary power.

Under certain circumstances the limbs move with much briskness and force in decapitated animals, in which sensation and volition are extinct. Some physiologists hold, indeed, that sensation and volition are properties of the spinal cord; and they would object to these cases, that no one is warranted in affirming the movements in question to be independent of the will. The animal has no means of informing us whether it feels or not, any more than the human head that has been severed by the axe or by the guillotine.

This point, however, has been settled by certain phenomena which are observed to occur, in the human body, under disease. Limbs completely palsied as to voluntary motion, and quite dead as to sensation, do yet, under certain conditions, contract and move when the integuments are pinched; the rational patient neither feeling the pinch, nor being conscious of the movements.

Whence does the impulse that leads to motion in these cases proceed — how is the motive power awakened?

The answer to this physiological question has a most important bearing upon the pathology of the nervous system.

It is no part of my purpose to enter into any history of the steps by which this curious problem has been worked out. Its solution is an achievement of our own time; and I may add, of our own country. I profess no more than to sketch, in mere outline, the leading facts that have been ascertained; yet I must, in passing, pay the tribute due to one indefatigable labourer in this department of science, whose sagacity has enabled him to seize the clue, and in great measure to unfold the mazes, of the labyrinth in which this part of the physiology of the nervous system was so long entangled. Dim and uncertain glimmerings of the truth appear in the writings of bygone authors, but it was never clearly discerned, and plainly stated, and successfully applied to the elucidation of a large class of disorders, until the publication, in 1832 or 1833, of Dr. Marshall Hall's ingenious and most interesting researches into "the functions of the medulla oblongata and spinal cord." Similar views appear to have suggested themselves, about the same time, to Professor Müller of Berlin. I must recommend you to study the works of these authors; and I may also point out, as fit writings for your perusal (since the doctrines I am now speaking of are comparatively new), Dr. Grainger's *Observations on the Structure and Functions of the Spinal Cord*; Dr. Carpenter's two works, *Principles of General and Comparative Physiology*, and *Principles of Human Physiology*; and a very able paper on the Pathology of the Spinal Cord, by Dr. William Budd, in the 22nd volume of the *Medico-Chirurgical Transactions*.

If, on the other hand, you wish to see how nearly the idea, which has been so happily simplified into an intelligible principle by Dr. Hall, was reached by earlier observers, you may consult the writings of Dr. Whytt, upon nervous diseases.

What, then, respecting this intricate subject, are the main facts and doctrines which modern research has made clearer?

It seems ascertained, that the movements of those muscles which acknowledge the empire of the will, depend essentially upon some momentary change in the condition of the spinal cord. This change (whatever may be its nature) is capable of being effected in three several ways.

First, volition, or emotion, originating in the brain, may send down an influence, which travels with electrical rapidity to the spinal cord: whence, the requisite change having been instantly produced, the motive influence passes, with proportional speed, along the nerves which connect the cord with the muscles to be moved.

Secondly, the change productive of motion may be wrought in the cord, whether the brain be attached to it or not, by mechanical, chemical, or electrical agencies, operating directly upon the cord itself.

Thirdly, the change productive of motion may be wrought in the cord, by an influence carried to the cord, not from the brain, but from the extremities of nerves distributed upon the internal and external surfaces of the body.

The action of this nervous circle, whereby, I say, an influence is first carried from the surfaces of the body, along nerves to the spinal cord—whence again an influence is transmitted, or *reflected*, as it were, to certain muscles along certain other nerves—has been called by Dr. Hall the *reflex function* of the spinal cord. The apparatus subservient to this function is named by him the *excito-motory system*; the nerves which carry the impression to the cord are *incident* or *excitor* nerves; those which convey the motive impulse from the cord, *reflex* or *motor* nerves. Dr. Carpenter's terms (which I like better, except for their similarity in sound) are *afferent* and *efferent* nerves.

Mr. Grainger believes that physiology indicates, and anatomy can exhibit, *four sets* of fibres belonging to the nerves and the nervous centres. *Sensiferous*, and *volition* nerves, connected with the grey substance of the cerebrum, and subordinate to the exercise of feeling and of the will; and *incident* and *reflex* nerves, connected with the grey matter of the cord, and belonging to the excito-motory system.

Whether this be the true state of the case, or whether the efferent fibres be the same, while the afferent fibres are different; the latter coming to the spinal marrow both from the brain and from the various surfaces, just as two trains may arrive at

Euston Square ultimately by the same rail, although the one starts at Derby and the other at Birmingham; or (which is perhaps the better illustration) just as, in some houses, the same bell is made to ring in the servants' hall, by pulling, indifferently, the dining-room or the drawing-room rope:—which of these two hypotheses is the more correct, I am not competent to determine.

This reflex action, independent of the will, and although attended often by consciousness and sensation, yet often also exercised when there is neither, governs the orifices by which air and food are introduced, and excrements are voided. The infant breathes and sucks by it; the adult uses his will for bringing nutriment into his mouth; in both, the act of deglutition, after the food has reached a certain point, is involuntary. The expulsion of the fæces, the urine, the semen, and the fœtus, is regulated by the same function. Nevertheless, most of these muscular acts are capable of being moderated and directed by volition. The reflex power, on the other hand, extends, both in health and in disease, to the entire system of the strictly voluntary muscles; during health it is manifested only in the maintenance of what is called their *tone*, their natural tension and firmness: in disease, as we shall hereafter see, it sometimes acts upon them with terrific energy.

Some of the difficulties which I enumerated in the beginning of the lecture, as impeding our researches into the diseases of the nervous centres, are insurmountable. One or two of them, however, appear to call for a more attentive consideration.

I say we often fail to discover *any* deviation from the natural condition of these nervous centres, or of their appendages; even when the disorder of their functions has been broadly displayed.

We are not to infer, from this, that no change has taken place in these parts. The only legitimate conclusion is, that the nervous functions are liable to be deranged, impaired, or suspended, by altered conditions, not traceable by our senses, or at least not yet discovered by us, of the organs which minister to those functions.

There may be only one such undiscovered disturbing cause, variable in degree in different cases; or (what is more probable) there may be several such conditions differing in kind. A blow or fall, which *jars* the brain; a sudden mental emotion; an electric shock; a teaspoonful of prussic acid; any one of these causes may destroy life, yet leave no vestige of its action in the nervous substance upon which it operates. It is probable that the fatal condition is not, in each case, the same.

We may even form a reasonable conjecture of the manner in which the invisible changes are sometimes brought about. We can conceive, for example, that *undue pressure* upon the nervous pulp on the one hand, or *insufficient pressure* on the other, may constitute conditions of the kind we are in search of; and I shall be able, I think, to convince you that such is sometimes the case. Again, we can conceive that such conditions may be furnished by the varying state of the cerebral circulation. In point of fact, we *know* of some changes in the circulation through the brain which have the effect, invariably, first of modifying, and at length, if they are continued, of arresting, the cerebral functions. If *no* blood be sent through the arteries of the brain, death in the way of *syncope* ensues; if *venous* blood circulate in those vessels, it leads to death by *coma*.

But whatever may be the nature of the unknown, and perhaps fugitive, physical conditions of the nervous centres, thus capable of disturbing or abolishing their functions, it is useful to keep in our minds a distinct and clear conception of the fact that there must be some such physical conditions. By steadily retaining this idea of their real existence, we may hope, at length, to get some insight into their nature; which we are the less likely to obtain, if we dwell only on the obvious and visible injuries effected in the nervous substance; associated, as they are apt to be, with so perplexing a diversity of symptoms. Indeed, by the help of this distinct conception, we are at once enabled to reconcile some of the seeming anomalies and inconsistencies to which I before adverted. The same symptoms, I repeat, have been found to accompany physical lesions of the nervous centres, apparently different in kind, place, and degree: and on the contrary, physical lesions, apparently identical in their nature, extent, and situation, are attended by different and contradictory symptoms. We must not attribute the symptoms, in such cases, to the visible physical lesions, but to some unperceived condition of the nervous centres concomitant with those lesions. The *proximate cause* of the *symptoms* escapes our notice. The obvious physical changes

may be remoter causes of the symptoms — causes of this proximate cause: but they may also be merely contemporaneous effects of some other remote agency.

I have adverted to deviations from the natural and healthy circulation of the blood through the brain, as being capable of modifying the nervous functions. Of such deviation one mode which is conceivable, and which has been assigned as a presumed cause of morbid phenomena, is a variation in the relative quantity of blood contained respectively in the arteries and veins that lie within the cranium. And it seems probable enough, that a healthy condition of the cerebral circulation, may imply and require a certain balancing and adjustment of the amount of blood carried in these two sets of vessels. But with this theory—that disturbance of the functions of the brain may result from an altered ratio of the arterial and venous blood therein—has been associated another; namely, that although the blood may, at different times, be variously distributed between the cerebral veins and arteries, yet that the absolute quantity of blood circulating within the cranium, is always and necessarily the same, or nearly so.

This notion, broached by the second Monro of Edinburgh, and upheld (as it then seemed) by experiments upon animals by Dr. Kellie, received at a later period the sanction and approval of Dr. Abercrombie. And, resting upon such authority, I had been in the habit of delivering the same theory, not, however, without some misgiving as to its soundness, in these lectures. It has been completely overthrown of late, by Dr. George Burrows.

The doctrine was this. The brain is closely shut up in an unyielding case of bone. Its surface must therefore be exempt from the influence of atmospheric pressure. Hence, supposing its substance to be unaltered and incompressible, it would seem impossible to empty the blood-vessels of the brain. The cavity being completely full, the blood which circulates in those vessels can neither be materially increased, unless something is displaced or compressed to make room for the addition; nor materially diminished, without the entrance of something to supply the place of the blood subtracted.

Dr. Kellie noticed that while, in animals bled to death, the other organs of their body were emptied of their blood, and blanched—the brain presented its ordinary appearance, and even seemed to contain more blood in its superficial vessels than usual. Having satisfied himself upon this point, he varied his experiment. He first made a small opening in the skull, by means of the trephining instrument, taking away a little circular piece of bone, and *then* he bled the animals until they died; and in these cases he found that the brain was as completely drained of red blood as the rest of the body. He did that with respect to the cranium which house-keepers do when they tap a barrel of beer. You know that if the barrel be quite full, you may introduce a fawcet at its lower orifice, but no beer will run out through it. The pressure of the atmosphere operates upon that portion only of the fluid which is now exposed to the air, and its effect is to keep the beer in. But if you bore a small hole with a gimlet through the top of the cask, and so admit air to the upper surface of the beer, it will then flow readily through the lower outlet. Dr. Kellie imitated this process of making a *vent-hole*, when he trepanned the skulls of sheep, and admitted air to the yielding membranes of the brain.

He availed himself also, in these researches, of what he considered the converse experiment. He desired to ascertain whether, under circumstances calculated to gorge the vessels of the *head*, those of the *brain* were or were not made really more full than usual. With this object he examined the brains of two men who had been hanged. When the scalp in these cases was divided, a great quantity of blood escaped; marking plainly enough the congestion of the vessels *exterior to the cranium*: but there was no such congestion observable *within*. “The sinuses contained blood, but in no extraordinary quantity; the larger vessels on the surface and between the convolutions were but moderately filled; and the pia mater was, upon the whole, *paler*, and less vascular than we often find it in ordinary cases.” Similar appearances have been noticed by myself. I paid particular attention to the condition of the head, during the examination, below-stairs, of the body of Bishop, the murderer of the Italian boy. When the corpse was brought hither after the execution, the eyes were bloodshot, and the lips and countenance turgid and livid. The inner

surface of the scalp, when it was turned back, and the exposed surface of the skull, were very red and bloody; and in one part, on the right side of the head, there was some blood *extravasated*. But when the bone had been sawn through, and the skull-cap removed, the large veins of the brain did not appear unnaturally full.

In the year 1826 I was present in St. Bartholomew's Hospital, at the opening of the head of a woman who had been hanged the day before, for murder. I find the following statement in a note which I made at the time. "The scalp was bloody, but the brain was of very natural texture and appearance, and not more than commonly full of blood."

Among the propositions deduced by Dr. Kellie from his observations and experiments, were the following:—

1. That in the brains of animals that have died of hæmorrhage, there is no lack of blood, but, on the contrary, very often a state of venous congestion.

2. That congestion of the cerebral vessels is not met with in those cases in which we should most expect to find it; in persons, for example, who die strangled.

3. That the quantity of blood in the cerebral vessels is not affected by gravitation: in other words, that it remains the same, whatever may be the posture of the body, and the position of the head.

Dr. Burrows, distrusting the whole theory, and unsatisfied with the experiments by which it was fortified, determined to repeat them, taking care, as much as was possible, to exclude every conceivable source of fallacy: and he has shown, most convincingly, that Dr. Kellie's conclusions were erroneous.

First, he demonstrated that hæmorrhage *has* a most decided effect in depleting the cerebral blood-vessels, and in reducing the quantity of blood within, as well as upon the outside of the cranium. Two well-grown rabbits were killed; the one (A) by opening the jugular vein and carotid artery on one side of the throat; the other (B) by strangulation. Round the throat of the first, as soon as it was dead, a ligature was tightly drawn, to prevent any further escape of blood from the vessels of the head.

"The contrast between the two brains in point of vascularity, both on the surface and in the interior, was most striking. In the one scarcely the trace of a blood-vessel could be seen; in the other, every vessel was turgid with blood."

He next investigated the effect of posture upon the condition of the intracranial vessels.

"Two full-grown rabbits were killed by prussic acid; and while their hearts were still pulsating, the one (C) was suspended by the ears, the other (D) by the hind legs. They were left suspended for twenty-four hours; and before they were taken down for examination, a tight ligature was placed round the throat of each rabbit, to prevent, as effectually as was possible, any further flow of blood to or from the head, after they were removed from their respective positions.

"In the rabbit (C) the whole of the external parts of the head, the ears, the eyeballs, &c., were pallid and flaccid; the muscles of the scalp and bones of the cranium were also remarkably exsanguine. Upon opening the cranium, the membranes and substance of the brain were pallid, the sinuses and other vessels were exsanguine—anæmic beyond my expectation.

"In the rabbit (D), the external parts of the head, the ears, eyeballs, &c., were turgid, livid, and congested. The muscles and bones of the cranium were of a dark hue, and gorged with blood, which at some parts appeared extravasated. Upon opening the cranium, the membranes and vessels were dark and turgid with liquid blood; the superficial veins were prominent, the longitudinal and lateral sinuses were gorged with dark blood, and there was staining of the tissues, if not extravasation of blood into the membranes. The substance of the brain was uniformly dark, and congested to a remarkable extent."

From these experiments, Dr. Burrows draws the logical inference, that "the principle of the subsidence of fluids after death operates on the parts contained within the cranium, as well as upon those situated in the thorax or abdomen."

And of that absence of vascularity sometimes observed within the skulls of persons who have died of strangulation, he offers a very satisfactory explanation.

In the first place, the cerebral vessels *are*, in some instances, highly congested. Something will depend upon the position of the rope; which may press unequally

upon the jugular veins on the opposite sides of the neck, leaving one of them more or less pervious.

"But there is another still more efficient cause of the occasional absence of congestion of the cerebral vessels after death by hanging. It is the subsidence of the *fluid* blood after death, while the body is yet suspended, through the cervical vessels which are not completely obliterated by the pressure of the cord. And, it should be recollected, there are some channels which are scarcely, if at all, affected by the compression of the rope. These other channels are the vertebral sinuses, and special plexus of veins, so ably delineated by M. Breschet.

Moreover, the manner in which the corpse is generally examined, proves an additional source of fallacy. All the great vessels of the neck are usually cut across, and the viscera of the thorax removed from the body, before the skull is opened. Then, while the head is elevated, during the operation of taking off the calvarium, and examining the brain, the blood, still fluid (as it almost always remains, after sudden death of any kind,) "gravitates from the cranium, and pours from the divided cervical vessels into the chest."

In further confirmation of Dr. Burrows' conclusions, I will read to you from the *British and Foreign Medico-Chirurgical Review* for April, 1855, a short account of a very curious and conclusive experiment made at Berlin, under the direction of M. Donder:—

"A portion of the skull of a rabbit was removed, the corresponding piece of dura mater cut out, an accurately-fitting portion of a watch-glass let into the opening in the calvarium, and the junction made air-tight with gum. Whenever, by compressing the nose and mouth, respiration was intercepted, within ten seconds increased redness of the pia mater could be seen with the naked eye. This condition was made still more evident by the use of the microscope: and each time some minutes elapsed before the congestion again diminished. A depending position of the head also increased the hyperæmia. Rapid abstraction of blood very distinctly diminished the diameter of the vessels."

By this refutation of a prevalent error, not unlikely to warp or mislead our practice in some cerebral disorders, Dr. Burrows has done the science of medicine an essential service.

The theory which he has demolished involved probably more than one erroneous assumption. Dr. Burrows thinks that the anatomical structure of the human cranium does not warrant the opinion that its contents are withdrawn from the pressure of the atmosphere. "The numerous fissures, and foramina, for the transmission of vessels and nerves through the bones of the cranium, appear to me (he says) to do away with the idea of the cranium being a perfect sphere, like a glass globe, to which it has been compared by some writers. If there were not always an equilibrium of pressure on the parts within and without the cranium, very serious consequences would arise at the various foramina of the skull."

We fall back, therefore, upon another principle, whereby some of the difficulty and obscurity which attend certain affections of the brain and nerves may be explained. I mean the principle of *varying pressure* upon the nervous substance. Physiologists say that the cerebral matter is incompressible. This is another of the questionable assumptions implied in the foregoing theory. Upon what grounds the opinion may rest, I am ignorant: but whether the brain be compressible or not,—whether, that is, it be or be not reducible by pressure into a smaller compass, it is clearly capable of having different degrees of pressure applied to it, and of being pressed out of its ordinary form. We shall see, hereafter, that by pressure exercised from within, by the distension of what are called the ventricles of the brain, the convolutions on its surface are sometimes flattened, and the natural furrows between them nearly effaced. Pressure there certainly is in what I shall have to describe to you as *hypertrophy* of the brain. There must be considerable pressure on the nervous pulp when blood is poured out within it from a ruptured artery in cerebral hæmorrhage. But the phenomena noticeable when a portion of the skull has been removed by the trephine, show very clearly that the encephalon sustains pressure from varying states of the circulation during perfect health. The surface of the brain, seen through the circular opening in the bone, is observed to pulsate; and to pulsate with a twofold motion. With every systole of the heart, the surface protrudes a little; and it again subsides

with the succeeding diastole. This shows that the tension of the arteries, produced by every contraction of the ventricles of the heart, exerts a degree of pressure upon the contents of the cranium. But the brain has also an alternate movement, corresponding with the movements of the thorax in breathing: rising with every act of expiration, and sinking with every act of inspiration. Now, during expiration, the blood escapes less freely from the head through the veins; and thus again vascular fulness is found connected with evidence of pressure on the parts within the head. In further proof of this, if any were needed, I might again refer to Dr. Kellie's experiments. He removed a portion of the cranium of a dog by the trephine. The brain was observed to rise and fall alternately, but so as always to fill the cranium; the rise being marked by a sort of protrusion through the hole that had been made. One of the carotid arteries was now opened, and in a minute or two afterwards there was an evident gradual sinking and receding of the brain from the margin of the bone. So likewise, when the blood was flowing from the rabbit (A) in Dr. Burrows' experiment, "the conjunctiva was observed to become pallid, and the eyeballs to shrink within the sockets."

It is certain then that, whether the cerebral pulp yields or not, there is a constant alternation of a greater and a less compressing force, exerted upon it during life. It is not improbable that this continual variation of the compressing force may be essential to the performance of the cerebral functions. May not the brain be thus incessantly *charged*, if indeed it be (as has been suggested by no less a philosopher than Sir John Herschel) "an electric pile, constantly in action," discharging itself by the nerves, at brief intervals, "when the tension of the electricity developed reaches a certain point?" However this may be, it is equally certain that the compressing force may transgress its natural limits, in either direction; may be too great or too little. The functions of the nervous centres may be perverted, or lost, when the pressure becomes excessive; or, on the other hand, when the pressure is insufficient.

It is plain that excess of pressure may cause fatal coma, or defect of pressure fatal syncope, and yet no evidence of the operation of these causes be left in the dead brain. And we may explain, by the help of this same theory of pressure, a very singular phenomenon observed in certain forms of cerebral disease; I mean the *occasional* recurrence only of the symptoms, although the organic disease itself be *permanent*. For example, we see continually persons who are epileptic: that is, they have fits of convulsion and stupor *now and then*, and appear perfectly well in the intervals. After the death of such patients we sometimes find organic disease of the brain; a piece of bone perhaps projecting from the cranium, or a tumour, or a cyst: and this we are apt to consider as a sufficient explanation of the preceding disease; but we are always pressed with this difficulty: if the tumour or piece of bone were the cause of the paroxysms, why had the paroxysms any cessation?

It seems probable, or not improbable, that in such cases as these, and in many others, the permanent morbid condition is a predisposing cause only of the occasional symptoms; rendering the diseased organ more sensible to variations in the circulation; to accidental circumstances which determine an undue amount of compressing force, or a deficient amount: and I think Dr. Abercrombie has gone too far when he says "we may safely assert that the brain is not compressible by any such force as can be conveyed to it from the heart through the carotid and vertebral arteries."

Dr. Kellie narrates the following curious circumstance:—"Mr. G., with a numerous train of distressing symptoms, which too well marked the existence of enlargement of the heart, and the violent propulsive energy of that viscus, had only one, characteristic of any disturbance within the head. On looking upwards to the whitened ceiling of a room, he saw a darkened spectrum, which vanished and reappeared with great regularity. It was soon discovered that the appearance of this umbra was synchronous with the systole of the heart, so that he used often, in my presence, to count his pulse with the utmost precision, by keeping his eye fixed on the ceiling, and numbering every appearance of the spectrum." In this case it is presumable that by each contraction of the left ventricle of the heart, plethora of the cerebral blood-vessels was produced, and therefore an excess of pressure upon the cerebral substance. In that which I am about to quote it seems probable, on the other hand, that comparative emptiness of the vessels of the brain, and a consequent defect of the requisite degree of pressure, occasioned the morbid phenomenon.

A gentleman, thirty years old, was reduced to a state of extreme weakness and emaciation by some complaint of his stomach. As the debility advanced, he became very deaf; and this symptom varied in the following instructive manner. He was very deaf while sitting erect or standing; but when he lay horizontally, with his head quite low, he could hear very well. If, when standing, he stooped forwards so as to produce flushing of the face, his hearing was perfect; and upon raising himself again into the erect posture, he continued to hear distinctly as long as the flushing continued: as this went off the deafness returned. (ABERCROMBIE.) An old clergyman, who was formerly my patient, was troubled by two grievances: deafness and an intermitting pulse. They were both always benefited by quina.

Objections, I should tell you, have been raised against this theory of pressure affecting the functions of the nervous centres; but I think the objections are susceptible of a satisfactory answer. I must content myself, however, for the present, with having pointed out the main grounds upon which the theory rests. The difficulties that attend it, and the considerations which diminish the force of those difficulties, will come necessarily before us on a future occasion.

LECTURE XXII.

Symptoms of Cerebral Diseases. Inflammation of the Dura Mater and Arachnoid, from external injury; from Disease of the Bones of the Ear, and of the Nose; from the poison of Syphilis.—Inflammation of the Pia Mater.

THE functions of the brain, summarily expressed, being sensation, thought, and voluntary motion, we naturally look for disturbances of those functions whenever the organ suffers disorder or disease. And experience has made us familiar with various forms of disturbance to which these same cerebral functions are liable. Let us pass them shortly in review.

1. The faculty of *sensation* may be morbidly keen, or morbidly obtuse; or it may be perverted: in other words, it may deviate in degree, or in kind, from the healthy standard.

The sensations referred to the several surfaces and structures of the body, and to the organs of sense, may (without any fault in those parts and organs) be preternaturally acute. Tenderness ascribed to different parts, their natural sensations being heightened into pain; a general state of irritability; intolerance of light, and of noise; are so many instances of this over-sensitiveness of the percipient organ.

Under the head of diminished or defective sensation may be ranked, numbness in all its degrees, up to total loss of sensibility, or *anæsthesia*; dulness of hearing, deafness; dimness of sight, blindness; failure, or absolute extinction of the senses of taste and of smell.

Perverted sensations, sensations unnatural in kind, or unprompted by their special excitements, are very numerous. To mention a few: giddiness; nausea; ringing sounds in the ears; ocular spectra; ill smells in the nostrils; false tastes on the palate; itching; and sundry uneasy feelings, many of which are indescribable. Various kinds of pain belong to this class; spirits violently high; causeless depression, anxiety, and dread.

2. Innumerable degrees and varieties of disturbance of the faculty of *thought* are met with. Delirium in all its shades; dulness and confusion of intellect; sundry defects of memory; incapacity of judgment; and every degree of stupor up to complete coma.

3. Of the function of *voluntary motion* there are also various kinds and gradations of derangement: twitchings of the muscles; tremors of the limbs; rigidity from spasm; irregular and involuntary jactitation; convulsions; muscular debility; palsy.

Now, as I stated before, there is, and there can be no physical exploration of the living brain. We are limited, therefore, in studying its diseases, to the rational symptoms. It becomes our task to interpret the import of the multiform disturbances of function just enumerated, in every case in which more or fewer of them appear; and when you are told that these symptoms are apt to present themselves in almost every conceivable order and combination, and, moreover, that many of them may be sympathetic of diseases of other parts than the brain, you will scarcely need to be further informed, that the language they speak is often very hard to construe; that we frequently fail to reach and discover, by these outward signals, the inward things they denote.

I am about to consider, in the first place, some of the *inflammatory* affections of the brain, and some which may easily be mistaken for inflammatory affections; and I warn you beforehand, that, in respect to exactness of diagnosis, we are sadly barren of certainties in these matters. Hints, sketches, approximations, are nearly all that I can promise concerning not a few of the many diseased conditions to which the brain and its appendages are obnoxious.

In the brain, as in other composite organs, inflammation may be general or partial. It may attack certain tissues only: it may be seated in the substance of the cerebral mass; or in the membranes that envelope it.

I need not tell any of you that the membranes which invest the brain are three in number; the fibrous dura mater, the serous arachnoid, and the pia mater, which is composed of blood-vessels held together by a web of areolar tissue.

Speaking generally, inflammation of the cerebral substance alone, is perhaps more common than inflammation of the investing membranes alone. The central parts of the nervous mass may and do suffer inflammation, while the membranes escape. But it seems to me scarcely possible that inflammation of the pia mater should take place without implicating also the surface of the convolutions with which it has so close a relation, and a vascular connexion so intimate.

Again, with respect to the membranes themselves, the dura mater may be inflamed while the pia mater remains unaffected. I believe also that the arachnoid may suffer inflammation, and leave the subjacent pia mater untouched. Whether the arachnoid ever escapes participating in the inflammation of the dura mater on the one side, or of the pia mater on the other, is to be doubted.

Can we separate and distinguish these several inflammations by assigning to each its proper external phenomena? Seldom; scarcely ever. Doubtless each has its peculiar symptoms; and if inflammation were often strictly limited to the one membrane or to the other, and if the course and events of the inflammation did not modify the condition of the brain itself, by causing variations of pressure, or by affecting the circulation of blood through it, then we might expect greater uniformity, and might hope by careful and repeated observation to seize upon the desired distinctions. But this simplicity is not exhibited by the inflammatory affections of the parts within the cranium. Inflammation commencing in one membrane is apt to spread readily and rapidly to the rest, and to the cerebral substance; and the complication of diseased conditions coexisting within the skull at the same time, throws confusion over the whole subject. This uncertainty of exact diagnosis is however of the less consequence, inasmuch as when we have learned that inflammation is going on in any part of the encephalon, we have learned enough to direct us as to the general plan of treatment to be adopted.

After all, certain symptoms do present themselves more frequently when one part is inflamed, and certain other symptoms more frequently when another part is inflamed; and it will be proper and convenient to contemplate certain forms of meningeal inflammation separately.

Let us, first, then, consider inflammation as it is confined, occasionally, to the *dura mater* — or to the *dura mater* and *arachnoid*.

This very rarely happens as an idiopathic or spontaneous disease; but it is not at all uncommon as a result of external injury. And we may advantageously trace its ordinary phenomena and consequences, by attending to these instances of traumatic inflammation of the *dura mater*. They were excellently well described, many years ago, by Mr. Pott. A man receives a blow on the head; the blow stuns him perhaps

at the time, but he presently recovers himself, and remains for a certain period, apparently in perfect health. But after some days he begins to complain; he has pain of the head, is restless, cannot sleep, has a frequent and hard pulse, a hot and dry skin, his countenance becomes flushed, his eyes are red and ferrety; rigors, nausea, and vomiting supervene; and, towards the end, delirium, convulsions, or coma. Meanwhile the part which was struck becomes puffy, tumid, and somewhat tender; and if this tumid portion of the scalp be cut through, the pericranium beneath it is found to be separated from the bone; moreover, the bone itself is observed to be altered in colour, whiter and drier than the healthy bone; and if a piece of this bone be removed, it is also seen that the dura mater on the other side of it is detached from the cranium, and sometimes smeared with lymph or puriform matter. This is a form of disease very often met with by the surgeon. I have watched, with much interest, several such cases under the care of my hospital colleagues. One or two of them I will briefly describe.

In the year 1833, during Christmas time, the coachman of a lady living in my neighbourhood fell, being intoxicated, into a cellar or area, struck in his fall one side of his head, and tore up the scalp over a considerable space. He was carried to the hospital, where the loose flap of integuments was cleansed and replaced. After some days erysipelas came on, and then a much larger portion of the scalp sloughed away, so that the bone was laid bare to a frightful extent, and looked, at a little distance, as he sat up in bed, like the tonsure of a monk. Nevertheless the man seemed wonderfully free from suffering or distress: his pulse, indeed, was frequent, but it was said to be so during health. His intellect was clear, and he had *no* head symptoms; or rather no brain symptoms.

In the early part of February, 1834, he had a shivering fit, which was followed by convulsions of the right side of the body, and subsequently by paralysis of the right arm and leg, and by stupor, from which he could easily be roused. He would put out his tongue when desired to do so; but to every question he answered "yes." A portion of the left parietal bone was evidently dead. Here the trephine was applied; and a piece of bone being removed, the dura mater was exposed. It looked as if it also had lost its vitality. Some pus lay upon it. No relief followed the operation.

On the 10th of February fluctuation was detected beneath the dura mater, which was then slit open. About three drachms of puriform fluid escaped. The patient died soon afterwards, having had no active delirium throughout.

The surface of the dura mater was found to be nearly of its natural appearance, except where the trepanning had been performed. At that spot it was dry and sloughy. Over the whole of the anterior and lateral surface of the left hemisphere there lay, upon the arachnoid, a thick coating of coagulable lymph, smeared with pus: this extended down the posterior part of the hemisphere also, nearly to its base. There was no other morbid appearance; no fluid in the pia mater, nor in the ventricles. The substance of the brain was everywhere perfectly sound and healthy: it was divided in all directions in search of an abscess, but nothing unnatural could be detected.

Another man came to the hospital to have a small incised wound of the scalp looked at. The injury appeared to be trivial; the cut was dressed, and the man made an out-patient. A few days afterwards he came again, incompletely paralytic on one side of his body. I saw this man's skull trepanned; he was perfectly calm and collected: that part of the dura mater which corresponded to the wound was found to be inflamed: and there was pus diffused over the arachnoid covering the cerebral convolutions on the same side. He sank quietly into a state of coma, and so died. Not the slightest incoherence or delirium had been manifested, there had been no convulsions, nor was there any other morbid appearance within the cranium.

I mention these cases to show you the grounds of my own opinion that inflammation, beginning in the fibrous membrane, *may* affect the arachnoid, without necessarily extending to the pia mater; just as inflammation *may* overspread the pleura, or the pericardium, without touching the lung or heart which those serous membranes respectively clothe. Here no sensible traces of inflammation were discovered, deeper than the free surface of the arachnoid; and there had been no disturbance, till towards the end, of the proper functions of the brain. I conclude that the disease did not pass beyond the serous membrane; for I can scarcely conceive inflammation of the

pia mater to exist without involving, in some degree, the surface of the brain; nor inflammation of the surface of the brain to exist without some manifest derangement of the cerebral functions. In the instances that I have been relating, the final stupor and palsy may reasonably be ascribed to *pressure* resulting from the *events* of the inflammation of the arachnoid; from the effused pus and lymph.

Inflammation of the dura mater is very rare as a simple and idiopathic affection. Dr. Abererombie relates one instance of it, as the only one he had seen; and even that was not a pure case of inflammation of the dura mater. There was pus upon that membrane, which adhered to the cranium over a space as big as a crown-piece, and at that spot was ulcerated. But there was also found an adventitious membrane *beneath the arachnoid* where it covers the brain.

Speaking generally, this complaint is marked by pain of the head, by fever, and by rigors which intermit; and so regular sometimes are the intermissions, that the practitioner may be tempted to believe that he has got an aguish patient, and to administer bark. The intellectual faculties, especially at the outset of the disease, are but little affected; which is just what we might expect. The dura mater and the arachnoid lying apart from the sensorium, their inflammation can have no other than an indirect influence upon its functions.

Although inflammation of the dura mater is very uncommon as an idiopathic or primary disorder, we very frequently meet with it as a secondary affection; and then there are few diseases more surely fatal, or less within the reach of remedies. It is as a consequence of what is called *otitis*, that physicians are chiefly accustomed to encounter inflammation of the dura mater. It results from disease of the internal ear, and of the petrous portion of the temporal bone. Sometimes acute inflammation arises within the tympanum, when there has been no previous disease: the patient has severe head-ache, and ear-ache; at length a gush of matter comes from the external meatus, but the pain does not, as it usually does in such cases, cease; it continues, or even increases in intensity: the patient begins to shiver; he becomes dull and drowsy; slight delirium perhaps occurs; and by degrees he sinks into stupor. In some instances no pus issues externally. More commonly symptoms of the same kind supervene upon a *chronic* discharge of purulent matter from the ear. It is scarcely possible to sketch an accurate general picture of this insidious, but most dangerous complaint. Next to seeing and watching actual cases of it, the best way of becoming acquainted with its phenomena is by attending to recorded instances. I will bring before you, therefore, some examples of inflammation of the dura mater, occurring in connexion with disease of the interior of the organ of hearing.

A youth, sixteen years old, applied to the late Dr. Powell (who has related the case in the fifth volume of the *Transactions of the College of Physicians*) on account of an eruption, with an acrid discharge, behind the right ear. He had become deaf five years before, after scarlet fever, but no discharge took place at that time from the ear. In the following year, however, he had the measles, and then an abscess formed in the right ear; and after giving him much pain, it burst. He had again suffered, three days before Dr. Powell first saw him, a sudden attack of very severe pain in the same ear. The pain quite deprived him of rest: but he had no fever, nor delirium, nor coma. He slept, indeed, a great deal, but that was the effect of opiates, which he took to relieve the pain. This symptom was quieted by the opium; but it always returned with severity if the medicine were suspended. A fetid discharge came from the ear. On the tenth day of this attack, after a most violent paroxysm of pain, his strength rapidly declined, and he died.

"When the head was examined, the structure of the dura mater was healthy and natural, but beneath this membrane the whole superior surface of the right hemisphere was covered with a layer of coagulable lymph and pus. The vessels of the substance of the brain were not more numerous or loaded than usual, and the brain itself was healthy in every part. In the base of the skull the dura mater adhered to the bone, except at one part, of about half an inch diameter, just over the petrous portion of the temporal bone, where it was black and sloughy. The subjacent portion of the bone itself was carious, black, and crumbling; and contained fetid pus."

In this case, you will observe, there was no symptom to mark the extensive mischief within the head, except the *pain*: the pulse never exceeded 72; the skin was warm and moist; there was neither fever, nor delirium, nor convulsion, nor coma.

A girl, aged nine, (I take this case from Dr. Abercrombie, whose volume on the diseases of the brain is full of practically instructive *examples*,) had been liable to attacks of suppuration of the ear, which were usually preceded by severe pain, and some fever. She suffered one of these attacks in the left ear, in July, 1810. Upon the discharge of matter from the ear she did not obtain ease, as she had done on former occasions; but continued to be affected with pain, which extended over the forehead. When Dr. Abercrombie saw her, he found that, besides the pain, she had some vomiting, and impatience of light. Her look was oppressed; the pulse 84. Blood-letting, purging, blistering, and mercury, were employed without relief. Two days afterwards there was a slight and transient delirium, a degree of stupor, and slight convulsions. She lay constantly with both her hands pressed upon her forehead, and moaning from pain, of which there had not been the least alleviation. On the fifth day from the commencement of the discharge, she continued sensible, and died suddenly in the afternoon, without either squinting, blindness, or coma, the pulse having been always under 90. A considerable quantity of colourless fluid was found in the ventricles of the brain, which, in other respects, was healthy. In the left lobe of the *cerebellum* there was an abscess of considerable extent, containing purulent matter of intolerable fœtor. The dura mater, where it covered this part of the cerebellum, was thickened and spongy, and the bone corresponding to this portion was soft, and slightly carious on its inner surface; but there was no communication with the cavity of the ear.

Here again pain was the most prominent symptom, and probably resulted from the partial inflammation of the dura mater. It is interesting to mark these two points:—that the disease in the bone imparted disease to the dura mater, although no passage was opened from the tympanum; and that this inflammatory state of the external membrane of the brain led (apparently) to deep-seated suppuration in the cerebellum; the parts lying between the abscess and the dura mater escaping.

This last, and somewhat singular circumstance, might have been owing (so at least I conjecture), to the extension of the inflammation from the suppurating ear to some of the veins of the skull; and the consequent formation in the cerebellum of one of those secondary abscesses so commonly noticed in uncircumscribed phlebitis. Two very remarkable instances of diffused inflammation of veins, and of its terrible effects, occurring in connexion with purulent otorrhœa, have fallen under my own observation: one of them in private practice, the other in the hospital. As I am not aware that such consequences as supervened in these cases upon otitis, have received much attention, I will briefly describe them.

The first of these two patients was a boy, eleven years old, whom I attended with Dr. MacIntyre and Mr. Arnott. He had had a discharge of offensive purulent matter from the ear since the time when, four years before, he had gone through scarlet fever. In August, 1833, he went, for a walk, into Kensington Gardens, and there lay down, and slept upon the damp grass. The next day he was attacked with headache, shivering, and fever. Strong rigors, followed by heat and perspiration, occurred very regularly for two or three days in succession; suggesting the suspicion that his complaint might be ague: but then pain and swelling of some of the joints came on, and were, at first, thought to be rheumatic. However, the true and alarming nature of the case soon became apparent. Abscesses formed in and about the affected joints; and one of these fluctuating swellings was opened, and a considerable quantity of foul, grumous, dark-coloured matter let out. After about a fortnight the child sunk under the continued irritation of the disease. The hip-joint presented a frightful specimen of disorganization; it was full of unhealthy sanious pus, the ligamentum teres was destroyed, the articular cartilages were gone, and matter had burrowed extensively among the surrounding muscles. The knee and ankle joints of the same limb were in a similar state. It is curious that the destructive disease of the joints was limited to those of the right lower extremity, while the primary suppuration was in the left ear. Unfortunately the head was not examined; but that the fatal disorder had penetrated from the ear to the dura mater, I entertain no doubt: in all probability the inflammation had involved the veins or sinuses of the head.

The second case had many points of similarity with this.

William Marriott, aged 19, was admitted under my care into the Middlesex Hospital on the 18th of October, 1834, having pain and tumefaction of the right shoulder,

wrist, and foot, with redness of the latter. He complained also of headache, vertigo, drowsiness, and of an occasional feeling of stupor. His skin was hot and dry, his face flushed, his tongue furred, his pulse frequent (112), and his bowels were relaxed. A puriform discharge came from his right ear.

He had been suddenly seized, a week before, with sharp pain in that ear, which lasted twenty-four hours, when the discharge commenced, and the pain was relieved. He then began also to have headache, which had never left him, and to be sometimes dizzy. Three days previously to his admission the rheumatism (as he supposed it to be) commenced in the foot. When this part was examined, the redness was found to be circumscribed, somewhat livid, and limited to the great toe. It had much the appearance of gout.

He soon began to be troubled with shivering fits, which recurred regularly every morning about the same hour, and were followed by burning heat of the skin, but no sweating. An abscess formed near the toe, and was opened by Mr. Mayo, and some healthy-looking pus evacuated. Next, a large fluctuating tumour near the shoulder was punctured, and three ounces of pus, mixed with blood, came out. After this incision the rigors ceased; but the abscesses continued open, and the discharge had an offensive smell. On the 14th of November it was discovered that matter had collected in the left hip: this also was emptied by puncture. On the 1st of December, a very large quantity, not less than three pints, of unhealthy and grumous pus, was let out from a vast abscess which had formed in the loins: and pus was noticed in his stools. The discharge from the shoulder came at last to resemble the lees of port wine.

During all this while the patient remained feverish, with a dry parched tongue, and a rapid and feeble pulse. The diarrhœa continued, more or less, throughout. For some time before his death, which happened about the middle of the month of December, the left leg and thigh had been much enlarged by œdema.

I was not able to be present at the inspection of the body; and I have to regret that in the report which I received of it, the condition of the brain, of its membranes, and of its veins, was not noted.

The right shoulder-joint was extensively diseased; the cartilages were destroyed by ulceration over a considerable space. Those of the left hip were entire, but the synovial cavity was full of foul matter. The joint of the great toe was implicated also in the abscess which had formed there. The femoral vein, on the left side, was plugged up throughout its whole extent, by a coagulum, which was firm and of a red-dish-brown colour at the upper part of the vessel, loose and darker towards the ham. The saphena was pervious; the iliac was free from disease.

The lungs had undergone partial disorganization. Several distinct portions of the pulmonary tissue were nearly solid, while the tissue immediately around them was crepitant and healthy. From these small solidified portions, purulent matter could be made to ooze by gentle pressure.

The mastoid cells of the right temporal bone were filled with pus, and there was a slit-like opening in the membrana tympani. The small bones of the ear were sound.

I much lament that in these instances, the direct link of connexion between the disease of the ear and the disorganization of the joints was not demonstrated: for seeing (they say) is believing. Yet the pain of the ear, the discharge of pus from the external meatus, the subsequent pain of the head, coming on with fever and rigors, and followed after a short interval by destructive suppuration in several distant parts, and, in the last case, the actual femoral phlebitis; these circumstances form a chain of presumptive evidence, amounting, in my judgment, to moral certainty, that the fatal mischief, in each case, found entrance through "the porches of the ear;" and that the dura mater underwent inflammation. The same evidence is scarcely less affirmative of the complication of cerebral phlebitis. Perhaps the veins of the diploë, which in the cranial bones are of considerable magnitude, were involved in the inflammatory mischief; perhaps the large sinuses of the brain. The close vicinity of the lateral sinus to the diseased bone, and its formation by a duplication of the dura mater, would seem to render such a complication highly probable.

These views, which were brought forward in my first course of Lectures here, in 1836, have been confirmed by the publication more recently (1841), in the *Medical Gazette*, by Dr. Bruce of Liverpool, of two cases witnessed by himself, of "Phlebitis

of the cerebral sinuses as a result of purulent otorrhœa." He refers to several other instances of the same kind recorded by different authors. This combination of disease is doubtless more common than had been heretofore supposed: and the important pathological considerations connected with it will probably receive further illustration, now that the attention of the profession has been called to the subject by Dr. Bruce's paper.

Dr. Griffin has published, in the *Dublin Journal of Science*, two examples of otitis attended with symptoms exactly resembling those of intermittent fever. One of them is as follows:—A young man, previously healthy, was attacked with fits of shivering, accompanied by pain in the left side of the head. At first the paroxysms were rather irregular, but they soon assumed the form of *tertian ague*; coming on every other day, at about the same hour; the cold fit commencing at noon, and lasting about half an hour, followed by a hot stage of somewhat longer duration, and then a profuse sweat. In the intermissions the pain in the head was trifling: there was no thirst, nor heat of skin, but he did not sleep. A tumour formed over the mastoid process on the left side, and was opened, and a quantity of extremely offensive brownish pus sprang out with great force. This gave much relief. The bone was carious over a space as big as a shilling. After about ten days, the pain in the head and in the mastoid process became very severe; the patient had violent shivering fits many times in the day, great thirst, heat of skin, vomiting and delirium; his face was flushed, and his pulse hard; and he died within a few hours after the accession of these last symptoms.

The most remarkable features in this case were the similarity of the fits of shivering to the paroxysms of ague, their regular recurrence at periods of forty-eight hours, and the circumstance that they seemed to be checked, for some time, by the treatment proper in ague; namely, the exhibition of bark. The occurrence of *quotidian* paroxysms of the same kind has been noticed in relating some of the previous cases.

I have related them to show you what different symptoms may result from inflammation of the dura mater; and to put you upon your guard against overlooking the cause from which such inflammation does frequently originate. The suppuration of the tympanum, and consequent disease of the bone, are more common in scrofulous persons than in others; and they are more apt to occur as a sequel of scarlet fever than in any other way. I conceive that the inflammation which affects the throat in that disorder, and which often constitutes all its danger, creeps along the eustachian tube into the interior of the ear. In strumous subjects the fire thus lighted smoulders on, or if it ever go out, is readily rekindled; that part of the temporal bone, in which the organ of hearing is principally lodged, becomes carious; the membrana tympani is perforated; the little bones of the ear come away; more or less deafness ensues: and from time to time, or habitually it may be, there is a discharge of pus from the external orifice. At length the inner surface of the bone participates in the disease; and then the inflammation is apt to be propagated to the dura mater, or to the lateral sinus, in the manner of which I have given you some instances. It is in the first onset of the inflammation in the ear that remedies are most likely to be efficient in preventing this catastrophe. Leeches applied early and repeatedly to the mastoid process, especially when that part becomes tender, as it often does in such cases, and counter-irritation afterwards, are the best means in our possession. If symptoms of acute inflammation within the head supervene, the complaint may demand more active treatment, which I shall describe when I have spoken of inflammation of the other membranes of the brain. After what has been said, it is unnecessary to point out to you that the prognosis in these cases is very unfavourable. But we are not to abandon them in despair. That inflammation of the dura mater may be recovered from, we know, by what happens in certain injuries of the head: and the following would seem to be an instance of recovery when the source of the mischief was situated in the ear. A young lady, after the usual symptoms in the head, lay for three or four days in a state of perfect coma, and her condition was thought utterly hopeless. Her medical attendants continued to visit her as a matter of form; and one day they were agreeably surprised to find her sitting up, and free from complaint: a copious discharge of matter had taken place from the ear, with immediate relief: and she *continued* in good health.—(ABERCROMBIE.) We cannot be sure in such a case that the matter came from the brain; but the symptoms made

that supposition exceedingly probable. The case shows clearly one of two things; either that pus may thus escape from the skull, and the patient get well; or that pus shut up in the cavity of the tympanum, or in the mastoid cells, may produce the urgent symptoms that are known to result from cerebral pressure.¹

[The frequency with which inflammation of the dura mater supervenes upon otitis, especially in children, should be kept constantly in mind; for it is only by a prompt, active, and judicious treatment, whilst the disease is confined to the internal ear, that we can have any hopes of saving the patient—when inflammation has extended to the membranes of the brain, the termination is very generally fatal.—C.]

Cases are recorded of analogous disease communicated from the carious *æthmoid* bone to the dura mater; the patients having had pain in the forehead and purulent discharge from the nose, and becoming at last forgetful and delirious, and dying in a state of coma. I have never met with an instance of this kind; nor of inflammation spreading inwards from the socket of the eye: but I make no doubt that each may occasionally happen.

That part of the dura mater which is reflected over the inside of the skull may be regarded as its internal periosteum. Like the periosteum of the tibia, of the clavicles, of the bones composing the sternum, like the pericranium itself, this membrane is liable to a specific inflammation, one of the secondary effects of the poison of syphilis. In this form of disease you will often find the outer surface of the cranium painful, tender, knobby, embossed with smooth, round projections of considerable size: and there is good ground for believing that, under similar circumstances of contamination, similar prominences arise from its inner surface also. There may be nodes on either side of these bones; within no less than without. And such internal nodes, by the pressure, or by the irritation which they cause, may give rise to cerebral symptoms: pain in the head, convulsions, paralysis, coma. Whenever such symptoms present themselves, you must not allow this possible mode of their production to escape your attention and inquiry. This care is the more important because for chronic periostitis of syphilitic origin—if not for every form of chronic periostitis—we possess a specific remedy in the iodide of potassium. To have discovered this curious and valuable truth should suffice to immortalize the name of the late Dr. Robert Williams. The effect of the iodide in these cases, when given in appropriate doses, is marvellously prompt and sure. The nodes cease to be painful, and begin at once to recede until they disappear. I should not prescribe less at first than five grains, three times daily; and I should not hesitate to carry the dose to ten or even fifteen grains, if the circumstances of the case should seem to require that increase. I have never seen any of those ill consequences from full, but not excessive, doses of the iodide, which are apprehended by some physicians of eminence. The remedy should be continued for some time after the external nodes have departed, or the symptoms produced by the presumed internal nodes have ceased, and it should then be gradually withdrawn.

These four then—*idiopathic* inflammation of the dura mater—very rare; inflammation of the dura mater by extension of disease from the *æthmoid* bone, or from the orbit—also infrequent; inflammation of the dura mater by extension of disease from the petrous portion of the temporal bone—very common; and syphilitic inflammation of the dura mater—also common enough: these four constitute the forms of inflammation of the outermost tunic of the brain which the physician may be called upon to treat. The inflammation is not always—nay, perhaps it is seldom, if ever—restricted to that tunic; but it begins there; and the essence of the disease is inflammation of the *dura mater*.

Acute arachnitis—by which I mean active and *uncombined* inflammation of the arachnoid membrane—is, I apprehend, a very uncommon disorder; although that term is of frequent occurrence in medical writings. I have shown you already that inflammation may pass from the fibrous dura mater to the serous membrane reflected over it; and thence (by what is sometimes called contiguous sympathy) to the oppo-

¹ This interesting subject—the connexion between affections of the ear and disease in the brain or its membranes—has now been clearly and completely set forth by Mr. Toynbee, in a series of Clinical Lectures, published in the *Medical Times and Gazette* for 1855.

site portion of the same membrane spread over the surface of the brain. So, likewise, inflammation may extend from the pia mater to the arachnoid. If simple arachnitis, of an acute kind, ever happen, it has not been my fortune to see or to recognise it; and I can tell you nothing about it. In truth, the authors who use the word arachnitis do not intend thereby to express unmixed inflammation of the arachnoid; but include under that term inflammation of the pia mater also. Some apply the name *meningitis* to that compound affection; and the only objection to this nomenclature is, that the dura mater is as much one of the meninges of the brain as either of the two others.

In the few remarks which I have to make upon *inflammation of the pia mater* (or, if you will, of the pia mater and arachnoid at once), I shall chiefly follow Dr. Abercrombie: because his observations are comparatively recent, and carefully made; because his veracity, and sobriety of judgment, and philosophical turn of mind, are well known; and because his cases (as regards this particular affection) are quite to the point, and his descriptions clear and concise.

But I must premise a word or two respecting the anatomical characters of the disease.

When the upper part of the skull, and the dura mater, have been removed, you may frequently see, on the surface of the exposed brain, what seems to be a thin layer of clear gelatinous substance: but this appearance is fallacious. Puncture here and there the transparent arachnoid, and a limpid fluid, like water, trickles out; and the jelly-like investment of the convolutions is gone. Now this thin serous liquid, thus collected in the meshes of the pia mater, may be the event of inflammation of that membrane: but it may also be produced, and it very often indeed is produced, by simple congestion and remora in the cerebral veins. Nay, a certain amount of serosity, in this situation, belongs to the condition of health. We cannot, therefore, with any certainty, infer, merely from seeing this serous effusion, that there has been inflammation: we judge of its import, by noting the co-existence, or the absence, of other traces of inflammation; and by the character of the symptoms that preceded death.

On the other hand, we may be sure that there has been inflammation of one or both of these tunics of the brain when we find *false membranes* between them; layers, *i. e.*, of coagulable lymph. In the effusion of this substance I conclude that the vessels of the pia mater play the main part; both because it is always, in such cases, excessively vascular, while the arachnoid is seldom found to be so in any remarkable degree, if at all: and also, because the false membrane commonly, though not always, sends down layers between those duplicatures of the pia mater which descend into the sulci formed by the convolutions; where, as you know, the arachnoid does not go. In fact, considering the arachnoid as the serous membrane of the brain, we should expect that, when inflamed, it would present the events or products of inflammation on its free surface; and we sometimes find them there; but this is very rare; and for my own part, I look upon those effusions which lie beneath the arachnoid, between it and the pia mater, as being furnished exclusively by the vessels of which the latter membrane is mainly composed.

Now, the inflammation of these membranes (taking them together) commences and declares itself, by no fixed or uniform symptoms. The most common and striking phenomenon is a sudden and long-continued paroxysm of *general convulsions*. Sometimes this is the first thing noticed. Sometimes it comes on after a few days of discomfort, slight headache, and vomiting. The convulsions recur, and at length end in coma. Sometimes, again, the first attack of convulsions is preceded by violent pain in the head, setting in quite suddenly, and attended with screaming. Considering, on the one hand, the intimate connexion between the pia mater and the grey matter of the convolutions, and, on the other, the presumed functions of that grey matter, we might expect that inflammation of the pia mater would soon be attended with some manifest derangement of the mental faculties. Accordingly, *delirium*, often violent and continued, is stated by most authors to accompany and denote inflammation of the membranes; and especially of the membranes where they invest the upper surface of the cerebral hemispheres. Yet I do not find that symptom mentioned in any of the various examples of meningitis recorded by Dr. Abercrombie. He does give cases, indeed, in which there *was much* delirium; but they were not

cases of meningitis of any kind. He relates them as instances "of a very dangerous modification of the disease, which shows only increased vascularity." I venture with great humility to question or criticise any opinion of Dr. Abercrombie's: but I entertain no doubt about the nature of the cases which he so describes; and I hope to convince you by-and-by that they are not examples of inflammation at all. They neither show the anatomical characters of inflammation, nor yield to the remedies of inflammation. Excluding these cases, I do not find delirium specified as a symptom of uncombined meningitis. I shall abridge one or two of the well-marked examples of the disease.

A girl, aged nine, woke suddenly in the middle of the night, screaming from violent headache, and exclaiming that some person had given her a blow on the head. For the next two days she complained of some, but not much pain in her forehead, and did not even remain constantly in bed: no alarm was felt about her. On the third day she was seized with violent and long-continued convulsions, and immediately after the convulsions she fell into a state of deep coma: she remained in this state, with a natural pulse, till she died on the sixth day of the disease.

When the dura mater had been removed, the other membranes appeared highly vascular, except where this appearance was concealed by a layer of yellow adventitious membrane, spread out betwixt the arachnoid and the pia mater. This was distributed in irregular patches over various parts of the surface of the brain, but was most abundant on the upper part of the right hemisphere. It was as thick as a wafer, and in some places dipped down between the convolutions. A considerable quantity of it extended over the surface of the cerebellum also.

A child two years old was suddenly attacked one morning with severe and long-continued convulsions. The convulsions recurred many times; in the intervals she was dull and torpid, in a state of partial coma, with occasional starting, and a frequent and feeble pulse. On the fourth day she sank.

The surface of the brain, when the dura mater was removed, was covered in many places, betwixt the arachnoid and pia mater, by an adventitious membrane. It was chiefly found above the openings between the convolutions, and in some places appeared to descend a little way between them. The arachnoid when detached seemed to be healthy; but the pia mater was in the highest state of vascularity throughout; and when the brain was cut vertically, the spaces between the convolutions were most strikingly marked by a bright line of vivid redness, produced by the inflamed membrane. There was no effusion into the ventricles, and no other morbid appearance.

In another example, the whole surface of the brain was covered by a continued stratum of yellow false membrane, lying between the arachnoid and pia mater, and in some parts following the course of the pia mater through the whole depth of the convolutions. The pia mater and arachnoid adhered together everywhere, very firmly, by means of it. Not a trace of it could be found either on the outer surface of the arachnoid, or the inner surface of the pia mater. The arachnoid itself, when separated, presented no unusual appearance, but the pia mater was everywhere excessively vascular. There was no serous effusion, and the brain and cerebellum were perfectly healthy.

Now in this dissection there was unequivocal evidence of acute and extensive inflammation of these membranes, or I should say of the pia mater; yet the symptoms had been very obscure. The child in whom the disease occurred was convalescent from a mild attack of scarlet fever. One evening he became very feverish, and complained of his belly. Three days afterwards he had frequent vomiting, followed by stupor, and some convulsive movements of his face and arms, and death took place four days and a half after the feverishness began. We learn from this case, that general and severe inflammation of the innermost membrane may exist, and prove fatal, without giving rise to any violent symptoms at all.

I must trouble you with one more history, because it affords another example of what I have mentioned as being rare; viz., the effusion of the products of inflammatory action upon the outer surface of the arachnoid,—marking therefore very distinctly the inflammation of that membrane. It was evidently combined, however, with inflammation of the *pia mater* also. A child, eight months old, died after more than three weeks' illness; which began with fever, restlessness, and quick breathing; afterwards there were frequent convulsive affections, with much oppression; and at

last severe convulsions, squinting, and coma. At an early period of the complaint, a remarkable prominence of the anterior fontanelle was noticed; in the second week this *increased* considerably; and in the third week it was elevated into a distinct circumscribed tumour, which was soft and fluctuating, and pressure upon it occasioned convulsions. It was opened by a small puncture, and discharged at first some purulent matter, and then bloody serum. No change took place in the symptoms, and the child died four days afterwards.

A deposit of thick flocculent matter mixed with pus was found covering the surface of the brain to a considerable extent, and lying *upon* the free surface of the arachnoid. There was a similar deposition also between the arachnoid and the pia mater, and considerable effusion into the ventricles.

If the sketches I have been giving you afford a true outline of the phenomena which attend acute inflammation of the pia mater, or of the pia mater and arachnoid jointly, what, you may naturally ask, is the nature of those cases in which there is high excitement, and much fever, and great delirium, and which are sometimes spoken of as *phrenitis* or as brain fever? Why these *are* instances of acute inflammation of the whole contents of the cranium; of the brain and its membranes; of the encephalon in short; and, therefore, the disease has been called, not improperly, *encephalitis*. Of this formidable malady I shall give you some account to-morrow.

FIG. 23.

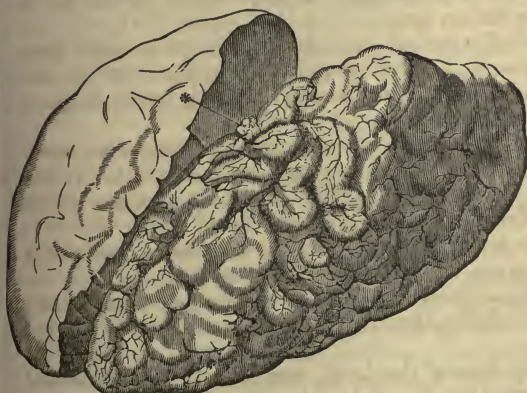


FIG. 24.

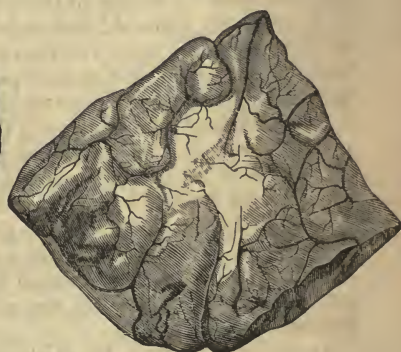


FIG. 23. — Subarachnoid effusion on the upper surface of the anterior lobe, causing an apparent obliteration of the interstices between the convolutions, and accompanied by increased vascularity.

* Enlarged Pacchionian bodies.

FIG. 24. — Portion of upper cerebral hemisphere of a young woman, aged 27, with purulent effusion under the arachnoid: there were two yellow symmetrical patches, one on each parietal surface, concealing the subjacent convolutions.

LECTURE XXIII.

Acute and general Inflammation of the Encephalon. Period of Excitement. Modes in which the disease may commence. Period of Collapse. Treatment. Delirium tremens.

ACUTE inflammation does sometimes appear to invade at once the whole of the parts that are lodged within the skull; or, beginning in one part, it extends rapidly to all the rest. As the contents of the cranium are called, collectively, the encephalon, so the disorder which I am about to consider has been named *encephalitis*. It is an

uncouth appellation, but it will serve its purpose. Cullen, and many others, apply the term *phrenitis* to the same disease. You may choose between these names, taking care to remember what they signify. The malady is sometimes described as inflammation of the *membranes* of the brain. I believe this to have arisen from the circumstance that the effects of the inflammation which become visible after death, are often more striking and obvious on the surface of the brain, or in its ventricles, than in the cerebral substance itself. An *abscess* in the nervous mass can scarcely be overlooked: a *softening* of the cerebral pulp may escape the notice of a hasty or an inexpert observer: and those changes of colour which sometimes denote increased vascularity of the same part, may very easily be passed over without attracting much attention.

Phrenitis, or encephalitis, or acute and general inflammation of the brain and its membranes, as it occurs in adults, presents two periods which are marked by different symptoms, and in most instances are very distinctly observable. In the first period what are called symptoms of excitement predominate: the functions of the organ are exaggerated as well as disordered; in the second period those symptoms appear which are comprised under the term collapse. Sometimes these two sets of symptoms, instead of following each other, are more or less mixed and confounded together. But the distinction is real, and requires to be attended to.

The symptoms that characterize the *period of excitement*, are pain of the head, often intense and deeply seated, or extending over a large part of it; a sense of constriction across the forehead; throbbing of the temporal arteries; flushing of the face; injection of the eyes, which have a wild and brilliant look; contraction of the pupils; preternatural sensibility to external impressions, amounting frequently to impatience of light, and of sound; violent delirium; want of sleep; paroxysms of general convulsion; a parched and dry skin; a frequent and hard pulse; a white tongue; thirst; nausea and vomiting; constipation of the bowels.

You are not to look for all these symptoms in every case; nor to conclude that your patient has not inflammation of the brain because the phenomena which I have been enumerating do not all present themselves, or do not take place in any regular order of succession.

In fact, we find, in actual practice, that encephalitis is apt to come on, to commence I mean, so far as symptoms are concerned, in three or four different ways.

Sometimes there is a sudden alteration of manner, and the patient, complaining probably of his head, becomes all at once and furiously delirious; and fever is lighted up. These are symptoms which cannot pass unnoticed, and which immediately direct one's attention to the head. They may, however, be fallacious, as we shall see by-and-by.

In other cases the first thing remarked is nausea or vomiting: and these symptoms may soon cease; or they may continue several days, and even sometimes throughout the whole course of the disease. Bitter fluids are brought up, yellow, or green, and evidently containing a good deal of bile: and whatever is introduced into the stomach, even a small quantity of the most simple drink, is immediately rejected. With this state of matters there is generally much constipation, and the bowels refuse to act except under the stimulus of strong purgatives.

It is important to attend to these symptoms; for occurring, as they usually do, with headache, they may easily deceive a person who is not previously aware of what they may portend. If the patient have not been previously subject to sick headaches, and if the epigastrium and abdomen be natural, not tender, nor distended, as they are apt to be when the stomach itself is in fault, and especially if the tongue be at the same time clean, we have the more reason to look narrowly into the case, and to suspect that some serious mischief, of which the nausea is a token, may be going on in the brain. I would observe, by the way, that where there is much vomiting of bile, persons are apt, both patients and their doctors, to blame the liver, to set down the disorder as bilious; but you ought to be aware, that whenever vomiting is often repeated, or long continued, bile is to be expected in the matters brought up. The action of the duodenum, as well as that of the stomach, is inverted; and the bile passes in the wrong direction. If you have ever suffered from sea-sickness, you must know that after the puking has gone on for a little while, bile is constantly voided.

Again, some cases of acute inflammation of the brain set in neither with sudden and great disturbance of the intellectual functions, nor with sickness and vomiting, but with a paroxysm of general convulsion, such as often ushers in an attack of meningitis. This symptom, according to Andral, is a much more certain sign of cerebral inflammation, than the occurrence of active delirium: and I quite agree with him in so thinking.

It is probable (but I speak conjecturally only) that this diversity of symptoms, marking the onset of encephalitis, may depend upon the part in which the inflammation begins: which is soon propagated from that part to the whole of the organ. I should suppose that when nausea and vomiting are the earliest symptoms, the inflammation has taken its point of departure in the cerebral pulp; in the substance of the brain: and that when the attack comes on with a sudden fit of convulsion, the inflammation has commenced in the pia mater or arachnoid. This is consonant with what we know of inflammation of those parts, when they are separately affected. Again, it seems to me presumable that the cases which are characterized by early and fierce delirium are cases in which the inflammatory action has invaded the superficial parts of the cerebral hemispheres, the grey portions of the convolutions. I say I offer these as conjectures of my own: what it is of importance for you to remember is, that inflammation of the brain does commence in the three several ways that I have been describing.

There are some cases, however, that cannot be brought within even this general rule. They begin in some irregular or obscure manner, or with some unusual phenomenon. Andral states that he has seen a few striking instances of inflammation of the brain, of which the first sign was a sudden loss of the power of speech: and Dr. Abercrombie relates a very remarkable case in which the same thing happened. I call it remarkable, both on account of the singular manner in which the disease first showed itself, and because it furnishes an example of encephalitis produced by direct exposure to intense heat of the sun—insolation; an event very uncommon in our climate. It occurred in the practice of a surgeon at Selkirk, in Scotland:—

“A young man, aged 16, bathed twice, on the 5th of June, 1818, in the river Tweed. After coming out the second time he lay down on the bank, and fell asleep without his hat, and with his head exposed to the direct beams of a hot sun. On awaking, he was speechless; but walked home, and seemed to be otherwise in good health. He was bled and purged, and the next day recovered his speech, but lost it again at intervals several times during the three or four following days. He was forgetful, and his look was dull and heavy: he made little complaint, but when closely questioned said he had a dull uneasiness at the back of his head. In a few days more he had squinting and double vision, and a very obstinate state of bowels, and his pulse was 60. After further bleeding the pulse rose to 86; but he sank gradually into coma, and died on the 30th.”

The substance of the brain in general was found highly vascular, and a very considerable extent of it was in a state of softening mixed with suppuration. The ventricles were distended with fluid, and the membranes in many places were much thickened. One very curious circumstance (affording perhaps some explanation of the readiness with which the inflammation was produced) was that the cranium was of very unequal thickness at its upper part. In one spot, as big as a sixpence, it was as thin as writing paper, and transparent.

However, the phenomena which I mentioned at first constitute the common and ordinary symptoms of acute inflammation of the brain and its membranes. They continue for a variable period; from twelve hours to two days, or more; and then they are succeeded by others, which characterize the second stage of the complaint, or the *period of collapse*, as it is called. These result, I apprehend, from the events and products of the inflammatory action; the violence of which is over, or abated. The patient ceases to complain of headache; instead of being excited or wildly delirious, he mutters indistinctly, and falls into a state of stupor, from which it is difficult, and at length impossible, to rouse him. His vision and hearing are no longer painfully acute, but dull, or perverted; strabismus and double vision are not uncommon; and the pupil from being contracted to the size of a pin's head, becomes first oscillating, then widely dilated, and ultimately motionless. The patient is not shaken, at this period, with violent convulsions; but twitchings of his muscles, and

startings of their tendons come on, and some of his limbs are agitated with tremors, or become powerless and palsied; the countenance is ghastly and cadaverous; cold sweats break out; the sphincters relax: at length the coma becomes profound, and life ceases.

The disease, when it proves fatal, as it too often does, mostly runs a rapid course. It may kill in as short a time as twenty-four or even twelve hours; or the patient may struggle on for two or three weeks. The morbid appearances met with in the dead body are very various. Serous or puriform effusion into the ventricles, or into the meshes of the pia mater; layers of coagulable lymph between that membrane and the arachnoid; softening of the cerebral substance, with pus infiltrated into the softened parts; or great vascularity, shown by a pink or purplish mottling of its cut surface, giving it a stained appearance.

Let us next consider the *treatment* required for this frightful disorder.

It is quite plain that for an organ so essential to life, and of such delicate organization as the brain, wherein changes so irreparable in their nature as many of those I have just enumerated, so readily take place under acute inflammation, we cannot hope to be of much service unless we see and treat the case at an early period. On this account it becomes exceedingly important to recognise the nature of the disease, at its very commencement; and, therefore, I have taken pains to point out to you the various forms which it may assume, while it is yet within the reach of remedial measures.

The principal of those measures are *blood-letting, purging, and the application of cold to the head*. All the particulars of the antiphlogistic regimen are to be rigidly observed; the patient should be kept as much as possible in silence, and in darkness, with his head high, and on a firm pillow. And the antiphlogistic remedies are to be employed with decision and energy.

With respect to *bleeding* I can only repeat what I have said before: the blood should be drawn in a full stream, and suffered to flow till some decided impression is made upon the pulse; or until syncope occurs, or is evidently at hand. After the patient has rallied a little, blood should be taken by cupping or leeches from the back of the neck, or the temples, or the mastoid processes; and these depletory measures must be repeated according to the violence or continuance of the symptoms which first demanded them.

The application of *cold* to the head is a remedy of great importance in this disease. The head must be first shaved: and the mere removal of the hair is sometimes followed by a manifest abatement of some of the most urgent symptoms; of the pain, for example, and of the delirium. In cases such as I am now supposing, it will not be enough to apply wetted cloths to the head: the application must be colder than the ordinary temperature of cold water; and it may be made colder by ice; and one way of effecting a permanent reduction of the superficial heat is to put some pounded ice with a little water into a thin and flexible bladder, and to lay it on the patient's head: there should not be too much ice, or its weight may be injurious. This is generally very grateful and pleasant to the feelings of the patient; and we often have the satisfaction of perceiving that, with the abatement of the external heat of the head, there is also an evident mitigation of the violent symptoms; the agitation and delirium are calmed, and the patient sleeps, or recovers his senses.

Another excellent and most powerful method of applying cold, is by pouring cold water in a slender stream upon the vertex of the head, until it produces some marked effect. Of course this, as well as all other strong measures, must be adopted with great caution, and its influence closely watched: I mean it is not to be left to the discretion, or indiscretion, of domestics and nurses. Dr. Abercrombie tells us that he has seen a strong man, submitted to the operation of this cold douche, "thrown in a very few minutes into a state approaching to asphyxia, who immediately before had been in the highest state of maniacal excitement, with morbid increase of strength, defeating every attempt of four or five men to restrain him." Of the effect of this measure in a somewhat different morbid condition, he gives an instance, which I will quote, because it shows, in the first place, the striking power of the remedy; and, secondly, the simple mode of applying it. A strong plethoric child, five years old, after being for one day feverish, oppressed, and restless, fell rather suddenly into a state of perfect coma. She had been in that state about an hour when Dr. Aber-

crombie saw her. She lay stretched on her back motionless, and completely insensible; *her face flushed and turgid*. She was raised into a sitting posture, and, a basin being held under her chin, a stream of cold water was directed against the crown of her head. In a few minutes, or rather seconds, she was completely recovered; and the next day was in her usual health.

This measure also is to be repeated, or not, according to the circumstances of the case.

Some persons recommend that a constant dripping of cold water upon the patient's shaven head should be kept up. This may easily enough be managed by means of a sponge and funnel placed a little above the head. Andral mentions his attending with another physician (M. Recamier) a young man who laboured under all the symptoms of acute inflammation of the brain. Cold water was made to drop slowly upon his head, and complete recovery took place, although no other active treatment of any kind was adopted.

This remedy, potent as it is, fails often of its purpose from the difficulty of ensuring its proper employment. The nurse sleeps; or, if awake, forgets or neglects the perpetual change and renewal of the wetted cloths: the bladder of ice is imperfectly adapted, or shifts its place as the restless patient moves his head: the dripping sponge wets the whole bed. To do the good of which it is capable—nay, not to do harm, by exciting reaction, when applied only at intervals—the cold must operate steadily, uniformly, and over a definite space. These objects seem to be attainable through an apparatus which has been devised by Dr. James Arnott, whereby cold (or, where it is wanted, heat) may be applied, with a suitable degree of pressure, or with scarcely any pressure, to any part of the body, for any required time. “A current of water of the appropriate temperature is made to flow through a thin waterproof cushion or bladder, in close contact with the body. The water runs into the cushion from a fountain reservoir raised above it, through a long flexible tube; and again, escaping from the cushion, it passes through another tube into the waste vessel. The cushion is of a size and form adapted to the part of the body on which the water is to act; and by a particular contrivance any pressure from its weight may be prevented. The part in contact with the cushion is kept moist, either by previously wetting the cushion, or by interposing a piece of wet lint, flannel, or other bibulous substance.”

If this apparatus—which I have not yet seen in action—prove easy manageable, it promises to be of essential service in many a sick room.

In strongly recommending this efficient remedy, cold, to your adoption, you will not understand me to advise that it should supersede the use of adequate blood-letting. It is to be employed as auxiliary to the lancet or the cupping-glass; not as a substitute for either.

The third remedy which I named, that is to say, *purging*, is also of great importance and efficacy. But it must be *hard* purging. There is a great tendency to obstinate constipation in most cases; and this must be overcome, and free and frequent evacuations from the bowels obtained: five grains of calomel and fifteen of jalap should be followed in three or four hours by a strong black dose; and after that I should give, in such cases, three or four grains of calomel every four hours, and repeat the black dose at least every morning, until the symptoms gave way. If the mercury thus exhibited affect the gums, so much the better; but we must not, in this disease, combine it with opium, to prevent its passing off by the bowels.

Dr. Abercrombie uses this strong language in reference to the value of purgative medicines in acute inflammation of the brain:—“In all the forms of the disease, active purging appears to be the remedy from which we find the most satisfactory results; and although blood-letting is never to be neglected in the earlier stages of the disease, my own experience is that more recoveries from head affections of the most alarming aspect take place under the use of very strong purging, than under any other mode of treatment. In most of these cases, indeed, full and repeated bleeding had been previously employed, but without any apparent effect in arresting the symptoms.” He has found the croton oil the most convenient medicine for this purpose.

Dr. Abercrombie is disposed to regard mercury as being useful in affections of the brain, chiefly in virtue of its purgative operation; and the opinions of a physician of his large experience and observing mind, must and ought to have great weight. But

I must not conceal from you my own persuasion that, in the early periods of acute inflammation of the encephalon (and it is of the early periods that I have hitherto been speaking), if the mercury come in a short time to produce its specific influence upon the gums, a great change for the better will often be perceived. Such is the result of my own observation. Recollect, however, that you are not to give calomel with the direct object of affecting the gums, but as part of the purgative plan, and you take the chance of its specific effect. You must not combine opium with it, for two reasons; first, you would thereby shut up the bowels, and deprive yourself of the use of one of your best weapons: and, secondly, you would incur the risk of augmenting and perplexing your patient's head-symptoms, and of puzzling yourself; since you would not be able to determine how much of the coma that ensued was owing to the progress of the disease, how much to your remedy.

When the second order of symptoms has arrived, those which are included under the general phrase of collapse, and which commonly result, I believe, rather from the products of the inflammation than from the inflammation itself; from softening, that is, and from pressure exerted by effused serum, or lymph; when symptoms of this order make their appearance, the time for doing good by active bleeding has generally gone by. If, however, blood-letting have not yet been employed, and especially if the pulse continue hard, whether blood has been already abstracted or not, it will be right to give the patient the chance of a trial of remedy. Of the propriety of doing so, take the following illustration:—"A girl, aged eleven, had violent headache and vomiting, with great obstinacy of the bowels: and these symptoms were followed by dilated pupils, and a degree of stupor bordering upon perfect coma; pulse 130. She had been ill five or six days; purgatives, blistering, and mercury to salivation, had been employed without benefit. One bleeding from the arm gave an immediate turn to this case, the headache was relieved, the pulse came down, the vomiting ceased, the bowels were freely acted upon by the medicines which they had formerly resisted, and in a few days she was quite well."—(ABERCROMBIE.) I must recite one other case—from among many which go to the same effect—to show the occasional influence of *hard purging*. "A young man who had had cough and dyspnoea, and been bled for these symptoms, appeared convalescent. One evening he became affected with headache, and some vomiting. About midnight, having got out of bed to go to stool, he fell down in a state of violent and general convulsion. The convulsion returned during the night six or seven times with such violence that one of the paroxysms continued without intermission for an hour. The pulse, during the night, varied from 60 to 120." (I should have mentioned before, this great and rapid fluctuation of the pulse in respect to its frequency, as being a very common circumstance and sign, in inflammatory affections of the brain.) "At first it was found impossible to bleed him, on account of the violence of the convulsions; but about seven in the morning a full bleeding was obtained, after which the convulsions ceased, except some slighter attacks during the day, which appeared to be arrested by pouring cold water over his head. The next day he was oppressed with occasional tremors of the limbs, and some vomiting, and he had one or two threatenings of convulsion. He took repeated doses of active purgatives with little effect; and on the following morning he appeared to be sinking into a state of perfect coma, with a pulse at 50. Croton oil was now given, which operated powerfully seven or eight times. He passed a good night; and the day afterwards was free from complaint."

Having this evidence of the separate efficacy of the three remedies—blood-letting, strong purgatives, and the local application of cold to the head—we have much encouragement to put them into combined operation in these very serious cases, especially when we have the opportunity of using them at an early period. Should the disorder happily yield to these measures, great care will long be required on your part, and great prudence on the part of the patient and his friends, lest the recent mischief should rekindle. A relapse is even more perilous than the first assault of the disease. Such prudence and care will consist chiefly in the avoidance and denial of all that might excite and disturb the brain; whether it be a premature return to animal food; or indiscreet and fatiguing interviews and conversations; or the too early resumption of the cares and concerns of business.

Are we to employ blisters in this disease? Not in the outset, during the period of excitement. They only add to the irritation, and make matters worse. And espe-

cially you should avoid putting them, as many are apt to do, upon the head itself, at that stage of the disease. We should not suppose, *a priori*, that they could then, and in that place, have any beneficial effect. They cannot divert the blood from the inflamed part; but they may attract it towards the encephalon. If they could be expected to do any good at all, it would be when they are placed upon the feet or legs. But this kind of revulsion is better accomplished by means of mustard poultices, or fomentations with hot water, which are often of much apparent service, in addition to the measures already spoken of. Experience confirms what reason teaches us to look for in this matter.

When, however, the patient had sunk into a state of coma, he has sometimes, in my experience, emerged from that condition after a cap of blistering plaster has been put upon his head. It is only when the violent symptoms of excitement have abated that I can venture to advise you to employ blisters: they may then be applied to the nape of the neck, or behind the ears, or to the head itself.

The symptoms which I enumerated as marking the period of collapse or sinking, are fearful symptoms; but the conditions on which they depend are not, necessarily, hopeless conditions. These symptoms do not always proceed from fatal disorganization of the brain, but sometimes (there is reason to believe) from simple exhaustion of the nervous power. And this is a point of critical importance. Patients apparently moribund are occasionally saved by the judicious administration of stimulants and restoratives; of ammonia, Hoffman's anodyne, beef-tea, and, it may be, of well-timed opiates. This plan of treatment you must therefore cautiously try, when an extreme degree of collapse occurs. If the structure of the brain be already seriously injured, and the disease irretrievably mortal, no harm can be done; while in doubtful cases, and when the symptoms result from mere depression of the vital powers, the patient may be rescued: and this chance in his favour must not be thrown away.

Do you ask whether there be any mode of discriminating these opposite conditions, one of which is within, and the other beyond, the range of possible recovery? I believe there is. If the tendency to *death by coma* be strong, the prospect is very discouraging: if, on the other hand, the symptoms that mark the mode of dying by *asthenia* predominate, you may hope to push the patient through. But to succeed, you must watch him hour by hour. Pallor, a feeble and flying pulse, extreme debility and tremors, coldness of the extremities, a want of power to respond to external impressions; these are alarming, but not absolutely desperate symptoms, especially if the mental faculties remain. Whereas profound stupor, partial palsy, profuse sweats, are of the worst omen; yet even these do not preclude the trial, together with blistering the head, of internal stimuli; and no other plan affords even a gleam of hope.

There is just one caution that I wish to mention before I leave the subject of acute encephalitis; and it applies to all cases of coma and insensibility, and especially when there is any paralysis mixed with the coma: it is, that you should daily ascertain that the bladder is emptied. Always make the attendants show you the urine that has been passed; and lay your hand upon the hypogastric region, and try whether there is any undue hardness and prominence there, produced by the distended bladder. I shall revert to this matter more particularly at some future time: and I content myself with merely suggesting its importance to you now, in all cases of head affection. If the patient *cannot* or *do not* empty his bladder, of course it must be emptied for him, by means of a catheter.

It would seem perhaps the most natural arrangement if I next proceeded to speak of such cerebral inflammations as are chronic, or partial. These forms of disease are more common, in adults, than acute and general encephalitis. I shall be obliged also to treat, separately, of inflammation of the brain as it is modified by its occurrence in strumous children,—of what is called acute hydrocephalus. But before I touch upon any of these, I am desirous to bring under your notice at once a very singular and extremely interesting complaint, which is not, I am persuaded, in its essential nature, inflammatory, but which may easily be mistaken, and has over and over again been mistaken, for acute inflammation of the brain and its membranes, with the consideration of which we have just been occupied. The mistake is the more serious, because the remedies that I have been recommending for encephalitis, and especially blood-

letting, not only are not required, but are in most cases positively injurious, in the disorder of which I am now about to speak; and which is best known under the appellation of *delirium tremens*. Nay, this affection of the nervous system may actually be brought on, in a predisposed subject, by the abstraction of blood. I go apparently out of my way in taking notice of this complaint now, but I do so that I may have the opportunity of contrasting it with encephalitis, while the phenomena of the latter disease are fresh in your memory. It certainly resembles it also in many respects: and it has been regarded as an inflammatory disorder by some excellent pathologists.

The symptoms which mark a decided attack of *delirium tremens*, and which have sometimes been found so equivocal, are very striking. You will be summoned to a man who is supposed to be mad, or to have brain fever. You find him with a red face, perhaps, and injected eyes, talking wildly and incessantly, fidgeting with his hands, affected often with tremors of the limbs, having a rapid pulse, and bathed in sweat. Now it is very natural that a person not on his guard should interpret these symptoms as indicating inflammation within the head. But if you look closely into the matter you will find in the state of the patient, and in his history, some things very peculiar. The delirium you will generally find to be, not a fierce or mischievous delirium, but a *busy* delirium: he does whatever you desire him to do, but he does it in a hurried manner, with a sort of unsuccessful anxiety to perform it properly. During the approach of the malady, while he is yet able to go about, he manifests great impatience of any interference, or advice, or assistance, in his ordinary duties, which he sets about in a bustling and blundering manner. His loquacity is extreme, and he refers to matters that are not present before him. He is not altogether inattentive to the objects and proceedings that are going on around him, but his mind wanders away to other subjects. There is an odd mixture of the real and the ideal in his thoughts and language. Sometimes he is very suspicious that those who are about him intend him some injury; or he fancies that he is surrounded by enemies. You will find also that he does not sleep; that he has not slept perhaps for several nights, but has been restless and rambling: and you will generally learn that he has been habitually intemperate, or subject to some great source of care, or anxiety, or excitement: and in many cases he has recently been somehow or other debarred from his customary stimulus. In addition to these points in his history, you will frequently be told that having been unwell, first he has been kept upon low diet, and then, as the delirium came on, he has been freely bled; and that he has been none the better, but commonly the worse, for the bleeding. When you gather such particulars as these from his friends (for upon his own statements you cannot place any reliance), and when you find the delirium to have the characters I have been attempting to describe, and especially when there has been obstinate watchfulness, and the tongue is moist, and the skin is sweating, you may be pretty certain that your patient is affected, not with inflammation of the brain, but with *delirium tremens*; and that if you bleed him further you will harm, instead of helping him.

But what are you to do under such a fearful state of things? Why the great indication is to procure sleep; and the remedy which, in nine instances out of ten, you will find successful, is opium. The beneficial effects of this drug, in tolerably favourable cases of *delirium tremens*, are really surprising. I will give you an example or two, which will be more instructive than any abstract description.

In the year 1831, I was requested by a most respectable practitioner in this town, to visit a patient of his whom he reported to have had phrenitis, for which he had been freely bled, cupped from the back of the neck, and purged; and who, he believed, was now rapidly sinking, and not likely to survive many hours. I found the patient, a middle-aged man, with a red face, ferrety eyes, a frequent pulse, bathed in perspiration, busy with his hands, which trembled a little, and talking much and incoherently. He was particularly anxious that his legs should not be scarified, told me he was willing to do anything I pleased, if I would not scarify his legs, nor let any one else scarify them. There was nothing the matter with his legs, nor had it entered anybody's head but his own, that they wanted scarifying. He had not slept for several nights. He had been intemperate, especially of late, drinking a good deal; and somewhat anxious about his affairs: he was a builder.

His former history was not very promising. He had brought up a good deal of

blood a few months before, and some years previously he had had jaundice; latterly he had been troubled with indigestion.

I saw him in the afternoon, and prescribed one-third of a grain of morphia: in the evening he was just in the same state. I then directed half a drachm of laudanum to be given immediately, and twenty drops every two hours afterwards, *till he slept*. I said to the gentleman who had called me to the case, that I thought it very likely our patient might be well the next day; he smiled, and shook his head. I was obliged to leave London early the next morning, for two or three days: on my return, I learned from the medical man that the patient took five doses of the laudanum, after which he fell asleep, and slept soundly, and for a long time, and then awoke (to his attendant's extreme surprise and satisfaction) sane and well.

I was asked by the apothecary of the Middlesex Hospital to see a publican in that neighbourhood. I found a large strong man between 30 and 40 years of age. He had been without sleep for several nights, somewhat incoherent, and (what is not usual in such cases) violent; threatening and striking those about him because they refused him access to strong drink. He was joint proprietor with another in a gin-shop, and for some time previously he had been a sot, and daily muddled with drink. He told me he was quite well. There was not much tremor. I found that the object of his partner and relations in sending for me was that I might sanction his removal to St. Luke's, for his strength made him altogether unmanageable, and his insane and extraordinary conduct was hurting the business of the house. I declined to take any part in consigning him to a mad-house, and recommended morphia. After one full dose he soon slept; and the next day he was quite rational, and comparatively well.

These are the broad outlines of delirium tremens: there are many other features wanted to complete the portrait of the disease; which I shall endeavour to paint at our next meeting.

LECTURE XXIV.

Delirium Tremens, concluded. Chronic Inflammation of the Brain. Softening, Suppuration, Abscess, Induration, Tumours in the Brain.

I DREW a rude outline yesterday, of that strange and interesting malady usually denominated *delirium tremens*. The disease is very common in this country; for its causes are in common and powerful operation. You will meet with it in every walk of life: and you will be almost sure to witness several examples of it during the course of every year, in any of our metropolitan hospitals. It is not a chronic or vague complaint, likely to be treated with *placebos*, or by waiting upon nature. Active measures are pretty certain to be adopted; and, in many cases, one plan of treatment, vigorously pursued, will hurry the patient to his grave; another plan will restore him to health with an almost magical celerity. It certainly bears a strong resemblance to that most formidable disease, inflammation of the brain and its membranes: but the great remedy for encephalitis acts like a poison in pure delirium tremens; and the drug, by the timely and careful administration of which we can often promise a speedy cure in delirium tremens, is one which we must carefully avoid, in the earlier treatment at least, of encephalitis. Accuracy of diagnosis, therefore, between these different disorders with similar outward signals, becomes of the very highest importance.

Delirium—tremens.—There is delirium always; and there is generally, but not always, tremor. The name is a good enough name, in my humble opinion; yet it has been found fault with, because the trembling is not in all cases present: and some have, therefore, christened it *delirium à potu*, or *delirium ebriositatis*. But these terms are open to just the same objection as the other; for though the disorder

is most commonly connected with intemperate habits, that is not always the case. One very curious fault has been discovered in the name: it is said that the delirium cannot *tremble*; and, therefore, that it is better to say, *delirium cum tremore*, or *tremefaciens*: and you would hardly suppose it, but there has been a sort of contention for the honour of thus mending the nomenclature of this disease. But they who object to *delirium tremens* appear to see no harm in *delirium ferox*: whereas it is just as incorrect to say *delirium* is fierce, as to say that it trembles: it is the patient who is furious, even as it is the patient who trembles; and all this dispute about a name is mere trifling. It matters not what we call a disease, so that the name conveys no erroneous theory as to its nature or treatment. No such source of error attaches itself to the term *delirium tremens*; and, therefore, if it be only to avoid the inconvenience of change, we will adhere to that term.

Recollect that the strong features of the complaint are sleeplessness; a busy, but not angry or violent delirium; constant chattering; a trembling of the hands, and an eager and fidgety employment of them. To these are added other symptoms which, though they are not so calculated to strike a looker-on, are of not less importance, inasmuch as they help to establish the diagnosis. The tongue is moist and creamy; the pulse, though frequent, is soft; the skin is perspiring, and most commonly the patient is drenched in sweat. The sweat is usually described as having an offensive or a peculiar smell: I cannot say that I have observed it to be so. The face also is said to be pale; but that, I know, is not always the case, and therefore this point cannot be relied upon as a distinguishing circumstance. In one of the instances which I related in the last lecture, the face was flushed, and the eyes red and ferrety.

Let me remind you, in a few words, of the peculiar characters of the delirium. If you question the patient about his disease, he answers quite to the purpose; describes, in an agitated manner, his feelings, puts out his tongue, and does whatever you bid him: but immediately afterwards he is wandering from the scene around him to some other that exists only in his imagination. Generally his thoughts appear to be distressful and anxious; he is giving orders that relate to his business to persons who are absent; or he is devising plans to escape from some imaginary enemy. He is haunted by ocular spectra; fancies that rats, mice, or other reptiles, are running over his bed; sees spiders crawling on the ceiling, or a horse's head thrust through the wall of his room. He addresses remarks to strangers, whom he erroneously believes to be present. He looks suspiciously behind the curtain, or under his pillow, and he is perpetually wanting to get out of bed; but he is readily induced to lie down again. It is very seldom that he meditates harm, either to himself or to others: there is rather a mixture of cowardice and dread with the delirium.

All the points that I have been mentioning require to be investigated in every case of this nature: and an inquiry into the previous history of the patient, into what the French call the commemorative symptoms, is equally important. In a large majority of instances you will find that he has been an habitual drunkard; and very frequently that from some cause or other this habitual stimulus has been diminished or taken away. Some accidental illness has befallen him, and he has been restricted to low diet, and, as a sailor would say, "his grog has been stopped." When, with symptoms such as I have described just now, you hear a history of this kind, you may be satisfied that the disease is not inflammation of the brain, but *delirium tremens*. I believe that habitual intoxication of any sort may lead to this disorder; but distilled spirits more surely than wine: wine more than beer. I make no doubt either, that what is alleged of the habitual use of opium, in preparing a person to suffer in the same way upon its being withheld, is quite true, although I have had but few opportunities of noticing such cases.

But the disease is not confined to drunkards, although it is so commonly connected with that pitiable vice, as to have been called *mania à potu*. You meet with it occasionally in men who have overstrained their nervous system by other modes of strong excitement. Long-continued mental anxiety, that state of mind in which gamblers and great speculators (who indeed are gamblers) are accustomed to live, may cause it; anything by which the mind is over-wrought. A well-informed medical man, of temperate habits, told me a few days ago that he was on the brink of *delirium tremens* in the year 1825. He had foolishly entangled himself in some of the speculations

which prevailed here like an epidemic at that period, and his mind was on the tenter-hooks of suspense and apprehension for some time. He could not sleep, and he found himself "everlastingly chattering." It comes on in the course of certain diseases; as sometimes, for example, in apoplexy: and it is a very common result of bodily injuries and accidents, and of surgical operations: or, I should rather say, that it often *follows* such diseases and casualties; for it is, even then, the consequence of the treatment and regimen to which the patients are subjected, rather than of the surgical or medical complaint. And it is certainly *more* apt to occur, under these circumstances, in old people; and in those who, being younger, are known to have been intemperate. So frequently does the delirium manifest itself upon the cessation of the accustomed spur, that the continually recurring stimulus has been regarded as the *predisposing*, and the privation of that stimulus the *exciting* cause of the affection. Sometimes, however, it comes on in men who are perpetually fuddled, even although they have not intermitted their usual indulgence in drink. We had a porter (an old soldier he had been) at the Middlesex Hospital, who was of great use to us as a subject to practise upon, and to show to the pupils. I never saw him so drunk as to be unable to perform his duty: but I cannot conscientiously say that I ever saw him sober. Every three or four months we were sure to have him in the wards with delirium tremens. Sometimes he fell into the hands of one physician, and sometimes of another; but in one of his attacks he slipped through our fingers. I am not certain that he was not nominally my patient on that last and fatal occasion: but assuredly he never exemplified the coming on of the disease from the adoption of more temperate habits. We often find that the malady shows itself immediately after an unusually severe debauch, which has disturbed the stomach and bowels, and left behind it a proportional degree of exhaustion and languor.

Without knowing why it should be so, my own experience would lead me to the belief that delirium tremens is very uncommon among women. The number of beds for females in the physicians' wards of the Middlesex Hospital is somewhat greater than for males. On the men's side of the house cases of delirium tremens are very frequent: whereas I scarcely remember any on the women's. Yet each sex is obnoxious to its main causes. The gin-shops of this town are said to draw a fearful crowd of votareesses. And we might expect that the more sensitive character of the female constitution would render them especially liable to this peculiar consequence of the abuse of alcohol. My experience, however, is such as I tell you. On the other hand, Dr. Roots thinks he has seen quite as many instances of delirium tremens attacking females as males. The result of M. Rayer's observation is more in accordance with my own. Of 176 patients seen by him, seven only (not one in twenty-five) were women. A still smaller ratio is recorded by Bang, ten in 456: less than one in forty-five. The disorder appears to be more common in the summer than in the winter months.

The peculiar nature of the complaint, and the proper method of treating it, were first brought into general notice in 1813 by a little work of Dr. Sutton's, of Greenwich. He saw a good deal of the diseases of the smugglers, and of the customers of the smugglers, who frequented the coast of Kent; and he was struck by the different event of this disorder in the hands of different practitioners, according as bleedings, or narcotics, were adopted. It is the same disease which Dr. Abercrombie speaks of as "a dangerous modification of meningitis, which shows only increased vascularity." Dr. Bright also includes it among his cases of "Arachnitis." Both these eminent physicians had learned, however, that the complaint requires a particular method of treatment. Of late years many essays and papers on the same malady have appeared in this country, in France, and in the United States, where the disorder is common. But even now it is not so well understood, throughout the profession, as it ought to be.

I apprized you, in the last lecture, that the great remedy in delirium tremens is *sleep*; and that our most powerful means of inducing sleep are to be found in opium. The opium must be given in full doses; and it must be fearlessly repeated if its desired effect do not soon follow. If the patients pass many nights without sleep, they will die. I have tried various forms of opium; and I am quite satisfied with morphia. Some persons, however, have not found it so successful as solid opium, or as the common tincture, laudanum. You may try the one or the other, or the one

after the other, if you please. No particular rules can be laid down that will suit all cases. After clearing out the bowels by a moderate purgative, you may give three grains of solid opium; and if the patient show no inclination to sleep after two or three hours have elapsed, you may begin to give one grain every hour till he does sleep. Or you may prescribe corresponding quantities of the acetate or muriate of morphia: or of laudanum: or of the black drop: or of Battley's sedative liquor. His room, meanwhile, should be kept dark and quiet. If he sleep for some time he will awake calmer and more sensible; perhaps perfectly so: and you must withhold the remedy, or continue it in smaller or less frequent doses, according to the circumstances of the case.

Dupuytren found opiate enemata of great efficacy in the cases of traumatic delirium which came under his care. That mode of administering the narcotic may properly be adopted, if there be any impediment to its reception or retention by the stomach.

Now sometimes this opiate treatment alone is quite enough: sometimes it is not. You will meet with patients who resist very large doses of the drug; but who presently sleep, or become composed, if you give some of their accustomed stimulus with it: "a hair (as the vulgar saying goes) of the dog that bit them:" if you put their opiate dose into a glass of gin, or a pint of porter. Nervous *exhaustion* goes along with and augments the nervous irritability. In such patients we commonly find the aspect pale and haggard, and the pulse small and weak. The disorder tends, then, to death by asthenia. You may obtain some clue to the particular cases which require this treatment, by examining into the previous condition of the digestive functions. If you learn that, notwithstanding the intemperate habits of the patient, his appetite for food has continued unimpaired, and his digestion sound, you will, I believe, generally find that good nourishing diet, strong broths, for example, and the opium, will suffice for the cure. But if the powers and natural sensations of the stomach have been injured and perverted, as is too often the fact, then a temporary recurrence to the habitual stimulus will frequently be necessary: and it is well to ascertain in such cases, what the stimulus has been, whether spirits, or beer, or wine, and to order it accordingly. Of course this is not to be continued after the patient has recovered from his delirium; but the stimulus under these circumstances must be cautiously withdrawn. When the stomach retains its power of digestion, the bad habit of drinking ought to be broken off at once: and if, after sleep, you can get the patient to eat a meal of beef-steaks, or of mutton-chops, it will always be right to advise it.

In hospital practice it sometimes becomes necessary to confine the patient to his bed by straps, or to muffle his limbs in a strait-waistcoat: but this is a most unfortunate necessity. Physical coercion, whether manual or mechanical, should never be resorted to, in delirium tremens, when by any means it can be avoided. The angry feeling and mental fret which it produces, and the exhausting bodily struggles to escape or resist the thralldom, are always highly injurious, and full of danger to the patient. A couple of strong and good-tempered attendants will not have much difficulty in persuading and managing the sick man, who is seldom either boisterous or obstinate: and if he be intractable by soft words, he will yield more patiently to their gentle restraint than to the force of manacles; while the appearance of coercion need not be continued a moment after his acquiescence.

There are some things which I find it necessary to mention, for the sake of recommending them. I know persons who in treating these cases always combine calomel with the opium. And they say that they cure their patients so; and I make no doubt that they do; neither can I doubt that the same success would generally have followed the same quantity of opium without the calomel. In *pure* cases of delirium tremens I advise you *not* to give calomel. I know no possible good it can answer: it is itself a source of great irritation to the nervous system in many persons: and if it come to affect the mouth, you inflict upon your patient a superfluous discomfort; and, I believe, in many cases, a downright injury. You will be told also of digitalis, as a specific remedy for the disease; or you may read of it: but do not be led away from the standard remedies which reason recommends, and large experience has sanctioned. Knowing what we do of the power of opium generally, and of its efficacy in this complaint in particular, I should consider myself guilty of a criminal trifling with human life if I made experiments with digitalis, upon the loose

reports of some one or two persons, of whose credit or information I knew nothing; and whose dicta had been transferred perhaps from some foreign journal to fill a vacant corner in one of our own. The combination of opium and antimony, which has been much praised by physicians of great judgment and experience, seems to me chiefly appropriate to certain modifications of the disease.

I have drawn the line between encephalitis and delirium tremens with sufficient clearness, because I have taken well-marked forms of each. But I am obliged to add that there are mixed cases, which are very puzzling when they occur, and exceedingly difficult to treat; and which require opiates on the one hand, and moderate antiphlogistic measures on the other. When the indications are uncertain, or equivocal, we must carefully weigh the different symptoms, and we must cautiously *try* the remedies. The circumstances that most distinguish the one form of the disease from the other are to be found in the *pulse*; which is hard and resisting in the earlier stages of inflammation of the encephalon, soft and compressible in delirium tremens: in the *tongue*; which is mostly parched and rough in the former, moist and creamy in the latter: in the *skin*; which is hot and dry in the one case, covered with sweat in the other: in the *countenance*; which is flushed in inflammation, and mostly (though not always) pale in delirium tremens: in the *tremors*; which are not common in the primary periods of inflammation of the brain: in the usual absence of *headache* in delirium tremens: and in the peculiar characters, which I need not recount, of the *delirium* in the two cases. If these symptoms contradict each other, as they sometimes will, you had better act on the *worst* supposition, and presume that there is inflammation, and employ antiphlogistic remedies: but you must not do so with a strong hand; you must use them cautiously, and watch their effects, and guide thereby your subsequent treatment. Take a small quantity of blood from the arm; observe whether it has the buffy coat; and note the condition of the patient afterwards. It is in these mixed or ambiguous cases that it will be proper to combine calomel or antimony with the opium. You will sometimes find a state resembling delirium tremens left after the subsidence of acute inflammation of the parts within the cranium, and requiring the treatment of delirium tremens.

The points of distinction just enumerated are obvious to the senses, and easy to note. Another, and probably a surer criterion than any or all of them, has lately been brought to light in some highly interesting researches of Dr. Bence Jones'; but, unfortunately, it is not self-evident, nor readily elicited. I allude to the contrast which Dr. Jones has shown to exist between the two diseases, in respect to the amount of earthy and alkaline phosphates excreted with the urine. In the severest cases of delirium tremens there is a marked diminution of these phosphates—in acute inflammation of the brain a considerable increase. Taking the average from three examples of each disease, the difference was in the proportion of 1 to 12. The extremes presented the extraordinary ratio of 1 to 225. Dr. Jones concludes that the "excess of phosphates may be regarded as resulting from inflammatory action going on in the brain, while the diminution of the same phosphates in delirium tremens must be considered as caused by the positive hinderance of that process of formation of phosphoric acid which in the healthy state is continually taking place."

I do not know that there is much good to be expected from counter-irritation in this disease. But after the more decided symptoms were gone by, I have sometimes thought that the recovery has been accelerated by the application of a blister to the nape of the neck.

Inflammation of the brain, and delirium tremens, are distinct diseases. Hence, in the mixed cases, of which I just now spoke, we may expect after death to find, and we often do find, unquestionable traces of inflammatory action within the skull. But pure delirium tremens frequently leaves behind it no morbid appearance whatever in the brain or its membranes. In other cases there is serous liquid collected in the interstices of the pia mater, or in the cerebral ventricles; and I have on several occasions seen the arachnoid thicker and less transparent than is natural, and sprinkled over with little spots or streaks of a milk-white colour. Changes of this kind we believe to be owing to chronic inflammation of the membrane. But, even in these cases, I see no reason for thinking that the fatal disorder had any connexion with the morbid state of the arachnoid. We meet continually with like appearances when there has been no delirium tremens; and we have delirium tremens without any such

appearances. The habitual abuse of ardent spirits leads to chronic inflammation in various parts and tissues of the body: in the blood-vessels, in the liver, in the kidneys, and in the arachnoid. We need not be surprised at finding that membrane thickened and partially opaque in the victims of delirium tremens; since they are chiefly men who have run a long course of intemperance. I believe that disease to bear the same relation, and no other, to the chronic arachnitis in such persons, as to the chronic hepatitis to which they are equally subject. There is but one morbid condition which, since my attention was first directed to it, I have found constant in persons dead of delirium tremens, and that is, a remarkably soft, pale, and flabby state of the muscular tissue of the heart. Mr. Solly tells us that "in all the cases which he has had the opportunity of examining after death, he has invariably found the hemispherical ganglion, or cortical substance, (of the brain,) pale and bloodless."

The chemist may be more likely to detect altered conditions in the brain, in these cases, than the anatomist. Dr. Percy has obtained alcohol from the brain of a person who died from excessive drinking; and from those of various animals which had been killed by that poison. These facts are interesting, but they do not help us much in our attempts to explain the phenomena of the disorder.

Cases such as I related in the last lecture, where violent symptoms are calmed at once, and the patient is rescued in a few hours from great apparent peril, make a strong impression upon those who witness them: and the practitioner gains amazing credit, and is spoken of to all their acquaintances as a wonderfully clever man. It is unfortunate that we are obliged to set off, against this advantage, a corresponding danger, when the disease ends ill, of being blamed without our deserving it. When these patients die (and they usually persist in their evil habits and die at last in one of the attacks of the disease,) when they so die, they are apt to die much in the same way as patients who are poisoned by opium; and if their friends are aware that we have been giving large and repeated doses of that drug, they sometimes have the charity to lay the death at our door; and you ought to be prepared for this: and I will conclude what I have to say upon the subject of delirium tremens by relating a case, in which I have no doubt that I suffered (though quite unjustly) under that kind of imputation.

Several years ago I was asked, one morning, by a general practitioner at the west end of the town, to see a patient with him: of whom he gave me this account. The man was about forty years old. He had been attacked some days before with sore throat, common cynanche tonsillaris. The tonsils and fauces were so much swelled that his deglutition was greatly impeded, and for four or five days he had scarcely been able to swallow anything. The night before I saw him he had become delirious, and then had been largely bled, and he was worse in the morning. His bowels had also been very much purged.

I found him propped up in his bed, with a coronet of leeches round his head. He was pale; there was no headache, nor affection of his breathing; his pulse was not very frequent, and it was quite soft and compressible. He was sweating profusely. He answered the few questions I put to him readily and pertinently, and then went talking on in a rambling manner about his business. He was a hackney-man or stable-keeper, in a large way. He said (I remember) that the boys were all ready to start, that there were two pair of horses going down the road, and that he must go and see after them; and much more on the same subject. His mind was busy about the execution of imaginary orders. He had not slept at all for some nights.

Upon my inquiring into his previous condition, his wife told me that without any turn for dissipation he had for some time been an habitual hard-drinker; that he had frequent dealings with the coachmen to the various families which he furnished with horses; and that he was obliged to drink something with each of them; so that every day he had many glasses of spirits, and a good deal of porter. She told me also that his mind had been anxious and uneasy; that the business was a large and harassing one; that he had embarked a considerable sum of money in it; and that it had not turned out so prosperously as he had expected.

Putting all these things together, there could be no doubt, either as to the character of the complaint, or as to the treatment proper to be adopted. Here was a man who had been living a life of continued mental and physical excitement. Suddenly the stimulus to which he had been accustomed was taken away; he could not swallow

even such nourishment as his case required or admitted. Then came on delirium—a symptom not belonging to the disease in his throat—and protracted watchfulness. He is largely bled, and profusely purged, and he gets worse instead of better under these remedies. At the same time his skin is moist and perspiring, and there is no hardness in his pulse.

I recommended that the leeches should be removed from his head; that he should take immediately (for he could swallow now) two grains of opium, and afterwards twenty drops of laudanum every two or three hours till he fell asleep.

Somewhat unluckily, his wife's brother—a very young man—was the apprentice or assistant of a surgeon in the neighbourhood of town, and he came in to see his relative. After hearing what I had said, he went home, and probably consulted his books, and then came back again with doubts whether the complaint really was delirium tremens after all. Whether in consequence of these doubts I cannot tell, but for some reason or other only one or two doses of the medicine were taken. I had offered to see the patient again in the evening, but his friends said they would send for me if he did not get better. They did not send. The patient did not sleep. At night, therefore, at ten o'clock, three grains of opium were administered. The result of this was, that he passed a quiet but a sleepless night. Perhaps (but I cannot be sure of that) if the opium had been persisted with, the case might have terminated otherwise. About eight o'clock the next morning I was summoned to him in a great hurry: when I got there he was dying, perfectly comatose, breathing stertorously, with blue lips and contracted pupils. He had appeared so much better at seven, that he was, for the first time, left alone for a quarter of an hour; and when they went back to him he was changed in the manner I have described.

The general practitioner with whom I had first seen the patient—a very sensible man—was much concerned at this issue of the case, and observed to me that doubtless our patient had been poisoned by the three grains of opium. I was able, however, to relieve his mind from this notion: and I have mentioned the case chiefly for the sake of guarding you against similar misgivings, under similar circumstances. The manner of dying was just such as opium will produce; but, then, death by coma is also frequently the termination of delirium tremens. Effusion at length is apt to take place into the ventricles, or into the meshes of the pia mater, and stupor comes on, and the patient sinks. But in this instance I was certain that his death had nothing to do with the opium he had taken, for this reason; that so long a space of time had elapsed—nine hours—between his taking the opium and the coming on of the comatose symptoms. Dr. Christison, in his elaborate and valuable work on Toxicology, states it as the result of extensive inquiry into this subject, that when opium has been swallowed in a poisonous dose, it almost always begins to act as a poison within an hour; that very rarely indeed has its specific operation been postponed much beyond the hour, except occasionally, when the person taking it was intoxicated at the time. In one remarkable instance a drunken man took two ounces of laudanum, and no material stupor followed for five hours. I guess that I incurred the reproach of recommending a fatal plan of treatment in the particular case I have now related; but I am quite satisfied that the opium was innocent of the patient's death, and I even think that his chance might have been much mended if the opiate, in smaller doses perhaps, had been steadily continued.

We may be content to bear occasionally these unfounded imputations, when we consider the other side of the account, and call to mind the far greater number of instances in which spontaneous recoveries are credited to us as cures; and the Doctor, like Belinda's Betty, is "praised for labours not his own."

[The account given by the author, of the character, phenomena, causes, and treatment of delirium tremens, is, upon the whole, so very judicious, that it would scarcely appear to demand any other comment, than one of general approval. But as there exists a very decided difference of opinion among American practitioners in regard to the proper management of the disease, a few words upon this point may not be improper.

Four different plans of treatment have been recommended, and the results of their extensive employment for a series of years, have been adduced, by their respective advocates, in evidence of the superior efficacy of each. One practitioner cures all or

nearly all his cases by repeated emetics, another, by the free exhibition of alcoholic drinks, and a third, by opiates in free doses, continued at short intervals, until sleep is procured — while a fourth considers that neither excitants proper nor opiates are necessary, but simply a state of tranquillity in a quiet and darkened chamber — with perhaps an emetic to unload the stomach in the commencement of the attack, and some gentle cathartic to keep the bowels open — and when the stomach will retain it, a light, nutritious, and easily digested diet.

The opiate practice is the one, in favour of the superior efficacy of which we have the most imposing weight of evidence — and it is unquestionably the one that will, in the majority of cases, when judiciously and cautiously managed, the most promptly and effectually remove the symptoms of the disease. That the opiate practice has been abused, we are perfectly aware. Under the supposition that opium to any extent that may be requisite to induce speedy sleep can be administered in delirium tremens with perfect safety, we have cause to fear that a state of coma has in more than one instance been induced from which the patient has never awoke. We have never been in the habit of administering large doses of opium, and have usually combined each dose with an equal quantity of camphor, and about half a grain of ipecacuanha. In young, robust, and plethoric subjects, we believe that the application of cups to the temples and nape of the neck, or even a moderate bleeding from the arm, is an important measure in the commencement of the attack, which should not be lost sight of.

That there are many cases of delirium tremens in which a perfect recovery may be effected without the administration of opium or of any stimulant is very certain — but our experience has taught us, that when the disease occurs in confirmed inebriates, with a broken-down constitution, and in whom there is almost complete destruction of the proper functions of the digestive organs, almost the only means by which it can be certainly and promptly arrested is opium administered in moderate doses at short intervals.

The treatment of delirium tremens by alcoholic drinks, while we can have no doubt of its very general efficacy, — is attended with an evil of too serious a character, to permit us to give to it, under any circumstances, our sanction. It cannot fail, we are persuaded, to confirm the patient in his intemperate habits — and thus render him liable to a renewal of the disease after a short interval. That it is not the only successful treatment we are convinced from ample experience. In the practice of our preceptor as well as in our own, which has extended now far beyond a quarter of a century, we have had sufficient opportunities for testing the value of the opiate practice in this disease, and have seldom been disappointed in its effects. We do not say that the patient will invariably recover under it. There are cases, in which, from the condition of the patient's system — the complication of the tremulent delirium with serious disease of the brain or other important organs, death is inevitable under any plan of treatment: we believe, however, that in the general run of cases, the success of a properly conducted opiate treatment will equal that of any other; while in the old, broken down drunkard, it, or the stimulant practice, is the only one upon which any dependence can be placed. Of the emetic treatment, as recommended by Dr. Klapp, we cannot, it is true, speak from experience; in the very few cases in which we have tried it, we were disappointed in its effects.

On the subject of the proper treatment of delirium tremens, the reader may consult with profit Dunglison's *Practice of Medicine*, 2d edition, vol. ii., page 274, and the able note by Dr. Gerhard in *Tweedie's Library*, American Edition, vol. ii., page 237. — C.]

I should wish to put you next in possession of what has been ascertained in respect to *partial* and to *chronic* inflammation of the brain, as these are met with in adults; for I must speak of some head affections of children separately. But I really do not know how to bring this part of the subject before you in a practical manner. If I were first to describe symptoms, and then to state what organic changes had been discovered after death preceded by them, I should have to tell you of different symptoms with the same morbid conditions, and of the same symptoms with different morbid conditions, in various individuals. I believe the best method, upon the whole, will be to describe the several morbid appearances which the brain is found to present;

and then to mention the symptoms that have *most commonly* been observed to occur in association with such morbid conditions. I must premise, however, that the whole subject is full of uncertainty and apparent irregularity. Doubtless, there *is* some constant and uniform connexion of cause and effect between the altered physical states of the brain, and the altered manifestation of its functions: but we have not yet been successful in our search after those settled relations, or we have but partial and imperfect glimpses of them.

One very remarkable condition of the brain has been several times mentioned in these lectures; viz. *softening—ramollissement*. A great deal of attention has been paid to this condition of late years, both in France and in this country: and some points in its pathology have been fairly made out. I will bring them together as concisely as I can. In the first place, the softening varies greatly in degree, from the consistence which naturally belongs to the cerebral substance, to that of thin cream. In its minor degrees it may be easily overlooked; and is more perceptible by the touch than by the eye. The cerebral matter is less coherent, but it is not yet discontinuous or broken down. It may be washed away, however, by letting a slender stream of water fall upon it; and the softened parts are thus easily distinguishable from those which retain their natural consistence. In the next stage of softening we recognise the complaint at once, for the softened parts undergo a change of form by their own weight: parts that are prominent in the healthy state, as the optic thalami, corpora striata, and convolutions, sink down, as it were, and are more or less flattened. If you make a horizontal section through a part thus diseased, a portion of the softened brain adheres to the knife, and is removed by it, and a depression is left. In a still more advanced degree, the natural texture of the organ in the softened part is entirely destroyed and confused by the change, diffuent: you may pour the softened matter out.

The *colour* of the softened portions varies also considerably. Sometimes they are unchanged in colour: sometimes they are quite white, and present a strong contrast with the tint of the neighbouring parts: sometimes they are marked with various shades of redness, from a rosy pink to an orange, or deep red, or even a mahogany brown. Often there are red spots mixed irregularly with the softened cerebral pulp, and giving it very much the appearance of a mixture of raspberries and cream. In other cases we find the softened mass of a pale yellow, or straw colour, infiltrated, as it were, with purulent matter: and sometimes it is mixed with serous fluid.

Now, it is well established that softening of the brain is a common result of two very different morbid conditions. It is often caused by inflammation of the softened part: it is still more often caused by what I may call its starvation; by the diminished supply of arterial blood, in consequence of diseased blood-vessels.

Can we distinguish these two forms of softening from each other simply by their physical characters? Sometimes we can: and sometimes, it must be confessed, we cannot.

Softening of the brain is usually partial: but this will not help us, for the parts that are most liable to have their consistence diminished through an inflammatory process, are the very parts that are most liable to be softened from defect of nutrition. The most vascular parts of the brain, in short: the grey matter of the convolutions, and the grey matter of the thalami, and corpora striata.

It is stated, however, that softening of the septum lucidum, and of the fornix, very frequently accompanies an accumulation of serous fluid in the lateral ventricles, and very rarely results from disease of the cerebral arteries.

If there be pus mixed with the softened brain, we know that there has been preceding inflammation. Again, if we find the arteries impervious, or unsound, we conclude that the softening has not been inflammatory. Sir R. Carswell states that obliterated arteries may occupy the softened cerebral substance, and often be seen ramifying through it; and that when this substance is removed by pouring water upon it, the solidified vessels retain their situation, and feel sometimes as hard as fine wires. But a much more common condition is that fatty degeneration of the capillary blood-vessels which I spoke of in a former lecture; and this may often be detected by examining, under the microscope, the softened piece of brain. We come to the same conclusion if, no microscope being at hand, we find the larger vessels, the carotid, vertebral, or basilar arteries, obstructed by atheromatous or ossific deposit; and a large portion of the brain unnaturally soft.

We have no certain test of the nature of the softening in its being red. The redness may be the result of inflammatory congestion: but cerebral hæmorrhage may occasion softening; and, on the other hand, softening may give rise to cerebral hæmorrhage. *This* may be said, however: that when the softening extends much beyond the redness, or the effused blood; or when the redness occupies several small portions only of the softened pulp; we may presume that the blood was extravasated subsequently to, and in consequence of, the softening. On the other hand, when redness and vascularity can be traced into the brain, some way beyond the softened part, we may regard the softening as the consequence of inflammation. And we adopt the same belief with still greater confidence, when around the softened and disorganized pulp we find the cerebral substance *hardened*, and of a uniform reddish colour.

In attempting to make the diagnosis between these two forms of softening, we get some assistance by noticing the *age* of the patient. Degeneration of the arterial tissue is almost peculiar to the advanced periods of life; whereas inflammatory softening may occur at any age; in children, in adults, or in old persons. There are other points also in the history and circumstances of the patient, by which our judgment may be aided, and which will be brought under your notice in a future lecture.

When you find the softened substance infiltrated with purulent matter, you may call the case one of *suppuration of the brain*. But suppuration also occurs in another form; viz., in the form of *abscess*. The pus is contained in a regular well-defined cavity, surrounded by cerebral matter, in a healthy or in a hardened state. Now in suppuration occurring in the brain, there is the same puzzling diversity of symptoms as in cases of simple softening. Still, in the main, there seems an approach to the same order of symptoms; convulsions in the earlier period constituting the most prominent feature of the disease; paralysis in the later. I will take one of Dr. Abercrombie's cases in illustration of the formation of encysted abscess in the brain.

A girl, aged eleven, thin and delicate, after having complained for some days of headache, was seized on the 11th of January with convulsions, which continued about half an hour: paralysis of the right arm followed the attack of convulsion. She was bled from the arm, and purged, and cold was applied to her head; and she was much benefited by this treatment. On the 13th the headache was much abated, and she had recovered a considerable degree of motion of the arm. On the 15th the headache increased again, and the arm became more paralytic, and she was again bled: and on the 16th and 17th the power of moving the arm was greatly improved. On the 18th, after being affected with increase of headache, and some vomiting, she became convulsed, the convulsion being confined entirely to the head, and to the right arm; the head was drawn towards the right side, with a rolling movement of the eyes; the arm was in constant and violent motion. She was sensible, and complained of headache. Being bled to eight ounces, the convulsion ceased instantly, and the headache was relieved; but the right arm remained in a state of complete paralysis. Her pulse, during the five following days, fell from 100 to 60; some headache continued; she had occasional vomiting; and the convulsive attacks returned several times; they were entirely confined to the right arm, which after the 23rd, was left in a state of permanent palsy. Hitherto no other parts of the body had been affected by the convulsion; but on the 24th it attacked the right thigh and leg, and left them powerless. The former remedies were repeated without any effect. The thigh and leg went through a course precisely similar to that described in regard to the arm, and on the 29th were permanently incapable of motion.

She was now, therefore, paralytic of the whole right side; she had no return of convulsion, was perfectly sensible, and made little complaint. Gradually she became dull and oppressed, and at length fell into a state of perfect coma, and died on the 14th of February, a little more than a month after the commencement of her illness.

In the upper part of the left hemisphere of the brain there were two distinctly defined abscesses, containing together from six to eight ounces of very foetid pus. They were lined by a firm white membrane; and a thin septum of firm white matter separated them from each other. The one was in the anterior part of the hemisphere, very near the surface; and the other immediately behind it. In the posterior part of the right hemisphere there was a small abscess containing about half an ounce of

pus. There was no serous effusion in any part of the brain, and no other morbid appearance.

In this very interesting case it is worth remarking how the convulsion preceded the paralysis, and how the palsy was more than once diminished by antiphlogistic measures.

It is reasonable to conclude—it can hardly be called a conjecture—that in such cases of partial disease of the brain as I have hitherto mentioned, the occurrence of convulsion or of rigidity, marks the inflammatory stage; and the supervention of permanent paralysis denotes the period of softening or suppuration, of complete disorganization, that is, of the texture of the brain in that part.

Partial inflammation of the brain, especially when it is chronic, sometimes produces a totally different change from any that have yet been described. Instead of becoming softer, or being converted into pus, the inflamed part is *indurated*; comes to resemble in consistence portions of the brain that have been for a short time immersed in weak nitric acid. In this state it is often unusually vascular and injected with blood. When the induration is greater in degree, the hardened part assumes the appearance of wax, or of boiled white of egg, or (as Andral says) of Gruyère cheese, and contains but little blood, but is, on the contrary, distinguished by its pearly whiteness. That these changes are the result of slow inflammatory action is the more probable, because they are sometimes found to exist around an old apoplectic clot or cell; the blood effused having acted as a cause of inflammation of the neighbouring part, just as any foreign substance might do. In the progress of cases in which partial induration is effected, convulsive movements are common, but paralysis does not appear to be so frequently present. The symptoms may go on for months, and often remit, and are again aggravated by paroxysms. These cases are the more interesting, because they offer a greater probability of cure than those that are attended with an opposite condition of the cerebral mass.

Besides these varieties of inflammation, and their consequences, the brain is often infested with *tumours*, which also give rise to a great diversity of symptoms. There are fibrous tumours which grow rather *around* the nervous matter than within it, and are connected with the dura mater. They have been found at almost all parts of the surface of the brain; at its base, at its sides, and towards its summit. Scrofulous tumours are also not uncommon: these are imbedded in the nervous substance, and assume a round form, for the reason formerly mentioned, viz., because the tubercular matter that is separated from the blood is not cast into any particular mould (as it is when it is effused into the small bronchial tubes), but is poured forth into the homogeneous pulp, which exerts an equal degree of pressure upon it on all sides. These scrofulous tumours of the brain are much more frequent in children than in adults; and they are more commonly met with in the cerebral hemispheres than in any other part of the brain, occupying the cortical and medullary substance indifferently. They differ from pulmonary tubercles in this respect, that they are seldom numerous in the same brain. Sometimes one only is found. They vary in magnitude from the size of a large pin's head to that of a hen's egg; and they are sometimes even bigger than that. The substance of the brain immediately surrounding these tumours may be unchanged, in which case it is probable that the tumours themselves give rise to no particular symptoms, the cerebral matter of the spots which they occupy having been gradually absorbed to make room for them; but at length important alterations take place in the neighbouring texture; congestions of blood, or softening, or suppuration; or pressure is exercised upon parts that are essential to sense or motion; and then the ordinary consequences of these changes declare themselves outwardly.

Of minute tubercular deposits upon or beneath the membranes of the brain, in strumous children, I shall have much to say in the next lecture.

Cancerous tumours occur also in the substance of the brain. They usually occupy a large portion of it before they extinguish life. Hydatids are sometimes found there.

Now of the occurrence of these various local maladies of the brain it is necessary that you should be aware, for you may expect to meet with them frequently in practice. And it is right that you should also be aware that they do not disclose their precise nature by any peculiar symptoms, or succession of symptoms. They all, sooner or later, disturb the functions of the organ in which they are situated; and

they may all disturb them exactly after the same fashion. We may judge, sometimes, *from other circumstances*, that the disease is of this or of that character. If we see scrofulous or cancerous disease in other parts of the body, we infer that the symptoms which denote disease of the brain are caused by scrofulous or cancerous tumours there situated; but from the symptoms themselves, we can only learn that there is some morbid condition of the brain.

I attended, with Dr. Latham, a youth, whose symptoms led us to believe that he had tubercular disease of the peritoneum; a very formidable complaint, which I shall more particularly describe hereafter. We thought it probable also, although there were no *physical signs* of pulmonary disease, that his lungs contained crude tubercles. After some time, he went down to the coast; and was there attacked with a fit of general convulsions. Up to that period he had shown no symptoms whatever indicative of organic disease within the head. On being apprized of this seizure, we expressed in a letter to the physician then attending him, our opinion that it had resulted from the presence of scrofulous tumours in the patient's brain. The convulsions returned a few days afterwards with great violence, and he died. It was as we had conjectured. The peritoneum was found studded with innumerable miliary tubercles: there were a few crude tubercles, of some size, around the roots of the lungs; and two large masses of the same sort in the brain. Here, you see, we were directed to a correct special diagnosis of the cerebral disease, simply by the evidence which had satisfied us that scrofulous tubercles existed in other parts of the body.

In the case of specific tumours there is really nothing to be done by way of cure. We must then treat the symptoms, and seek to alleviate them as they arise. When it appears likely, or not unlikely, that the cerebral symptoms may be the result of cerebral inflammation, we must give the patient the chance of being benefited by some of the remedies of inflammation: we must treat the case in this instance upon the *most favourable* supposition. The class of remedies from which most may be hoped in equivocal cases, are local bleeding, counter-irritation, and especially the cautious and regulated employment of mercury. I have stated to you before, that I have known several obscure but threatening symptoms of brain disease clear entirely away, when the gums were made sore by mercury, and kept slightly tender for some little time. It is possible that we may sometimes do our patients harm by this mercurial treatment. We may, now and then, accelerate the arrival of death in persons whom nothing could save; but we must not be deterred from giving them this chance of being rescued from a disorder which may be susceptible of cure, but which, if unchecked, will be inevitably fatal.

LECTURE XXV.

Hypertrophy of the Brain:—Atrophy. Acute Hydrocephalus: its Anatomical Characters; its Scrofulous Nature; Premonitory Signs; different Modes of Attack; Stages of the Disease; Causes.

THERE is a very curious morbid condition of the brain, to which I shall advert before I take up the consideration of certain cerebral diseases as they occur in *children*. The condition of which I am about to speak I was totally ignorant of till I had been for some years in practice. In the spring of 1833 I admitted a young woman, 19 years old, into the Middlesex Hospital. Her countenance was sallow, and her lips pale. She complained of pain in her chest and limbs; of great and increasing debility, and wasting; and of nightly perspirations. She had some cough, and a frequent pulse; and although no morbid sounds were audible in her lungs, I suspected that they might contain small or scattered tubercles. She had been in the hospital scarcely a week, when she had a violent fit of epilepsy; and after recovering from it,

she told us, for the first time, that she was *subject* to such attacks. The convulsions recurred on the same day, and she became insensible, and remained so during the whole of the next day, and till the evening of the day after, when she died. During this period of insensibility she had many convulsive fits; the pupils were dilated, the pulse 100, small and feeble. Leeches were applied to the temples, a blister to the neck, and afterwards to the shaven head, and other measures were used, but in vain.

When the surface of the brain was exposed by the removal of the skull-cap, and of the dura mater, it was observed that the convolutions were remarkably flattened, so that the little furrows between them were nearly effaced: and the surface of the arachnoid membrane was perfectly dry. These are not very unusual, though they are unnatural appearances. I had often seen such before: and I ventured to say that we should find some cause of strong pressure in the central part of the brain: effusion of serum into the ventricles, or a large extravasation of blood, or a growing tumour. Any such source of centrifugal pressure either prevents altogether the outpouring of the natural sub-arachnoid moisture, or forces it away into the spinal canal. But to my great surprise, and much to the discredit of my prophecy, we found nothing of the kind. The ventricles were even smaller than natural, and contained scarcely any moisture. The skull-cap was afterwards examined, and the bone was found to be uncommonly thick, dense, and heavy; and its inner surface, without being rough, was very irregular. I regret that, in this examination, the state of the blood-vessels of the brain, and the consistence of the cerebral matter itself, were not particularly noticed. In the record made at the time by my clinical assistant, it is merely stated that the brain was otherwise healthy. There was no disease in the lungs.

This dissection interested me much, for I had never seen, nor heard of, anything like it before. But upon looking into some modern authors, I discovered that the same phenomena had been noticed by several observers, who had very properly (as it seems to me) considered them as the result of *hypertrophy* of the brain. There is a very good memoir upon the subject, by M. Dance, published in the fifth volume of Breschet's *Répertoire d'Anatomie*: and Andral gives an account of the disease in his *Pathology*. It appears that Morgagni had not overlooked it, for he speaks of instances in which the brain seemed too big for its bony enclosure. When, in these cases, the skull is sawn through, the upper loose portion of bone starts up, as if moved by a spring, and the edges of the bone remain widely apart. Laennec also, in Corvisart's *Journal*, states that upon opening the bodies of persons whom he had thought affected with hydrocephalus, he had been surprised at finding a very small quantity only of fluid in the ventricles, while the convolutions on the surface of the brain were strangely flattened; proving that the cerebral mass had undergone strong compression, which could only have arisen from its preternatural volume, and undue nutrition.

Besides the characters I have mentioned, the hypertrophied and compressed brain is firmer and tougher than natural; it contains but little red blood; and sections of it are seen to be unusually dry and pale.

In most of the cases of hypertrophy of the brain recorded by authors, the patients had suffered epileptic fits, or rather paroxysms of convulsion; and in some of them the convulsions terminated in paralysis. Andral states that the intellectual faculties have been observed, in some instances, to become dull and obtuse. Many of the patients were subject to severe headaches. All these symptoms are common to various cerebral complaints. The diagnosis of this rare disorder can be no better than conjectural; and its treatment we have still to seek.

Andral remarks, what is very true, that hypertrophy of the brain, *i. e.*, an undue and disproportionate development of that organ, may, and does happen, without giving rise to any morbid phenomena at all. But, in such instances, the *brain-case* is equally enlarged in capacity; so that no pressure upon the cerebral mass results from its own preternatural growth. It is only when the brain increases faster than the bony sphere which contains it, that the hypertrophy becomes a disease. In my patient there was also, in one sense, hypertrophy of the skull; the bone was considerably thicker, and more compact and heavy, than is usual; but the capacity of the cavity had not undergone a proportional augmentation: nay, it might, for anything I know, be diminished in consequence of the increased thickness of the bone; the case may

have been one of concentric hypertrophy of the bone, without any fault of the brain itself: but what makes this the less probable is, that in other cases the skull has been found of the ordinary thickness and density; but too small for its contents.

It is of some importance for you to be aware that the brain, and its case, may be extravagantly developed without there being any disease, or any symptoms of disease. M. Scoutetten gives an instance of this which he observed in a child five years old. Its head was as large as that of a well-grown adult person. The skull was from a line and a half to two lines in thickness. The dura mater adhered firmly to the bone, and the cerebral mass exactly filled up the cranial cavity. The superior and posterior part of the brain was developed beyond measure, so that to reach the ventricles it was necessary to make an incision nearly three inches in depth. There was nothing unusual to be remarked in any of the cerebral functions of this child; it was just like other children of the same age in respect of intellect. It died of acute inflammation of the bowels.

The late Dr. Sweatman met with just such another child a few years ago: and I refer to his description of it the rather, because cases that occur near home are always more interesting, and satisfactory, than those which we merely read of in foreign authors. Dr. Sweatman had never read of anything of the kind: but in August, 1834, a little boy, two years old, was brought to him on account of the size of his head. It had been gradually increasing from the age of six months, till it had become so large as by its weight to prevent the child from continuing long in the upright posture. The boy was active and lively, though thin. He never had had any fit or convulsion; but occasionally seemed uneasy, and then would relieve himself by laying his head upon a chair. He had never squinted, nor was he subject to drowsiness, or startings during sleep; and his pupils contracted naturally. His appetite was good, and all the animal functions were properly performed. Dr. Sweatman got Mr. Mayo to see the child with him: they both set it down as a case of hydrocephalus, but agreed in thinking that in the absence of symptoms it would be wrong to risk disturbing his digestive organs by active medicines. In the early part of 1835 the child died of inflammation of the chest, and Dr. Sweatman and Mr. Mayo examined the head. I here show you a cast of it. It measured, from ear to ear, over the vertex, twelve inches; from the superciliary ridges to the occipital, thirteen inches; and in circumference twenty-one inches. The anterior fontanelle, which was quite flat, measured across its opposite angles two inches and a quarter by one and a half; the posterior fontanelle was completely closed, as was the frontal suture. There was no absorption of bone at any part; on the contrary it was becoming thicker. The dura mater adhered with great firmness to the skull; and a layer of false membrane, as big as a crown-piece, was found upon its upper and anterior part. Beneath the arachnoid at that part there was slight jelly-like effusion. In all other respects the organ was sound. The convolutions were perfectly distinct, and retained their proper rounded shape. All the ventricles were found empty, and not dilated. The surfaces, however, of the medullary matter, exposed by repeated sections, presented very unusual vascularity.

The lesson we learn from cases of this kind is, that we are not to regard every child that has a very large head as a hydrocephalic child; and especially that we are not to inflict upon such a child a course of mercury, or other active remedies, unless some morbid symptoms appear. The *nimia cura Medici* may in these, as in many other cases, destroy health; produce disease where none existed before.

[The subject of Hypertrophy of the Brain would appear to demand a more extended notice than has been given to it in the text. There is reason to believe that the disease is of more frequent occurrence than is generally suspected, and that to it are to be ascribed many cases of convulsions, epileptic attacks, inflammation and softening of the brain, and even of idiocy, in which the connection has heretofore been entirely overlooked; we know that hypertrophy of the brain has, in more than one instance, been mistaken for chronic hydrocephalus, and that even the operation of tapping the brain has under such circumstances been proposed, nay, probably performed.]

Hypertrophy of the brain, or at least a condition of that organ predisposing it to undue and more or less rapid augmentation in bulk, is often congenital. Thus children are not unfrequently born with heads of dimensions far exceeding the usual

standard—while, in other instances, the head soon after birth is found to augment rapidly in bulk, disproportionately to the growth of the rest of the body, and within a short period to attain an enormous magnitude.

When the cranium is developed in the same ratio with the brain, at first no morbid symptoms are produced, or only slight ones. In almost every case, however, we have observed more or less apathy, dulness, and drowsiness, to accompany these cases of undue development of the brain, from a very early period.

After, however, the disease has existed for some time, and particularly when there is a disproportion between the morbid development of the brain, and the expansion of the cranium, the patient becomes affected with apathy to external objects, a disposition to somnolency—great irritability of temper, giddiness, habitual headache, attended with severe exacerbations at irregular intervals, and inordinate appetite. The intellect becomes more and more obtuse, verging occasionally upon complete idiocy. There is, usually, a debility of the muscles of the extremities, particularly of the inferior, which constantly increases, until, finally, complete paralysis results.

The bowels are usually torpid, and the pulse remarkably slow. In many cases, the muscles are affected with convulsive twitchings, at first slight, and occurring at long irregular intervals, but gradually becoming more severe and frequent, until regular convulsive paroxysms ensue. The convulsions, not unfrequently, assume all the characteristics of epilepsy.

In some cases there suddenly ensues a considerable reduction, and, occasionally, an entire abolition of sensibility. In other instances, the patient is suddenly attacked with acute delirium, quickly followed by complete coma and death. Mania was observed by Andral in one case. In the majority of cases that have fallen under our notice, the patients have been inclined to fat.

The disease is divided by Andral into two stages:—1st. The chronic, marked by few symptoms, or simply by slight obtuseness of intellect—more or less headache, either permanent or intermittent—vertigo, apathy, drowsiness, broken at irregular intervals by convulsive paroxysms. All of the foregoing symptoms may occur, simultaneously or successively, in the same case, or only one or a part of them may be present. 2d. The acute stage, marked by sudden attacks of violent convulsions, idiocy, epileptic paroxysms, deep coma, or the ordinary symptoms of acute hydrocephalus.

The prognosis in cases of hypertrophy of the brain is not necessarily unfavourable. As Dr. Lees correctly remarks, the affection of the brain is rather an error of development than an actual disease, and the excess of nutrition will often cease, and the brain may even return to its normal state. Many patients who have laboured under cerebral hypertrophy have entirely recovered, others will arrive at puberty, or even a more advanced age, with but little suffering or inconvenience, while others again die at an early age from the accidental occurrence of hyperæmia of the brain, convulsions, or meningeal inflammation. In some instances death occurs suddenly during an attack of convulsions—or, the patient becoming more and more comatose, death finally ensues without a struggle. The chief danger arises from the very great susceptibility of the hypertrophied brain to disease, especially upon the occurrence of any accidental affection of one of the other organs, or of either of the affections incident to childhood.

Upon dissection, the brain is found to be enlarged in size, the convolutions being flattened—the blood-vessels containing a diminished amount of blood, the cortical substance exhibiting in consequence a morbid paleness, with but little or no serum within the ventricles or beneath the membranes. The substance of the brain is in many cases increased in density. Sometimes, according to Sims, the hypertrophy is confined to one lobe, or to the corpora striata or thalami; in all cases, the hypertrophy is chiefly confined to the cerebrum, the cerebellum being seldom much, if at all affected. Instances occasionally occur in which the vessels of the brain are injected with blood; others where a slight amount of reddish serum is found at the base of the brain,—and others again with a clot of blood within the substance of the brain, and rupture of the fibres of its medullary portion; in all these cases, it will be found, upon an inquiry into their history, that death was preceded by symptoms of cerebral disease, in addition to those which properly belong to simple hypertrophy of the brain.

It is important to distinguish the hypertrophied state of the brain from chronic hydrocephalus, to which, particularly in its advanced stages, its phenomena bear a strong resemblance, so much so as to have caused the two to be not unfrequently confounded. Drs. Lees and Muncmeyer point to a particular and very striking projection of the parietal protuberances, in hypertrophy of the brain, as a valuable guide in our diagnosis, while Dr. Hennis Green suggests the difference in the sensation communicated to the fingers when pressed upon the fontanelles in children affected with the two diseases, as a diagnostic sign. The sensation being that of a tense membrane filled with water in cases of hydrocephalus, and of a firm solid substance in cases of hypertrophy. Dr. Mauthner, in his work on Diseases of the Brain (Vienna, 1844), lays down the following diagnosis between these two affections:— In hypertrophy, it is the posterior part of the skull which is first observed to become abnormally prominent, the projection of the forehead occurring subsequently; whereas, in chronic hydrocephalus, the enlargement of the forehead is one of its first results. The latter affection is usually associated with a general emaciated condition of the body; the former with a leucophlegmatic habit, and an increased deposition of fat. The constitutional symptoms likewise differ in the two affections—restlessness, convulsions, and sopor, mark the *early stages* of chronic hydrocephalus, while spasmodic affections of the respiratory organs are among the earliest indications of hypertrophy of the brain, but seldom occur until the advanced stage of hydrocephalus. (*See Condie on Diseases of Children, 4th edition.*)—C.]

Having told you what I know of *hypertrophy* of the brain, it is proper that I should say a word or two respecting the opposite condition; of *atrophy* of the cerebral mass. There are two forms of this affection: one is congenital, and results from imperfect development, or from an arrest of development, of the brain in its fetal state. In the other the change appears to take place in consequence of disease, either in the membranes of the brain, or perhaps in its arteries; though the effect of disease in the arteries is usually softening, which is a species of atrophy. But in the atrophy to which I am now alluding, the *volume* of the atrophied part is diminished, not its *consistence*. And the diminution of size may extend only to a few convolutions: or it may be most manifest in the interior of the organ; in the optic thalami and corpora striata for example. There is still another alteration to which some have applied the term atrophy, though improperly, I think: I allude to those cases, which I shall speak of more particularly soon, in which the form and disposition of the cerebral substance is altered, the convolutions being unfolded, and the nervous matter spread out by a large collection of fluid in the interior cavities of the brain, constituting the disease called *chronic-hydrocephalus*. I have not much to say upon what may be styled atrophy *proper* of the brain: that it will give rise to symptoms we cannot doubt, but that it shows itself by any peculiar or characteristic symptoms is what I have not discovered.

I shall content myself, on this subject, with showing you Cruveilhier's representation of a strongly pronounced example of atrophy of the entire cerebrum on one side. The drawing from which this engraving was made, was painted from the body of a patient who died in the Hôtel-Dieu, dropsical, in consequence of disease of the heart. He was forty-two years old. When you look at the engraving you will perceive that the left side of the cerebrum is diminutive compared with the right. It filled up, however, a larger space than it appears to do in the plate, for the lateral ventricle on that side was distended by a quantity of serous fluid, which ran out when the ventricle was punctured; and then the surface of that side of the brain sank down, and collapsed. Still the convolutions on that side, and all the dimensions, are remarkably less than on the other. The anterior lobe projects half an inch further on the right than on the left side. The frontal bone, you will observe, is much thicker; twice as thick on the atrophied as on the natural side; and the frontal sinus is very wide and open. The internal parts of the brain are all diminished in proportion. There was a large quantity of serous liquid filling and distending the subarachnoid areolar tissue. The nervous matter was whiter and harder on the atrophied side. One very curious thing is, that the *left* lobe of the *cerebellum* was the bigger of the two; but there was no such marked difference between them as between the two sides of the cerebrum.

Now the patient in whom this singular disproportion between the two sides of his brain was met with, had been incompletely hemiplegic, as long as he could recollect, on the right side; and the imperfectly palsied limbs were shrunk and withered, and the fingers of the hand contracted. Yet he had managed to walk about with the help of a stick; and there was nothing remarkable, one way or the other, in the state of his intellectual faculties.

The same condition has been seen on both sides of the brain: the organ itself existing in miniature as it were, and lying at the lower part of the vaulted cavity of the cranium: the intermediate space being filled up with water. This condition of the cerebrum is accompanied by idiocy. In long-standing cases of this description you must not suppose that the nervous matter has been compressed into a smaller compass by the effused fluid; but that the fluid has been poured out to fill that part of the skull which is empty of brain, and which must be filled with something.

I proceed in the next place to the consideration of that disease to which the name of *acute hydrocephalus* has been given. By that term I desire to signify *inflammation of the brain*, as it frequently occurs in *children*, and especially in *scrofulous* children. The inflammatory character of the disorder, though not always very clearly expressed in its symptoms, is sufficiently attested, in many of the fatal cases, by the changes discovered within the cranium.

I made some observations, in the last lecture, respecting the nomenclature of diseases, and said something in defence of the term *delirium tremens*. Now it must be confessed that the complaint we are about to consider was unfortunately named, when it was called *hydrocephalus*. I repeat that it matters not at all how we denominate a disease, provided that its title does not involve any erroneous notion of its *nature*. I think *hydrocephalus* a bad name, because it reminds us of one circumstance only of the malady, viz., the serous effusion, which so far from being the *cause*, or the *essence*, is only a *frequent effect* of the disease; nay, it is no uncommon effect of other morbid conditions also, besides inflammation. But *hydrocephalus*, or water in the head, is an appellation so established, both among ourselves and with the public, that I cannot venture to propose any change.

In early life, simple encephalitis is not often seen; and when inflammation of the brain does befall a child of healthy frame and constitution, it resembles in its general course and features the same complaint, occurring in the adult patient. What we call *acute hydrocephalus* is always, I believe, associated with the *scrofulous diathesis* — always an instance of *scrofulous disease*. Allowing for diversities of structure and function, *acute hydrocephalus*, *phthisis pulmonalis*, and *tabes mesenterica*, may respectively be regarded as the ordinary results of the same morbid tendency, manifesting itself in the three great cavities of the body, the cranium, the thorax, and the abdomen.

If you have recourse to books for a knowledge of this disorder, you will meet with endless discussions, and most perplexing differences of opinion, respecting its true pathology, and its proper management. To check, or to verify, by individual observation, the notions received from indiscriminate reading, requires peculiar opportunities, such as few enjoy save those who are largely engaged in the practice of midwifery, and familiar, as the natural consequence of that practice, with the diseases of children. By far the best exposition that I have seen of what is known upon the subject, is given by Dr. West, in his published “*Lectures on the Diseases of Infancy and Childhood*.” Upon a careful selection of accredited facts from various writers who have preceded him, Dr. West has cast the clear light of his own well-used and large experience.

In the first place, *acute hydrocephalus* is an *inflammatory disease*.

We are led inevitably to this conclusion by its *symptoms*, which much resemble those that occur where undoubted inflammation has arisen from injuries of the head: by the *appearances seen on dissection*, which are always such as inflammation might have produced, as softening, and the effusion of serous fluid; and frequently such as nothing but inflammation could have produced, as suppuration, and the formation of adventitious membranes: and lastly, by the unequivocal relief afforded by blood-letting, and other evacuations, the blood drawn being also sometimes sisy.

Let us take the least equivocal of these three kinds of evidence—the morbid appearances presented after death. What are they?

In some cases we find traces of inflammation of the membranes of the brain; a firm attachment of the skull-cap to the dura mater; occasionally some adhesion of the opposite surfaces of the arachnoid to each other. Sometimes there is an effusion of serous fluid beneath the arachnoid, in the meshes of the pia-mater, and especially in the depressions between the convolutions. You would suppose, upon looking at this collected fluid through the arachnoid, that it had the consistence of jelly: but it is not so. If you divide the arachnoid by means of a sharp scalpel, a perfectly limpid fluid makes its escape. More commonly the arachnoid is drier than is natural, has a dull lack-lustre aspect, and feels sticky to the touch. Not unfrequently there are layers of coagulable lymph interspersed between the arachnoid and pia-mater: this is a most unquestionable evidence of foregone inflammation; and it is most often met with in the strongly marked cases. When portions of the cerebral mass are removed by slicing it, a great number of red points are frequently to be observed, speckling the cut surface. I mention this appearance just to say that, to the best of my belief, it does not warrant any conclusion in respect to the state of the brain before death. We find these red spots numerous in many cases, where there had been no cerebral affection manifested during life; and they are not always to be seen when we are certain that there was inflammation.

[The grey substance of the convolutions, in cases in which the sub-arachnoid tissue is strongly injected, is usually of a pale rose, or bright red colour. The lining membrane of the ventricles is occasionally injected, opaque, or covered with a pseudo-membranous exudation, or with numerous white flocculi, which become very apparent when the membrane is immersed in water. It is often easily separated from the cerebral substance. The plexus choroides is very often injected, and thickened; sometimes, however, it is pale and discoloured, and lined with small hydatiform cysts; this latter appearance has, also, been found in the cellular texture of the pituitary gland.—C.]

With respect to the nervous matter itself, it is said to be sometimes softer throughout than natural, and occasionally it has been found infiltrated, as it were, with serous fluid; *wet*, and so rendered soft. Gölis describes an instance of this kind, in which, he says, the fluid could be expressed from the cerebral substance as from a sponge.

[In some cases the substance of the brain has been found of a firmer consistence than natural, and to a certain extent hypertrophied. A case is related by Gölis, in which, upon opening the skull, the whole brain expanded, so that it could not again be replaced within the cranium. The convolutions are sometimes flattened, apparently from pressure against the skull.—C.]

A more common and characteristic change is *softening of the central parts of the brain, with an effusion of serous fluid into the ventricles*. Generally the effused fluid is thin and watery; serosity rather than serum. It contains less animal matter, perhaps, than any other animal production. Dr. Bostock found that of 100 parts, 98·6 consisted of water, 1 part of salt, and 0·4 only of animal matter. It is not, therefore, in common, coagulable by heat. The quantity effused is uncertain; speaking generally, it varies from two to six ounces.

In 38 out of 40 cases, in which death had taken place under the symptoms of acute hydrocephalus, Dr. West found an appreciable quantity of fluid in the ventricles; and in 34 of these cases the quantity was considerable, amounting to several ounces.

[In many cases the amount of effused fluid is very trifling; in some scarcely a trace is to be discovered. The effusion may take place in the arachnoid or sub-arachnoid tissues, or in the ventricles, or in all these parts at the same time. The greatest amount is generally met with in the lateral ventricle—occasionally the quantity is so great as to enlarge the posterior cornu, elevate the fornix, rupture the septum lucidum, and thus establish a free communication between all the ventricles. The cellular tissue of the choroid plexus may also be distended with serum. When

the serous effusion in the brain is considerable, it is often found also in the spinal canal.—C.]

The effused fluid is not always, however, clear and limpid: sometimes it is turbid, like whey, or even puriform, with flocculent shreds floating in it. These have been considered as flakes of coagulable lymph; but I question whether, in many cases, they are not mere fragments of the softened and broken down materials in the neighbourhood; for the septum lucidum, the fornix, and other parts forming the walls of the ventricles, are very commonly found to be soft and pulpy, or entirely disorganized.¹ The septum is perforated perhaps by a ragged irregular opening, the softened portion having fallen out: the fornix has lost its consistence, and often its figure, or falls asunder when the most gentle attempt is made to raise it. It was Dr. Abercrombie's opinion, not only that this softness is the result of inflammation, but that in very many cases of acute hydrocephalus, inflammation of these central white parts constitutes the essence of the disease. He relates two striking examples, in which this softened condition of the septum lucidum, fornix, and corpus callosum, without any effusion of serum, or any other morbid appearance, was found, after death, preceded by symptoms which are usually considered to indicate acute hydrocephalus.

It has indeed been thought that the softening of these central parts may sometimes be the consequence of their maceration in the effused fluid. But this notion is disproved by the fact that the ventricles are often found full of fluid when there is no defect of consistence in the cerebral substance forming their walls. Among thirty-eight cases, carefully noted by Dr. West, there were thirteen in which no central softening existed, although the ventricles contained fluid in every case but one. He refers to the statements of a German, Herrich, who found central softening of the brain in forty-seven only out of seventy-one instances, in which the ventricles contained from three to eleven ounces of serum. Finally, M. Rokitsansky has ascertained, by direct experiment, that slices of cerebral matter may be soaked, for hours, in serum, without undergoing any change of consistence.

In most instances the membrane lining the ventricles exhibits distinct traces of inflammatory action; is seen to be vascular, opaque, even obviously thickened.

[In the cells of the arachnoid membrane there is often deposited a concrete yellowish matter, either soft and inelastic, or somewhat firm, elastic, and of a shining appearance. It is deposited either in patches, or in lines bordering the blood-vessels; and, as is the case with all the indications of inflammation in this disease, it is more commonly met with at the base than at the summit of the brain. Granulations and miliary tubercles are often interspersed in its midst. The whole base of the brain is often covered with a continuous layer of the yellowish gelatinous deposit alluded to. This deposit differs from the matter effused in inflammation occurring in persons unaffected with tuberculous disease; the difference is thus traced by Barthez and Rilliet (*Malad. des Enfants*, tom. iii). The former is almost always solid, the latter almost always fluid; the former occurs more especially at the base of the brain, the latter upon its convex surface; the former is of limited extent, particularly when upon the surface of the hemispheres, the latter may spread over the greater portion of the surface of the brain; finally, the former is almost invariably found in the cells of the pia mater, while the latter occurs habitually in the great cavity of the arachnoid.—C.]

Dr. West gives an interesting account of the inflammatory changes presented by the superficial investing membranes. Upon the convexity of the brain these alterations are often comparatively slight; while at its base they are almost always conspicuous. In thirty-four out of thirty-nine fatal cases he found the membranes of the base to be the seat of disease, more or less extensive, and always exceeding that which existed at the vertex.

"The least considerable (he says) of the morbid changes in the membranes at the base of the brain consists in a milky or opaline condition of the arachnoid and pia mater, but chiefly of the former, sometimes extending over the whole lower surface of the cerebrum, but seldom being equally apparent in that part of the membrane which invests the cerebellum. But, besides this opacity, we usually observe much

¹ [This is the opinion of Rilliet and Barthez.]

more distinct evidence of inflammatory action in the effusion of yellow lymph beneath the arachnoid. This is generally found about the olfactory nerves, which are often completely imbedded in it; while a similar effusion extending across the longitudinal fissure unites the two hemispheres of the brain together. A deposit of the same kind likewise reaches up the fissure of Sylvius in many cases, and connects the anterior and middle lobes of the brain with each other; or if poured out in less abundance, it may be seen running up in narrow yellow lines by the side of the vessels as they pass from the base of the brain towards its convexity. It is in the neighbourhood of the pons varolii, however, and about the optic nerves, that the most remarkable alterations are met with. The opacity of the arachnoid is here particularly evident, while the subjacent pia mater is opaque, much thickened, and often infiltrated with a peculiar semi-transparent gelatinous matter, sometimes of a dirty yellowish-green colour. This matter is sometimes so abundant as perfectly to conceal the third and fourth nerves, and at the same time to invest the optic nerves with a coating two or three lines in thickness; though, on its being dissected off, the substance of the nerves beneath appears quite healthy. When this morbid condition exists in a very considerable degree, it extends beyond the pons, and involves the membranes covering the medulla oblongata, especially at its anterior surface."

Enough, I think, has been said, to convince you of the inflammatory character of this fearful malady. But, secondly, acute hydrocephalus is a *tubercular* disease.

Occasionally, scrofulous tubercles, of considerable magnitude, are discovered in the substance of the brain; and it is probable that these would have been *more* frequently met with, if they had always been carefully looked for. They consist of a cheesy kind of matter, like that of large tubercles in the lungs.

Much more commonly the tubercular deposit manifests itself in the shape of small granules, scattered, many or few of them, upon or between the membranes of the brain. This fact has not hitherto received, in this country, that degree of notice which its great importance deserves. It has engaged the attention, for some years past, of several of the French physicians. The following clear summary is given by Dr. West of the result of their observations.

"The conclusion to which we are led by their careful investigation of the subject is, that the peculiar granular appearance which various parts of the membranes of the brain often present in this disease, is not due to inflammation, as was once supposed, but is occasioned by the presence of minute tubercular deposits. These deposits often assume the form of minute flattened spherical bodies, of the size of a small pin's head, or smaller, and either of a yellowish colour, and rather friable under pressure, or greyish, semi-transparent, and resistant, almost exactly resembling the grey granulations which are sometimes seen in the lungs or pleuræ of phthisical subjects. They are likewise sometimes met with in what would seem to be an earlier stage, when they appear like small opaque spots, of a dead white colour, much smaller than a pin's head, and communicating no perceptible roughness to the membrane. This appearance is often observed in the arachnoid covering the cerebellum, and those parts of the base of the brain where the arachnoid is stretched across from one portion of the organ to another. The flattened yellowish bodies are more frequently seen at the convexity of the brain, and on either side of the hemispheres. They generally follow the course of the vessels that ramify in the pia mater, and accordingly occupy the sulci between the convolutions much oftener than their summit. The firm grey bodies are mostly seen about the pons, or imbedded in the pia mater in the neighbourhood of the optic nerves, or projecting from the surface of the membranes that cover the medulla oblongata. They are also often deposited in the arachnoid lining the occipital bone, and are then sometimes collected in considerable numbers around the foramen magnum. These bodies, sometimes of a grey, at other times of a yellow colour, are likewise met with, though less frequently, in the substance of the velum interpositum, or imbedded in the choroid plexuses; and in both of these situations they are sometimes very abundant."

"These bodies, however, do not always retain the appearance of distinct granules, but sometimes on separating two folds of the arachnoid, which had seemed to be glued together by an effusion of yellow lymph or concrete pus, we find that the matter which formed these adhesions is not homogeneous, but that it consists of an aggregation of minute granular bodies, connected together by the lymph or pus in which

they are imbedded. This appearance is often met with at the convexity of the brain, and close to the longitudinal fissure, and rather more towards its posterior than its anterior part; a strip of this yellow matter, half an inch long by two or three lines broad, connecting together the two hemispheres of the brain, or the two surfaces of the arachnoid. Sometimes two or three deposits of this kind are observed at the convex surface of the brain, but they are generally more extensive at the base of the organ, where they occupy the longitudinal fissure and the fissure of Sylvius, and frequently connect opposite surfaces of the brain so closely together as to render their separation impossible without injury to its substance."

The reasons which have convinced Dr. West of the tubercular nature of these deposits are—

"1st. That they are always associated with tubercle elsewhere.

"2d. That their abundance is not in proportion to the amount of inflammatory mischief.

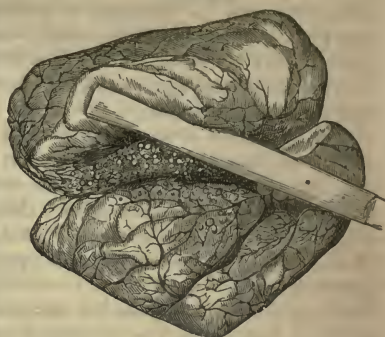
"3rd. That they are sometimes met with in cases where no head symptoms were observed during life, and unconnected with any sign of inflammation discovered after death:—and

"4th. That their chemical composition and their microscopic structure are identical with those of tubercle in other organs of the body."

In this disorder the tubercular deposit may either provoke by its presence the supervening inflammation, or render the brain and its membranes more liable to suffer inflammation from other influences: in other words, the tubercles are probably sometimes the sole exciting cause of the inflammatory mischief, sometimes merely a strong predisposing cause. We see, in the changes which lie beyond and precede the inflammation, too plain a reason for the generally hopeless character of the resulting malady.

[Tubercles, varying in size from that of a pin's head to that of a pea, are very generally found scattered irregularly over the surface of the pia mater, following it between the convolutions; occasionally, however, they occur in distinct patches of an inch or more in extent. They are commonly hard, and semi-transparent, sometimes opaque and of a whitish, greyish or yellowish colour. They are found upon all parts of the surface, the convex and lateral portions as well as the base, in the inflexures of the convolutions, and in the fissures. According to Rilliet and Barthez, they are more frequent upon the convex surface of the hemispheres than at the base. Dr. Hamernjh (*Schmidt's Jahrbucher*, 1845) found them more frequently at or near the base of the brain. They are much more abundant upon the brain than the cerebellum. They are met with, also, imbedded in the grey matter of the brain, and are here often surrounded by a halo of redness, usually connected with an enlarged vessel, ramifying from the pia mater. More rarely, tubercles are detected in the medullary portion of the brain, where they are often overlooked in consequence of their pale, semi-transparent, yellowish tint. The plexus choroides is, also, often covered with tubercles. They are very commonly met with, likewise, on the serous membranes of the thorax and abdomen, in the lungs, and occasionally in the substance of the liver. In *twenty-seven* out of *thirty-three* cases of hydrocephalus, Barthez and Rilliet found tubercles or granulations, associated with inflammation of the pia mater; in *four* cases the meningitis was unattended by any trace of tubercular deposition in the encephalon; and in *two* cases, the granulations or meningeal tubercles were unattended with any traces of inflammation. In all the thirty-three cases the symptoms were nearly identical.—C.]

FIG. 25.



Deposit of tubercular matter in the Sylvian fissure of the brain of a child, aged 19 months, who died ten days from the first appearance of head symptoms, which were treated antiphlogistically. The white circular spots represent the tubercles, which were surrounded by highly congested blood-vessels. The white deposit examined by the microscope showed granular matter and granular corpuscles varying in size.

After what I have already stated in respect to simple inflammation of the brain in adults, you will be prepared to hear that acute hydrocephalus (remember, I restrict that term to the same inflammatory malady as it occurs in strumous children)—I say you will not be surprised to learn that acute hydrocephalus furnishes a great variety of symptoms; and many variations in the mode of their coming on, and in their combination, and succession.

It is obviously of the greatest importance to recognise acute hydrocephalus in its *earliest* stages; and even to look out for indications of its approach. I shall, therefore, describe those changes in the state of the young patient, which have been found to be, in many cases, premonitory that the disease was impending. But such symptoms are by no means always followed by acute hydrocephalus; nor is acute hydrocephalus always preceded by such symptoms. Still, when they do occur, they should put us upon our guard.

The *precursory symptoms* to which I allude consist chiefly in a morbid state of the nutritive functions. The child loses his appetite; or his appetite becomes capricious: he sometimes appears to dislike his food, and sometimes devours it voraciously: his tongue is foul, his breath offensive, his belly enlarges, and sometimes is tender; his bowels are torpid, and the evacuations from them unnatural; the stools are pale and contain but little bile; or they are dark with vitiated bile, fetid, sour-smelling, slimy, or scybalous; and the child loses his former healthy aspect, becomes paler and thinner. Even already there are obscurer indications of derangement in the cerebral functions; the child is heavy, taciturn, languid, slow, dejected; his customary spirit and activity are gone; he grows fretful and irritable, or drowsy and listless, and is manifestly uneasy; and sometimes he manifests a little unsteadiness and tottering in his gait.

In some children, when the disorder is at hand, or incipient, an unnatural wakefulness is often observable: or restless sleep—attended by grinding of the teeth, or moaning—is interrupted by sudden awakings in distress and alarm. A frequent sudden cry or scream, a clenching of the little fists, and a turning in of the thumb towards the palm of the hand, give warning also of the approaching malady.

Now when this sort of alteration is observed in a child who has any hereditary title to scrofula, or bears the marks of the strumous *diathesis*, or is even a precocious and particularly clever child, and still more if he present any other indication of strumous *disease*, there will be much reason to apprehend that mischief is brewing within his head. I advert to these tokens of scrofula, because the cerebral inflammation is, in every case probably, of a scrofulous character. But there is this peculiarity in it, which distinguishes it from scrofulous inflammation in most other parts, viz., that as it occurs in an organ of very delicate structure, and one which is essential to life, its progress is more rapid, and it is more necessary to treat the disease promptly.

It has been made a question whether the derangement of the digestive organs that has just been described is or is not the immediate exciting *cause* of the affection of the brain; or whether both the abdominal and cerebral disorder are not common and concurrent effects of the same cause. You will not have much difficulty in replying to that question. It is said that the stomach and bowels are more in the way of being acted upon by injurious influences than the brain, and that, therefore, the inflammatory quality of the complaint may be supposed often to arise from their derangement; and great good, it is alleged, is done, the disease of the brain is often *prevented*, by remedying the disordered condition of the stomach and bowels. On the other hand, it may be stated that a similar derangement of the digestive organs often comes on and lasts long in children, without leading to hydrocephalus; and hydrocephalus often attacks a child in whom no such symptoms of abdominal disease have appeared. We can never be certain, therefore, that hydrocephalus has been prevented, in any given case, by remedies addressed to the digestive organs. I cannot think the question is one of much practical importance. Whether the disturbances of the nutritive functions *cause* the brain disease, or merely *indicate* it, they are equally valuable in directing our attention to the head.

In these little patients any source of irritation seems to act as an *exciting* cause: surgical operations, which are sometimes necessary at that tender age—falls or injuries of any kind—painful dentition.

There are, at least, three several ways in which this disease may make its attacks; and with these it is proper that you should be acquainted.

In the first place, it may come on *gradually*; after such symptoms as have already been spoken of as being premonitory. Probably this is the way in which it most frequently commences. After a period, of uncertain duration, in which the child has complained of occasional pains in the belly and head, and signs of derangement of the stomach and bowels have been present, the pain in the head begins to be more severe and to recur more frequently. It is not mere headache, but generally a sharp shooting pain, recurring at intervals; sometimes it affects one side of the head more than the other: the little patients wake and shriek out with the pain, and this in children is a very characteristic symptom. As coma comes on, this shrieking gives place to an habitual moaning, which is scarcely less characteristic. Very often in the beginning of the disease there are pain and stiffness at the back of the neck; sometimes there is much pain of the limbs in the early periods, and in some children extreme tenderness of the scalp, so that they cannot endure to have the head shaved. The pain of the head becomes complicated with vomiting, and both these symptoms are aggravated by motion. Very often nausea is excited by the erect posture, and the patient begs to lie down. The child sighs frequently, and looks grave or sad; his eyes are pained by a strong light, so that he knits his brows. The pulse becomes rapid, and the disturbance and irregularity in the abdominal functions increase. This stage of the complaint may last several days, the child becoming daily more weak and more peevish, and looking more and more ill.

In the second form of attack there are no premonitory symptoms; or they occur for a very short while only, before the disease sets in suddenly and violently, with acute pain in the head and high fever; or with convulsion: the face is flushed, the eyes are brilliant; there is intolerance of light and of sound, and there are pain and tenderness of the abdomen. When the disease commences in this manner, there may be some reason to hope that it is simple encephalitis; cerebral inflammation unaccompanied with tubercular deposit. The symptoms are not unlike those which sometimes mark the onset of continued fever. You may find these varieties described in Dr. Cheyne's excellent treatise on this disorder. "We are led to suspect," he says, "some deeply-seated evil from the frantic screams and complaints of the head and belly, alternating with stupor, or rather lowness, and unwillingness to be roused; and we are struck with the great irritability of the stomach, which exists in a degree beyond what we generally find it in the fevers of this country; retching and vomiting being brought on by a change of posture, and certainly by every attempt to sit up in bed; and the disordered state of the bowels, which attends this irritability of the stomach, is also remarkable: and when at any time the child has a little respite from the violence of these symptoms, we find our suspicions confirmed by his looks; for when the features do not express pain or terror, there is not unfrequently a vacancy of look, the eyes being *set*, with an expression of dejection which is peculiar to certain diseases of the brain." The mode of attack which has now been described, although the most regular in its progress, is not so common as the first, nor as the third, which I have yet to mention. The *third* way in which the disease makes its advances is very insidious; the head symptoms supervene upon the subsidence of some other malady: presently after the disappearance of an eruption from the scalp; during the decline of scarlet fever, small-pox, hooping-cough, or any inflammatory or febrile complaint; and even after painful dentition. In these cases the early symptoms are often but slightly marked, or do not take place at all; the sudden occurrence of convulsions or paralysis affording the first evidence that the brain is implicated. This is the most dangerous form of hydrocephalus. It has received the expressive title of *water-stroke*.

In whatever way the disease makes its invasion, it is apt to be attended with many and variable symptoms; and different observers, with a view of facilitating their description of the disease, and of making it more intelligible and more easily remembered, have divided the symptoms into groups, and considered each group as characteristic of a particular *stage* of the malady. But they have not all done this in the same way. It may be of use, however, to inform you of the different classifications which have thus been proposed. Dr. Whytt, who was almost the first person in this country who wrote upon this disease (I believe Dr. Paisley, of Glasgow, was the first:

you may see his paper in the third volume of the *Edinburgh Medical Essays*), Dr. Whytt, I say, whose description is an extremely good one, took the *pulse* — which undergoes very remarkable variations in the course of the disorder — as the ground of his division. He makes three stages of it therefore; the first, in which the pulse is frequent; the second, in which it is slow and irregular; and the third, in which it again becomes frequent and feeble. These successive fluctuations in the pulse are to be noticed in very many cases. Dr. Gölis, again, an eminent German writer on hydrocephalus, whose little work was translated by the late Dr. Gooch, as being the best book on the subject that he was acquainted with, makes four stages, according to what he believes to be the condition of the *brain* in each. First, he has the period of *turgescence*, which corresponds with that period in which the premonitory symptoms occur; secondly, the period of *inflammation*; thirdly, the period of *effusion*; fourthly, the period of *palsy*. The two last would appear to be almost identically the same. Dr. Cheyne makes three stages; which he finds marked, not like Dr. Whytt, by the state of the circulation, but by the state of the nervous system. Thus he calls the first the period of *increased sensibility*, when every stimulus produces an inordinate impression. In the second stage, that of *diminished sensibility*, the child is not easily roused, his pupil is dilated, and his pulse slow; he is lethargic, with obstinately costive bowels. The third stage with him is that of *palsy and convulsions*, in which there are squinting, rolling of the head, stupor, convulsions, with a rapid thready pulse.

Cases often occur, however, that baffle all these attempts at classification. Convulsions, instead of being among the *last*, are not seldom among the very *first* symptoms. The pulse is sometimes remarkably *slow* at the *outset*; sometimes *frequent* through the *whole disease*; and sometimes *perfectly natural*.

I do not make these statements to magnify the difficulty of distinguishing the disease; for the diagnosis is really not so difficult as it has sometimes been represented; but to show you that you must not trust to any succession of symptoms, still less to any one symptom, as being pathognomonic.

The symptoms that occur during the first stage are very variable, as you may suppose from what I have said of the different modes in which the disease is apt to set in. Those that are most constant are, pain of the head, severe shooting pain I say it seems to be, for the child puts its hand there, and cries out frequently "Oh! my head;" restlessness; inability to sit up; very disturbed sleep, with grinding of the teeth, and from this sleep the child often starts apparently in terror, and with a scream. The head is hot externally; the little patient is annoyed by light and by noise; the pupils are contracted most commonly during this stage; the child is unwilling to be disturbed, and, therefore, does not reply readily to questions; but the replies, when made, are correct and rational. This stage is marked also by vomiting, a total loss of appetite, a white tongue, offensive breath, costive bowels, unnatural stools, green often, or black, like tar, scanty, and high-coloured urine. Dr. Gölis says that the abdomen, which has been tumid and tender perhaps, sinks down and becomes flat, without any increased excretion by stool; and that this is a very characteristic symptom. The intestinal gases disappear. The pulse in this stage is frequent and sharp. In short, the symptoms are such (in general) as indicate very plainly that inflammatory action is going on within the head. Now the symptoms that characterize this first stage of the complaint sometimes rapidly pass into those which belong to the second. They may not be present for more than a few hours; or they may last a day or two, or several days; it is very seldom, I believe, that they continue longer than a week. The period answers, in the general character of the symptoms, to the period of *excitement* in encephalitis, which I repeat is very much the same disease, modified by its occurrence in the adult and otherwise healthy subject.

So also the *second stage* of acute hydrocephalus corresponds, in its general features, with the period of *collapse* in encephalitis. The pulse becomes irregular, extremely variable and fluctuating, and often *slow*: it is easily accelerated, however, by the smallest exertion — by taking the child out of bed, or even raising him into a sitting posture. With this slowness of the pulse come on a diminution of sensibility, and general heaviness and stupor; the pupils dilate, the light is no longer troublesome, the vision is imperfect, often it is doubtful whether the child sees at all. If the eye be closely examined and watched, the degree of light remaining the same, the size of

the pupil will frequently be seen to fluctuate, or oscillate, till at last it is wide open and immovable. While this goes on, squinting takes place, and double vision when the child can yet see anything. One or both eyes are turned in, or more rarely outwards. Noises do not now disturb or irritate the child—who lies on his back, with eyes half closed, in a state of drowsiness or stupor, which is occasionally interrupted by some cry or exclamation expressive of pain. Convulsions frequently occur, but not uniformly; slight and partial spasmodic twitchings; or general and long-continued convulsions; paralysis; sometimes hemiplegia. The vomiting generally ceases. The urine and stools are passed unconsciously. Sometimes the child, with feeble and tremulous hands, is incessantly picking his lips, or boring his fingers into his ears or nostrils.

This stage may last a week or two. And what is remarkable, it is often attended with remissions, sometimes sudden and sometimes gradual—deceitful appearances of amendment, and even of convalescence. The child regains the use of its senses; recognises again its attendants; appears to its anxious parents to be recovering;—but in a day or two it relapses into a state of deeper coma than before. And these fallacious symptoms of improvement may occur more than once.

The third stage does not differ materially in the character of the symptoms that accompany it, from the second, except that the pulse again becomes frequent, nay, uncommonly rapid; beating sometimes 200 strokes in the minute, so that you can scarcely count it. Dr. Whytt, in one instance, reckoned more than 210 pulsations. The child rolls its head perpetually from side to side; moans continually; waves its hands in the air, or one hand, the other frequently being palsied; sometimes there is paralysis of one side, and convulsive twitchings of the other. The circulation is very unequal; one part of the body will be found hot and dry, and another covered with a cold sweat; the cheeks are alternately pale and flushed; the child is raving, or insensible; the rapid pulse gets more and more weak; and at length the patient expires. In many instances death takes place in the midst of a strong convulsion. This last period is of very uncertain duration; it may be over in a few hours, or it may last a fortnight.

For my own part, I conceive that for all practical purposes it would be quite enough to make two stages only of this disease. In the first, the symptoms are those of inflammation of the parts within the cranium, or of some of those parts; in the second, we have the symptoms that result from the consequences and products of the inflammation, from softening, and from the effusion of serum. And frequently these sets of symptoms are, in some respects, common to both these causes; and more frequently still the causes coexist, effusion taking place, yet the inflammation going on. And we may understand how the whole collection of symptoms may vary and fluctuate, and assume an uncertain character, according as the inflammatory process has ceased, or is still in progress; according as it exists alone, or is mingled with the further source of cerebral disturbance that is furnished by its own events; and according as the inflammation may have come to an end, while its events remain behind, and declare their presence by appropriate signs in proportion to their place, their extent, and their various kinds and combinations.

The disorder with which acute hydrocephalus, in its commencement, may be most readily confounded, is the remittent fever of childhood. I borrow from Dr. West the criteria which help us to discriminate between these two complaints.

The vomiting, which is so grave a symptom of approaching hydrocephalus, is often absent in remittent fever, even at its onset; or if present, it soon ceases, and is not succeeded by that abiding nausea which is frequent in hydrocephalus. In remittent fever, the bowels are often relaxed from the very first, or speedily become so; and the evacuations present no resemblance to the scanty, dark, or mud-coloured stools which are avoided in hydrocephalus, but are usually foecal, watery, and of a lightish colour. Tenderness of the abdomen is nearly constant in remittent fever, and is greater in the iliac regions than elsewhere, and wind can always be felt in the intestines. The tongue is not moist as in hydrocephalus, and is seldom much loaded, but has only a thin coating of yellow fur in the centre and towards the root, while it is very red at the tip and edges, and becomes dry at an early stage of the disease. In hydrocephalus there is frequently a great distaste for drink as well as for food, while although the appetite is lost in cases of remittent fever, yet the patients have a strong

desire for drink, especially for cold drink, to quench the urgent thirst. The heat of skin in remittent fever is extremely pungent and much greater than in hydrocephalus, in which, although there is great dryness of the surface, yet the temperature is seldom much increased. The pulse in remittent fever is much quicker than in hydrocephalus, continues quick throughout, and never becomes unequal or irregular, while its frequency is in direct proportion to the elevation of the temperature of the skin. In remittent fever the child makes few complaints about its head, but delirium is of early occurrence, especially at night; in hydrocephalus, on the contrary, true delirium hardly ever occurs till an advanced period of the disease, and is sometimes absent altogether. In remittent fever, as its name implies, there are distinct remissions and exacerbations of the symptoms, the patient getting better towards morning, and worse again as night approaches; while, though there are many fluctuations in the course of hydrocephalus, yet we observe no *definite* periods, at which the symptoms invariably remit, or are increased in severity.

It may further aid the diagnosis to remember the facts, that remittent fever is very rare at an earlier age than five years, and is scarcely ever met with in children under three; while at least the half of all cases of acute hydrocephalus occur in children who have not completed their fifth year.

You will find a good deal said by some writers on this disease, of morbid appearances found in other parts besides the brain, and especially in the abdominal organs,—enlargements of the liver, or spleen; inflammation of their peritoneal covering. I believe that careful investigation would generally detect tubercular deposit, in greater or less quantity, in various organs: in the lungs and bronchial glands, in the glands of the mesentery, and in the mucous follicles of the intestines. Sometimes there is tubercular ulceration of the bowels, which may produce diarrhoea, and so far tend to perplex the diagnosis. One remarkable change is said to be common, viz., intussusception of the small intestines. This probably takes place a short time only before death, and appears to be the result of spasmodic or irregular movements of the bowels, analogous to those which are observed in the voluntary muscles. The intussuscepted portions are easily pulled out, and show no marks of inflammation.

Many persons, as I have already hinted, lay great stress, when discussing the pathology of acute hydrocephalus, upon the previous unhealthy state of the nutritive apparatus. They hold that the primary disease—the *fons et origo mali*—lies in the stomach, or bowels, or liver; and that the brain affection is secondary, and caused by sympathy with these distant parts: and this opinion they fortify by referring to the frequency of organic disease, met with after death, in the abdominal viscera. In accordance with these views of its origin, they propose to cure, or to prevent, hydrocephalus, by redressing the faulty condition of the digestive organs.

Now this, in my judgment, is not only an erroneous, but an unsafe doctrine: for it tends to divert our attention from the head, and to misdirect the treatment. The grand predisposing cause of acute hydrocephalus is certainly the scrofulous diathesis, and this is why we see the complaint run so often in families: so that one child having died of that disorder affords much ground for apprehending that others belonging to the same family may become victims to it. The constitutional tendency is hereditary, and children born with it are liable and likely to have strumous disease set up in various organs at once, or perhaps in succession: not, however, a succession of cause and effect, but of common relation to one pervading disposition. We need not be surprised that scrofulous inflammation should affect the brain, and abdomen at the same time. When we find obvious organic disease of the brain, scrofulous tubercles for instance, which must have been antecedent to the hydrocephalus, it would be just as absurd to look to the abdomen for the cause of the hydrocephalus, as it would be to seek in the brain for an explanation of the cause of jaundice or of dysentery, when the liver or the colon were known to be diseased.

I do not mean to assert that the morbid conditions of the brain and of the abdomen are perfectly independent each of the other. The vomiting that is so constant a feature of acute hydrocephalus, the constipation that is so common a consequence of head affections, afford familiar evidence of the influence which cerebral disorders may exercise upon the abdominal functions. Conversely, any disease in other parts of the body may react injuriously upon the brain, and may sometimes be regarded as an exciting cause of disease in that organ.

The period of life is also a strong predisposing circumstance; acute hydrocephalus being peculiarly a disease of childhood. It is not, however, as I once erroneously believed, most frequent in very early infancy. In five only of forty fatal cases, in which Dr. West had the opportunity of confirming his diagnosis by an examination of the dead body, were the patients under a year old. Nine others were under three years of age; twenty between three and six; five between six and nine; and one between ten and eleven years old. The disease may indeed occur at any age up to the twelfth or fourteenth year. After that period it is comparatively rare.

Do we not trace, in this statement, the same protective influence of the period of lactation, which I formerly mentioned as being so conspicuous in the analogous disorder, strumous ophthalmia?

Acute hydrocephalus usually runs its course in from two to three weeks. It may destroy life within four or five days. It may be protracted, and still prove fatal, to the fourth week of the declared disease.

Whatever tends to deepen and aggravate the scrofulous *diathesis*—improper or insufficient nutriment, exposure to cold, inadequate clothing, impure air—may be regarded as a *predisposing* cause of acute hydrocephalus. And whatever tends to call scrofulous *disease* into action, may be reckoned among the possible *exciting* causes of acute hydrocephalus. Any general irritation may bring it on. It sometimes supervenes upon the drying up or repression of eruptions, as tinea capitis, or sores behind the ears. Such eruptions, therefore, occurring in strumous children, we must not attempt to cure suddenly; and free purging should be employed when they begin to disappear. The irritation produced by difficult and painful dentition is a very frequent exciting cause; and this is a source of danger which, in many cases, may be obviated by timely and judicious management. Violent heating exercise has sometimes, apparently, kindled the cerebral inflammation. Among the exciting causes we may place all physical injuries which jar or stun the brain; blows on the head, falls from a height, although the head may not be the part struck; and all moral agencies which shock or strongly disturb the nervous system; severe bodily pain, violent fits of anger, sudden fright. Gölis goes even so far as to say that great terror and distress of mind *in the mother* during the latter months of pregnancy may lead to the occurrence of acute hydrocephalus in the child; and he brings forward this curious fact in support of his opinion:—A large proportion of the children that were born in Vienna soon after the bombardment of that place by the French, in 1809, were seized with convulsions within a month after their birth, and died of inflammation within the cranium; effusion of coagulable lymph between the membranes, and of serum in the ventricles, being discovered on dissection.

LECTURE XXVI.

Acute Hydrocephalus continued. Prognosis and Mortality of the Disease. Treatment; Blood-letting; Purgatives; Cold; Mercury; Blisters. Prophylaxis. Spurious Hydrocephalus. Chronic Hydrocephalus, or Dropsy of the Brain. Shape of the Head and Face. Anatomical Conditions. Symptoms.

THE disease, of which I described the symptoms in the last lecture, acute hydrocephalus, is a very dangerous disease: and when once it is fairly established, most of the patients die: very few of them recover. Our chance of saving life, by appropriate treatment, is always greater in proportion as the complaint, or the tendency to the complaint, is detected *early*; and for that reason the precursory symptoms possess so high an importance.

When our treatment commences while the symptoms are as yet rather those of the precursory state, than of the confirmed disease, it is impossible to say how many of

those cases which, under such treatment, terminate favourably, would otherwise have ripened into well-marked hydrocephalus; and we must be content to have it said, without its being possible for us to refute the assertion, that not all of the disorders which we treat as acute hydrocephalus are really instances of that complaint. We must act upon the worst supposition, and not wait until the nature of the symptoms *demonstrate* that the malady is present, while they demonstrate also, at the same time, that it is well nigh hopeless. These are cases which peculiarly demand decision on the part of the medical man; and we are bound to act, in some instances, upon very slight indications; as when, for example, we perceive what we think threatenings of acute hydrocephalus in a scrofulous child, or in a child belonging to a family in which others have already been cut off by that disorder.

It has been supposed by some, that the case is hopeless after effusion has taken place, but we cannot be sure of that; nay more, there are no symptoms by which we can ever tell *for certain* that effusion *has* taken place.

I remember to have heard it gravely maintained, in the debating societies which I sometimes attended when a student, that there are no such things as absorbents, and no absorption, in the brain; and therefore that perfect recovery from serous effusion in that organ is impossible.

[There is no evidence that the effusion within the cranium, which is often to a comparatively small extent, is the cause of danger in hydrocephalus, or even of the symptoms which mark the latter stage of the disease.—C.]

But this notion is refuted by plain and well-known facts. We shall see hereafter, that blood poured forth within the nervous pulp is capable of being removed by absorption. How an opinion so palpably erroneous could ever have found credit, except with that class of men who can or will believe nothing which they cannot see, I am at loss to guess.

The prognosis, always doubtful or bad, is a little better when the disease is violent, and occurs in tolerably healthy subjects, than when it creeps on slowly and insidiously, and in weakly scrofulous patients. In the former case there is more room for the adoption of active measures; and the disease is more likely to be amenable to remedies, and less likely to be obstinate; it is also less likely to depend upon a permanent cause, such as the existence of scrofulous tubercles in the brain. We may permit ourselves to take hope from the very uncertainty of our means of diagnosis; hope, that the disease may be simple inflammation, independent of scrofula; a slender hope even then.

The probable issue of the disease is often judged of by the state of the pulse. The quick pulse belonging to the early stages of the disease will become slow; but it may become slow in two very different ways; it may diminish in frequency in a gradual and moderate manner, and then we may hope that the alteration proceeds from the progressive declension of the fever: or it may drop suddenly, which would be a reason for our fearing that the second stage of the disease was about to establish itself. We must take care, under the former circumstances, not prematurely to assert that the disorder is on the decline, and the patient safe. On the other hand, if the pulse have been morbidly slow, a gradual and slight increase in its frequency must be considered as a favourable omen; while its rapid and great acceleration would show that the disease was passing into its worst and final stage.

I have already cautioned you against being misled by that deceitful truce, and apparent improvement, which is apt to take place in the course of the disease. If the signs of amendment continue, or make progress, during two or three entire days, we may venture to admit a little more hope. But the patient can never be considered secure while any approach to what are thought symptoms of effusion remains; while the pupil continues dilated, for example; or even so long as it does not contract briskly under a strong light.

The prognosis is especially bad when acute hydrocephalus supervenes upon other disease; or when it is engrafted (as it sometimes is) upon the chronic form of the disorder. It is very seldom that the acute form subsides into the chronic.

To show you, however, that we are warranted in the expectation of sometimes carrying our patient through this most perilous malady, I will mention a few statistical

facts that have been recorded in respect to its mortality. Dr. Odier, of Geneva, states that, upon an average, eighteen cases of acute hydrocephalus occur every year in that place; and of these, six get well; *i. e.*, the recoveries are to the deaths as one to two. Dr. Gölis, to whose work I referred in the last lecture, and who had the charge of a large institution for children in Vienna, gives an account of thirty-seven cases, out of which five recovered. He had seen, upon the whole, forty-one instances of recovery from acute hydrocephalus. Dr. Mills, who has also written on the disease, has narrated twenty-eight cases, all of which died but seven; and Mr. Bricheteau lost four out of eleven. Adding these together, and taking the average, we have seventy-six instances of the disease, and nineteen recoveries; exactly one in four. The cases in which recovery took place were mostly those in which antiphlogistic measures were adopted *early*; and I must confess my own suspicion that they were, most of them at least, cases of what I have called *simple* encephalitis, occurring in children previously strong and healthy.

If the "acute hydrocephalus" of the English, and the "tubercular meningitis" of the French writers, be allowed to be convertible terms, and to signify always the same disease, we must greatly circumscribe the hope which might otherwise be deducible from the preceding statement. Dr. West declares that he has never yet seen an instance of recovery from advanced hydrocephalus; that he has known but one in which the child got well after the disease was well marked, and the second stage had commenced; and that he had observed a favourable issue in a very few cases only, even though they came under treatment immediately upon the appearance of the premonitory symptoms of water on the brain. And he quotes from M. Guersant, of Paris, "who has probably had a larger experience than any other man now living in the management of children's diseases," the following discouraging testimony:—

"Tubercular meningitis may sometimes terminate by recovery in the first stage, though the nature of such cases is always more or less doubtful. In the second stage I have not seen one child recover out of a hundred: and even those who seemed to have recovered, have either sunk afterwards under a return of the disease in its acute form, or have died of phthisis. As to patients in whom the disease has reached the third stage, I have never seen them improve, even for a moment."

The *treatment* of acute hydrocephalus is difficult to conduct; and scarcely less difficult to describe and teach. The disease being essentially an inflammation, requires, in its earlier periods at least, the remedies of inflammation. But we must ever bear in mind that our patients are children; and, for the most part, weakly, because scrofulous children. Their time of life, and the presence of the strumous diathesis, both forbid that strenuous appliance of antiphlogistic remedies, which might be proper and necessary in adults of strong and healthy frame. We take our weapons, however, in either case, from the same armoury.

The only event of the inflammatory process compatible with the safety of the patient is resolution. To this end, therefore, must our efforts be earnestly directed. If the child be feverish, the pulse sharp, the head hot, the cheeks flushed, the pain severe, and if, moreover, the case be seen early, there need be no doubt about the propriety of abstracting blood. It is a matter of obvious importance to ascertain how far we may safely and beneficially carry this measure, in the diseases of infants. Dr. John Clarke, a physician of large experience (the elder brother of the present Sir Charles Clarke), found that very young children would very well bear the loss of blood, even to fainting, once or twice; but that their vital powers were apt to sink if the bleeding, to that extent, were oftener repeated. It is better, in my opinion, to apply leeches to these little patients, than to cut one of their veins. Dr. West wisely advises that the leeches should be applied to the crown of the head, rather than to the temples, where they dangle about the eyes and terrify the child; or than behind the ears, where they are liable to be rubbed off as he rolls his head from side to side. Recollect that, upon very young children, leeches produce an effect tantamount to that of venesection. Their bites bleed more freely than in grown persons, on account of the greater activity of the capillary circulation in children. No general rule can be prescribed in respect to the number of leeches to be used; three will take as much blood in one case as half a dozen in another; but assuming that one leech will, on an average, cause the discharge of one ounce of blood, we may apply three of them to a strong infant of six months, when the symptoms are violent. Of course the further

efflux of blood must be stopped if syncope occur. In older children the quantity of blood requisite to be taken will be somewhat larger; six ounces drawn from a vein is a full bleeding, I should say, for a child five or six years old. I mention these quantities as mere approximations, as guides to what you may expect to find practically needful: the true measure and test of salutary blood-letting being in this, as well as in other inflammations, the effect it has at the time. The first bleeding, in what manner soever the blood be taken, should be a sufficient one; should produce some decided and manifest impression. By attending to this rule you will break the force of the early disease more surely, and more safely too, than by drawing blood in frequent driblets; a mode of using the remedy calculated to subdue the patient rather than to overcome his malady. You must afterwards go on with the leeches to the head, or you must withhold them, according to the exigency of the particular case; according to the state of the pulse, the continuance or the cessation of the pain, the increase or diminution of the fever, the previous strength and condition of the child, and so forth. Very rarely, however, will any repetition of blood-letting, in any way, be advisable or safe. I must once more admonish you that, as you have to deal with scrofulous children, any *superfluous* removal of blood, the abstraction of more than is required for controlling the inflammation within the head, will be likely to prove injurious to the general system; and even dangerously to depress the vital power. After the occurrence of general convulsions, or the full formation of the comatose state, a further prosecution of the bleeding has sometimes been rapidly followed by death.

Next in rank and importance to bleeding, if not even before it, come *purgatives*. They are to be exhibited with the threefold view of correcting depraved secretions, of clearing the alimentary canal of its irritating contents, and above all, of deriving, as the phrase is, from the head: producing a discharge of the watery parts of the blood, and taking off the stress from the cerebral arteries. The best forms of purgative medicine to be used for these purposes with children, consist of calomel and jalap, or calomel and scammony; and if these do not act freely, senna and salts must be given in aid of them. I have already made you acquainted with Dr. Abercrombie's high estimate of the efficacy of purgatives in inflammation of the brain, whether in the child or in the adult. Dr. Whytt, again, states that he never saw even temporary relief of the symptoms produced by any other means than those which increased the evacuations. Purgatives are to be administered, therefore, at an early period. But sometimes the stomach is so irritable that it rejects them. A previous bleeding will often correct this; and it is no small part of the benefit derived from the abstraction of blood, that it prepares the way for the more effectual operation of aperients, and of mercury. A large clyster will often be of service, both in settling the stomach, and in procuring stools, when there is much vomiting, and a continual rejection of medicine given by the mouth. Dr. Cheyne mentions a form of medicine by which he sometimes succeeded in quieting the irritable stomach, and procuring evacuations: namely, a drachm or two of magnesia, saturated with lemon juice, to be given every two or three hours. Dr. West recommends half a scruple of nitre, with a drachm of Epsom salts, dissolved in some aromatic water, or in veal broth. You may sometimes get calomel and scammony, however, to remain on the stomach, when almost every other medicine is rejected. The purgative plan should be steadily persisted in for several days.

To show you how torpid the bowels are apt to be in this disease, and how difficult it sometimes is to procure evacuations from them, I may mention the following circumstances which I heard Dr. Alison relate as having occurred in the practice of his uncle, the late Dr. Gregory of Edinburgh. He had one patient who took 140 grains of calomel in the course of five days: yet his bowels were not relieved, till he had also taken two doses of jalap, the first of 30 and the second of 35 grains. In another case, a child of twenty-eight months took in nine days 350 grains of calomel (nearly 40 grains a day); and in six of these days, 136 grains of jalap (more than 20 grains a day): the effects were a gentle purging from the jalap, none from the previous calomel, and but slight salivation. The child recovered after having been nearly in a comatose state. Of course large doses of this kind are never to be given, until the inefficacy of smaller ones has been ascertained.

Cold applied to the head:—I have before given you examples of its power. It is

especially useful in the early periods of the disease, when there is much heat, and when evacuations have been obtained. I am doubtful about the propriety of keeping ice in contact with the surface of the head in very young children. It will in many cases be sufficient to lay a linen rag wet with cold water (or spirit and water, to promote evaporation), upon the child's head, taking care to renew it frequently, not merely as often as it becomes *dry*, but as often as it becomes *hot*. Dr. Darwall states that he has known cases, which seemed utterly hopeless, retrieved by letting water fall in a succession of small drops upon the scalp, and continuing it until the head no longer recovered its high temperature upon intermitting the dropping. I scarcely need say that under all circumstances it is expedient to keep the head somewhat elevated. The influence of this mode of applying cold to the head is increased, and perhaps rendered safer, by immersing the lower extremities of the patient at the same time in warm water.

Different opinions have been held in respect to the value of mercury in this disease. Knowing how powerful an influence it has in arresting inflammatory action, and that the inflammation in acute hydrocephalus often leaves behind it traces showing that it was of the adhesive kind, I should not omit giving mercury; but (as I stated when upon the subject of encephalitis) I should not give it with the direct object of affecting the gums, of producing ptyalism. I believe the evidence touching the efficacy of mercury carried to salivation in acute hydrocephalus is this:—that some few very desperate cases have got well, the improvement commencing at the time when the mercurial influence on the system was becoming apparent; and that in other cases, the occurrence of salivation has been followed by *no* alleviation of the symptoms, but the disease has run on, unchecked, to its fatal termination. In truth, it is a very difficult matter to salivate a child; there is a great reluctance in the system, at the earlier periods of life, to take on the specific mercurial action; and the disinclination seems peculiarly strong during the presence of this disease; and the younger the child, the more difficult is it to affect the gums. Perhaps this may be considered fortunate; for when salivation does take place in these little patients, it sometimes proceeds to an alarming extent. Dr. John Clarke, who employed calomel largely in a variety of diseases, never saw more than three instances in which salivation was produced in children under three years of age.

If you are desirous of taking the chance of benefit from the specific influence of mercury, you had better give calomel as a part of the purgative plan, and rub in some of the mercurial ointment; you had better do this than lock up the child's bowels by combining opium with the calomel; not to mention the injurious effects of opium upon young children in general, and in the early period of head affections in particular. The calomel should be given steadily, in equal doses, at equal intervals. Green evacuations from the bowels, resembling wet tea-leaves or chopped spinach, usually follow its continued administration: and this appearance (like the rising of the gums in adults) is generally regarded as a proof that the influence of the mineral is felt by the system, and that it is doing all the good of which it is capable.

Upon the whole, I believe it will be found that they who have had the most ample experience of this perilous disease, have ended with the conviction, that moderate local depletion, and the regulated exhibition of mercury in small quantities, afford, generally, a better chance of success than the large bleedings, and the full and frequent doses of calomel, which have sometimes been recommended.

Of blisters I may repeat the substance of what I stated when we were considering encephalitis. I should abstain from them at the commencement of the disease. Even when applied at a distance from the head, they are apt to prove a source of hurtful irritation in these young and susceptible subjects. But in the second stage of the malady, I believe blisters are often of good service. They may be applied to the nape of the neck, or, which is better, to the head itself: and several may be applied in succession; or the ulcerated surface may be kept open by the help of irritating ointment, such as the unguentum cantharidis, or the ceratum sabinae.

[Dr. Wood (*Prac. of Med.*) remarks: "One additional remedy should be employed in this form of meningitis, from its supposed influence over the scrofulous habit of body, and in the hope, that if it do not promote the absorption of the tuberculous matter, it may possibly prevent its deposition. I allude to iodine. I would commence

with it in such doses as the stomach of the child could bear, and continue it throughout the treatment. The iodide of potassium, or the compound solution of iodine (U. S. Ph.) should be employed. The iodide of mercury might, with great propriety, be substituted for the calomel at the stage at which it is desirable to aim at the mercurial impression; and, in this case, the other preparations of iodine should be abandoned."—C.]

These are the main remedies to which we trust in the treatment of acute hydrocephalus: bleeding, purgatives, cold, in the outset; mercury and blisters, of more equivocal efficacy than the former, in the more advanced stages of the disease. When there is much irritability towards the decline of the disorder, or in its latest period, opiates may cautiously be tried; they sometimes have appeared to be extremely beneficial: two or three grains of Dover's powder furnish a very eligible form of opiate in such cases.

I do not feel called upon to say anything, in addition to what I stated in a former lecture, about other remedies that have been proposed in acute hydrocephalus; digitalis, colchicum, squills, antimony. These may be useful, when they act as diuretics: but they have no specific virtue. I have told you the remedies which I believe to be the best; and which will save the patient, when judiciously used, if the case be within the compass of our cure; and you will do well to learn how to manage these powerful means. I am confident you will find *that* more to your purpose than trying now this and now the other remedy, because it is new, or because some persons tell you they have been wonderfully successful with it.

[When the disease has reached the paralytic stage, its fatal termination is usually supposed to be inevitable; but Dr. Christie, of Scotland, and Dr. Woniger, of Hamburg, have each recently reported a case, in which a cure was effected after paralysis had occurred, by the administration of iodine. Dr. Christie employed a solution, containing sixteen grains of iodide of potassium, and four grains of iodine to one ounce of water, given in the dose of a teaspoonful every four hours, at the same time that a weak ointment of the biniodide of mercury was rubbed upon the child's scalp. Dr. Woniger gave a solution of one drachm of iodide of potassium, dissolved in half an ounce of water, in the dose, at first, of forty, and subsequently of fifty drops every two hours. In the case of Dr. Christie, the first indication of improvement occurred in thirty-six hours after the employment of the iodine was commenced with, but in Dr. Woniger's case, not until after the end of seventy-two hours. In both the recovery is said to have been complete and permanent.—C.]

Let me say a word in reference to the *prevention* of this disease: concerning which your advice will be sure to be asked again and again. In families, in which acute hydrocephalus has occurred, or which show decided marks of the scrofulous diathesis, the earliest attention should be paid to any deviation from the healthy condition of any of the functions. Weaned children in such families should be kept upon a nourishing but light and unstimulating diet; consisting of well-dressed vegetables, farinaceous substances, and a moderate proportion of animal food. Particular care should be taken to keep the bowels regular; not that weakening purges should be given, but the bowels should be fairly relieved at least once every day. Any disturbance of the digestive organs should be immediately corrected; by antacids, laxatives, change of diet, and sometimes by mercurials, as the hydrargyrum cum cretâ. Such children should also, if possible, be brought up in the country, and freely exposed to mild and dry air; and in winter great care should be taken to have them sufficiently clothed. Exposure to the contagion of small-pox, measles, scarlet fever, or whooping cough, should be scrupulously guarded against. During the hazardous period of dentition, the state of the teeth and gums must be sedulously watched. There is good reason for believing that a seton or an issue in the neck or arm has been very serviceable in warding off and preventing attacks of the disease. Dr. Cheyne mentions some striking instances of the good effect of establishing an artificial irritation at some distance from the brain, when there has been a disposition to disease in that organ.

There is another caution, too, which you will often find reason for suggesting: and that is, not to press or encourage the development of the mental faculties in children

who are quick and intelligent beyond their years. Parents are apt to be proud of the early acquirements of their little ones: they are not aware that such precocity of the mind implies danger to the health of the body; and they provide them with instructors, and to a certain extent abridge their hours of exercise and amusement, that they may do justice to their cleverness. But it is our duty to admonish such parents of the risk they are thus running: to advise them to think only, for the present, of corroborating the corporal strength of the child; and to avoid over-cultivation of his intellect until this dangerous period of his existence is got over.

There is still one point remaining, and one of the utmost importance, in relation to the acute hydrocephalus of children. I told you in the last lecture, that in general the diagnosis was not very difficult. But there is a form of disorder very apt to be mistaken and treated for acute hydrocephalus, by those who are not forewarned; and one which may be rendered fatal, if the *remedies* of acute hydrocephalus be directed against it. Encephalitis, whether it occur in the child or in the adult, has its spurious double. As, in morals, every virtue has its corresponding vice, which apes its actions and assumes its garb, so it is also with many opposite bodily disorders: and it is of great moment that we should be capable of discerning the essential difference of character that lurks beneath external similarity of feature. It is a most curious, but unquestionable fact, that *anæmia* of the brain, a diminution of its natural supply of red blood, and exhaustion of the nervous power, will produce symptoms very much resembling those which result from the diametrically opposite condition. To excess of pressure on the one hand, and to defect of pressure or support on the other, there are many phenomena in common. If you pay no regard to the state of the general circulation, as indicated by the temperature and by the pulse, you will find the actual symptoms of syncope, and of apoplectic fulness, to be identically the same. When a human being bleeds to death,—as many do from wounds, from uterine hæmorrhage, and so on,—what do we see? Why the patients may have nervous delirium, become convulsed, and then insensible, with a wide and fixed pupil. The outward visible signs of concussion and of compression of the brain are very much alike. The vulgar always confound them, and are clamorous that a vein should be opened: a measure which would be proper and useful in the one case, but murderous in the other. It is the same with the functions of other parts: we have palpitation of the heart when that organ is insufficiently supplied with blood; palpitation when it is over-loaded: dyspnœa, or hurried breathing, when the lungs are congested; hurried breathing, when blood does not arrive in them plentifully enough. You must see that the importance of distinguishing between the causes of these analogous phenomena is immense. Several authors in modern times have noticed the condition of the brain to which I now wish you to attend, and which may be called *spurious hydrocephalus*. Dr. Marshall Hall, Dr. Abercrombie, and the late Dr. Gooch,—each of these three physicians appears to have discriminated, for himself, the spurious from the genuine disease; but their several accounts of it were made public in the order of time in which I have here mentioned their names. Dr. Gooch's Essay is entitled—“*Of some Symptoms in Children erroneously attributed to Congestion of the Brain.*” His description of the disorder in question is very graphic. It is chiefly indicated, he says, by heaviness of the head, and drowsiness. The age of the little patients whom he had seen so affected was from a few months to two or three years; they were generally small of their age, and of delicate health, or had been exposed to debilitating causes. The physician finds the child lying on its nurse's lap, unable or unwilling to raise its head: half asleep; one moment opening its eyes, and the next closing them again, with a remarkable expression of languor. The tongue is slightly white, the skin is not hot; at times the nurse remarks that it is colder than natural; in some instances there is now and then a slight and transient flush. In all the cases that Dr. Gooch saw, the bowels had been already disturbed by purgatives; the symptoms had invariably been attributed to congestion of the brain; and the remedies employed had been leeches and cold lotions to the head, and purgatives—especially calomel. Under this treatment the patients had gradually got worse, the languor had increased, the pulse become quicker and weaker, and at the end of a certain number of days the children had died. In two instances he had known coma to come on during the last few hours; stertorous breathing, and dilated and motionless pupils.

Dr. Hall describes a very similar set of symptoms: the face pale, the cheeks cool or cold, the eyelids half closed, the eyes unattracted by any object put before them, the pupils unmoved on the approach of light, the breathing irregular and suspirious, the voice husky. These symptoms are sometimes preceded by irritability, and a feeble attempt at reaction; in which case the diagnosis requires extreme care and circumspection. He attributes the disorder, which he calls the "*hydrocephaloid disease*," principally to exhaustion. In early infancy the exhaustion owes its origin chiefly to diarrhoea, or catharsis; in the later periods of infancy, to the loss of blood, with or without a relaxed condition of the bowels. The diarrhoea is often produced by improper food, and frequently succeeds weaning; or it results from the ill-timed administration of purgative medicine. The exhaustion from loss of blood generally follows the application of leeches, for some previous complaint — or for this very complaint itself, when incipient, and misunderstood.

I will take one of Dr. Gooch's cases in illustration, and give it you in his own words. "I was going out of town (he says) one afternoon, when a gentleman drove up to my door in a coach, and entreated me to go and see his child, which he said had something the matter with its head, and that the medical attendant of the family was in the house, and was just going to apply leeches. I went with him immediately, and when I entered the nursery I found a child ten months old, lying in its nurse's lap, exactly in the state which I have already described; the same unwillingness to hold its head up, the same drowsiness, languor, absence of heat and all symptoms of fever. The child was not small of its age, and had not been weak; but it had been *weaned* about two months, since which it had never thriven. The leeches had not been put on. I took the medical gentleman into another room, related the foregoing case (*i. e.*, a case in which a child had been leeches out of its life), and several similar to it, which had been treated in the same way, and had *died* in the same way. Then I related to him a similar case which I had seen in the neighbouring square, which had been treated with ammonia and decoction of bark, and good diet, and which had recovered; not slowly, so as to make it doubtful whether the treatment was the cause of the recovery, but so speedily that at a third visit I took my leave. He consented to postpone the leeches, and to pursue the plan which I recommended. We directed the gruel diet to be left off, and no other to be given than ass's milk, of which the child was to take at least a pint and a half, and at most a quart, in the twenty-four hours. Its medicine was ten minims of the aromatic spirit of ammonia in a small draught every four hours. When we met the next day the appearance of the child proved that our measures had been right; the nurse was walking about the nursery with it upright in her arms. It looked happy and laughing. The same plan was continued another day; the next day it was so well that I took my leave, merely directing the ammonia to be given at longer intervals, and thus gradually withdrawn; the ass's milk to be continued, which kept the bowels sufficiently open without aperient medicine." This case contains both a picture of the morbid state, and a summary account of the treatment it requires. Instead of the *sal volatile*, you may occasionally substitute with advantage from five to ten drops of brandy mixed with arrowroot. You are to restrain diarrhoea, if it exist; give the child plain nourishing diet — there is none so good for it as that furnished from a healthy mother's breast; caution the nurse or mother against raising it into the upright position; keep its extremities warm with flannel; and, if the season permit, let a current of mild fresh air blow freely over it.

Bear in mind, then, the distinctive characters of this spurious hydrocephalus — the pale, cool cheek; the half-shut, regardless eye; the insensible pupil; the interrupted, sighing respiration: and when the mere symptoms are more ambiguous, your judgment concerning the true nature of the case will be much aided by tracing the manner in which they came on, and the causes to which they seem to be attributable. In very young children — in respect to whom the question is most likely to arise — you may often determine between congestion and exhaustion, between fulness and emptiness, between too much and too little pressure, by a very simple and easy test, which is not adverted to, so far as I remember, by any of the three writers whom I have mentioned. I mean by taking notice of the state of the unclosed fontanelle. If the symptoms proceed from plethora, or inflammation, or an approach to inflammation, you will find the surface of the fontanelle convex and prominent, and you may

safely employ, and expect benefit from, depletion. If, on the other hand, the symptoms originate in emptiness and want of support, the surface of the fontanelle will be concave and depressed; and in that case leeches, or other evacnants, will do harm, and you must prescribe better diet, ammonia, and so forth.

All that has hitherto been said has reference to *acute* hydrocephalus, which is an *inflammation*. I have next to speak of *chronic* hydrocephalus, which is a *dropsy*. From some cause, not well understood, a watery fluid collects within the skull, most commonly in the ventricles of the brain; and this occurring at the earlier periods of life, before the whole of the brain-case has become solid, the containing parts yield to the increasing pressure, and the size of the head is augmented in various degrees; at the same time the cerebral functions are more or less deranged. This dropsy of the cranial cavity often commences before the period of intra-uterine life is completed, and the head of the fœtus becomes so large that it cannot pass with safety into the world. Accordingly, many of these infants perish at the moment when their separate existence commences: — *nascentes moriuntur*. The pressure of the maternal pelvis is fatal to them; or the diseased head bursts; or it is crushed by the accoucheur, to preserve the life of the mother. The skull is emptied of its contents, and the shell, if I may so call it, collapsing, passes through the natural outlets.

In many cases, however, the dropsical skull is expelled entire and unhurt, and the infant lives for a shorter or a longer period. Sometimes the fluid does not begin to accumulate till after birth: in a few days, however, or after some weeks, or some months even, the head is perceived to enlarge with a rapidity quite disproportioned to the growth of the other parts of the body; and enlarging, it becomes misshapen also. The intervention of the membranous partitions called fontanelles and open sutures, between the ununited bones, allows the centrifugal pressure of the gradually accumulating water to modify the shape of the head. These membranous interspaces are unnaturally wide, and more numerous than in healthy children. Nevertheless the process of ossification goes on, but the bones are extremely thin. We see little islands of bone in seas (as it were) of membrane. By degrees, if the child survive, the proportion of membrane to bone becomes less and less, and at length the whole brain-case is hard, and firmly closed up, its surface exhibiting an unusual number of joinings; there are many *ossa triquetra*.

In the meantime the direction and relations of the loose and yielding bones are altered. The os frontis is tilted forwards, so that the forehead, instead of receding a little, rises perpendicularly, or even juts out at its upper part, and overhangs the brow. The orbital plates of this bone are apt to be forced downwards, made to slant backwards, and flattened; sometimes they are rendered even convex towards the orbits. The parietal bones bulge above towards the sides; the occiput is pushed back; and the head becomes long, broad, and deep, but flattened on the top. This, at least, is the most ordinary result. In some instances, however, the skull rises up in a conical form, like a sugar-loaf. Not unfrequently the whole head is irregularly deformed, the two sides being unsymmetrical. Some of these rarer varieties of form are fixed and connate; others are owing, probably, to the kind of external pressure to which the head has been subjected.

While the *skull* may be rapidly enlarging, the bones of the *face* grow no faster than usual, perhaps not even so fast; and the disproportion that results gives an odd and peculiar physiognomy to the unhappy beings who are the subjects of this calamity. They have not the usual round or oval face of childhood. The forehead is broad, and the outline of the features tapers towards the chin. The visage is triangular. This great disproportion of size between the head and the face is diagnostic of the disease, and would serve to distinguish the skull of a hydrocephalic child from that of a giant. Heartless parents sometimes make a wretched profit of the deformity. A penny show of this kind existed very recently in the immediate vicinity of this College.

When, after death, we explore the physical causes of these singular deviations from the natural figure and bulk of the cranium, we find that they proceed from the pressure of accumulated water: the complaint is manifestly a dropsy. But the situation of the water, and the condition of the brain itself, are subject to some curious varieties.

In a certain number of cases the brain is incompletely formed; deficient in some of its parts, or even altogether wanting. That portion of the cranial cavity which should contain the nervous pulp is filled up by a thin pellucid fluid. From some unknown cause, operating during the period of intra-uterine life, the progressive formation of the brain has been arrested. Marks of imperfect development are often visible in other parts of the same infants; they have a hare-lip, a bifid spine, or a fissured palate. It is in cases of this kind generally that the skull, unnaturally small perhaps, is pinched up into a conical peak, and has considerable thickness. They are obviously hopeless cases. To the physiologist they are subjects of much interest; for the practical physician they have none.

FIG. 26.



A hydrocephalic skull from a girl aged 11 years: the enlargement of the skull is effected by its elongation, and by the depression and hollowing of its base. An increase of width appears to have been prevented by the premature and complete closure of the sagittal suture. The coronal suture, and that between the frontal and parietal bones and the suture, also, of the sphenoid, are wide open. The superior walls of the orbits are pressed downwards. The bones generally are thin and light. St. Bartholomew's Museum.

But in the majority of instances, when the infants survive their birth, the liquid is contained in the central cavities or ventricles of the brain, which are expanded into one. The convolutions are unfolded, and the cerebral matter is spread out into a hollow sphere; the irregularities of the surface have disappeared; the whole of the brain is smoothly extended in a thin layer, immediately beneath the bones and the membranes that connect them, and surrounds the enclosed liquid like a bag. Less frequently a different state of matters is seen. The liquid, instead of being included within the cerebral substance, lies in the cavity of the arachnoid, close to the dura mater; while the brain, perfect in all its essential parts, is at the bottom of the cavity. The difference, however, is more apparent than real: the two conditions are substantially the same, only that, in the one case, the solid parts that lie around the ventricles gradually expand as the fluid slowly collects, much as an air-balloon dilates in proportion as gas is introduced within it; while in the other case the seams or commissures (as they are technically called), that unite the hemispheres of the brain, give way, or are deficient, so that the ventricles, and the general sac of the arachnoid form together one huge cavity; the hemispheres are turned aside, or folded back, the surfaces that naturally have a *central* aspect look *upwards*, and seem to constitute the summit of the cerebrum. This was the state of the parts within the immense skull from which the largest of the casts before you was taken. It belonged to a man named Cardinal, who died in Guy's Hospital, in 1825, and of whom Dr. Bright has given a very interesting account.

Now some of the consequences of this distension of the brain and skull with watery fluid are simply mechanical. The child is top-heavy. His large unwieldy head is too much for the muscles of his neck to sustain without fatigue; or even, when they are unassisted, to sustain at all. He walks gently and carefully, like a person balancing a heavy load upon his head; or he holds and partly carries his head with his hands, as a milkmaid steadies and supports her pail; or he reclines the weight of his burden upon the chair, or table, as he sits.

But far more important effects of the disease are those which relate to the three great functions of the brain. The child is soon found to be blind; or, what however is less common, deaf; or palsied in one or more of its limbs; or idiotic; or all these. In other words, the special senses, the power of voluntary motion, and the mental faculties, are apt to be defective or perverted. Instances, however, do occur, in which these functions are, for some time, but little deranged. The greater number of those who are afflicted with dropsy of the brain either recover, or die during their infancy. Still, a few survive, bearing their complaint to the adult period, and even to old age; and in some of these individuals, who, with excessively large heads, have yet numbered many years of existence, the intellect and the senses, if not entire and

perfect, have been sufficiently effective to answer the common wants and purposes of social life: the moral emotions strong, the feelings lively and correct, the memory tolerably retentive, the reasoning powers respectable. Dr. David Monro relates the case of a hydrocephalic girl, six years old, whose head measured two feet four inches in circumference. She is described by him as being "as lively and sensible as most of her age," and as "having a strong memory." Dr. Bright's patient, Cardinal, was nearly thirty years of age when he died. He was born in 1795. At the time of his birth, his head was only a little larger than natural; but it had a pulpy feel, as if it were almost destitute of bony matter. A fortnight afterwards, it began to increase rapidly; and when he was five years old, it was but little less, according to his mother's account, than when he died. He could not walk alone until he was nearly six, and then only on level ground. If he attempted to run, or to stoop, he fell down. He was sent to school when he was about six, and soon learned to read well and to write tolerably; but writing he soon gave up, because, as he was near-sighted, it obliged him to stoop, which he could not conveniently do. When a candle was held behind his head, or when his head happened to be between a spectator and the sun, the cranium appeared semi-transparent; and this was more or less the case till he was fourteen years old. About the age of twenty-three, epileptic fits began to occur; and after that his health, which previously had been very good, failed somewhat. The ossification of the skull was not complete till two years before his death, the anterior fontanelle being the last part that closed. It has been mentioned that he was near-sighted; but he was very quick of hearing, his taste was perfect, and his digestion good. Dr. Bright states that his mental faculties were very fair, and his memory tolerable; but it was not retentive of dates. It was said that he was never known to dream. There was something childish and irritable in his manner, and he was easily provoked. He died, at last, of fever and diarrhœa. There were seven or eight pints of fluid within the cranium, in contact with the dura mater. On the base, or floor, of the skull lay the brain, with its hemispheres opened outwards, like the leaves of a book.

How comes it that the cerebral functions are thus sometimes fulfilled, or go on so well, when the machinery through which the mental powers are manifested—the instrument whereon and whereby the immaterial principle mysteriously operates—is so palpably and greatly deranged? How comes it that life, and especially the life of the mind, subsists at all? These questions open very interesting considerations. It would appear, from such cases as I have been referring to, that the curious arrangement and collocation of the several parts of the brain are rather matters of convenient package than of necessary relation. The pulp which furnishes the medium of sense, and thought, and volition, is there, but it is disposed in an unusual shape. In neither of the two varieties of the malady that have been described as being compatible with prolonged existence, is there any necessary diminution of the cerebral mass. The brain itself, which forms a bag in the one case, and is split in halves in the other, has been found to weigh quite as much as a healthy brain at the same period of life. There has been no loss, therefore, of substance; the pressure has been gradual, and it has not been made to act injuriously by counter-pressure; no effectual resistance has been afforded by the rigidity of the brain-case: and thus the unopposed distending force neither causes absorption of the cerebral pulp on the one hand, nor, on the other, induces coma, or convulsions, or idiotcy, by its compression.

Most commonly, however, the mental and voluntary functions are maimed or perverted; and these serious calamities make parents look at a large head in a young child with anxious solicitude. But you are aware, after what I stated on this subject in the last lecture, that the head *may* be extravagantly large without dropsy of the brain, and without disease.

We have just seen, that while the brain itself is gradually unfolded, or its hemispheres are parted and turned aside, by the liquid accumulating within the cranium, the functions of the organ may suffer but little, so long as the yielding brain-case permits the expansion or separation of the nervous substance, without inordinate pressure. But as soon as undue pressure begins to be exercised, then morbid symptoms arise, or the defects that have previously shown themselves are aggravated. Hence that period of life becomes a perilous period, at which the skull, by the closure of its fontanelles and sutures, loses its capability of further expansion. In some rare

cases the closed sutures re-open under the augmenting pressure, and a respite is thus obtained. Dr. Baillie has recorded an instance in which this happened in a boy seven years old. A similar case is mentioned in Dr. Yeats' work on hydrocephalus. The patient was a boy nine years of age. The sutures of his skull separated again after having been united; and it was remarked that the teeth in the jagged edges, whereby the bones interlock with each other, were much fewer than is usual. If this be always so when the sutures give way, it will serve to facilitate our understanding how such a separation can take place. The skull may, however, go on expanding, although the sutures are permanently closed; there still being left intervals between the several points of ossification, which intervals are covered by membrane only. The beautiful preparation on the table, showing this remarkable state of the cranium, I have borrowed for your inspection from Dr. Sweatman's museum.

Indeed, although I have spoken of this complaint as being especially a disease of childhood, it does occasionally commence long after the skull has become a complete case of bone. Enlargement of the head, in these cases, is impossible; but this circumstance, and the symptoms it is apt mechanically to produce, form the only differences between the disorder as it affects the child and the adult. In both cases disturbance of the cerebral functions arises, and at length convulsions or coma close the scene. In both, a dropsical state of the ventricles of the brain constitutes, often, the only morbid change presented after death. A young and distinguished lawyer of my acquaintance had one or two attacks of rather sudden loss of consciousness, while engaged in the Court of Chancery; by degrees he became dull, stupid, forgetful, and, at length, insensible. In this condition he died. A large quantity of serous liquid was found distending the ventricles of his brain. No other alteration could be detected.

Dr. Baillie describes a case of chronic hydrocephalus that occurred in a man fifty years old. Six ounces of fluid were contained in the lateral ventricles. He had been paralytic on the right side of the body; and for eleven months before his death had lost the recollection of his own language, with the exception of four or five words; which he employed, with different intonations, to express his various wants.

The celebrated Dean of St. Patrick's afforded another instance of the same disease, attended with a similar interruption of the power of discoursing. The case, as related in Sir Walter Scott's *Life of Dean Swift*, is curious, and contains an early suggestion of a piece of practice which in our own time has met with more favour. "A few days afterwards he sunk into a state of total insensibility, slept much, and could not without great difficulty be prevailed on to walk across the room. This was the effect of another bodily disease, his brain being loaded with water. Mr. Stevens, an ingenious clergyman of his chapter, pronounced this to be the case during his illness, and upon opening his head it appeared that he was not mistaken; but though he often entreated the Dean's friends and physicians that his skull might be trepanned, and the water discharged, no regard was paid to his opinion or advice."

He remained from October, 1742, to October, 1745, in a state of silence, with few and slight exceptions; and died in the 78th year of his age.

Gölis also mentions three instances in which this disease began in advanced life; two of the patients were above seventy years old; the third, who was a physician at Vienna, likewise died in the decline of life, having suffered under the disorder for ten years.

Now, what can we do in these wretched cases? Seldom much good, I am afraid. Yet something we must try, for parents will flatter themselves with hopes of a cure: and to say the truth, there have been, under judicious management, a sufficient number of recoveries to forbid our despairing in any case, and to make it incumbent upon us to employ carefully all those measures which have occasionally brought the disease to a favourable termination. Gölis even affirms, that of the cases which began after birth, and which he saw and treated early, he was fortunate enough to save the majority.

LECTURE XXVII.

Treatment of Chronic Hydrocephalus; Internal Remedies: Mechanical Expedients; Bandages, Tapping. Symptoms of Spinal Disease. Inflammatory conditions of the Spinal Marrow.

THE cure of chronic hydrocephalus may be attempted by internal remedies, or by external mechanical expedients, or by both.

The internal remedies by which most good appears to have been effected, and from which, therefore, most is to be hoped, are diuretics, purgatives, and above all, mercury, which is believed by many to have a special and powerful influence in promoting absorption. Conjointly with these, the abstraction of small quantities of blood from the head, by means of leeches, has been found beneficial.

Gölis advises that calomel should be given in half-grain doses, twice a day; or, if that quantity should purge too much, in doses containing only one-fourth of a grain. At the same time he would rub a scruple or two of mercurial ointment, mixed with ointment of juniper berries, upon the scalp, every night. He recommends that the head should be kept constantly covered also by a woollen cap. Infants require, he says, no other nutriment than good breast-milk; while older patients should take a moderate quantity of meat. In mild weather they should be as much as possible in the open air. Under this plan of treatment he affirms that he has known the circumference of the head decrease by half an inch or an inch, in a period of six weeks or three months; and that perseverance in this method has frequently, in his experience, been followed by perfect recovery, both of the mental and of the bodily powers. If no improvement should be perceptible in two months, he advises that diuretics should be given, *with* the former remedies, the acetate of potash, or squills, or both: that an issue should be made in the neck, or in each arm, and be kept discharging for several months. And he thinks that when convalescence has once begun, it may often be much accelerated by minute doses of quina; the fourth of a grain, for example, thrice daily.

In a disease so unpromising as chronic hydrocephalus, we are warranted in trying any plan that has been found, or supposed, to be useful. An apothecary of considerable experience—now dead—once took the pains to write out and send me the particulars of two cases in which he had seen a peculiar mode of administering mercury successful. I will give you them nearly in his own words.

In the year 1817, he had under his care a lad, named Scott, labouring under chronic hydrocephalus. He had been ill two or three years, was nearly blind, had very little use of his lower extremities, and could not walk across the room without support. He suffered violent pains in his head, and was unable to bear the least pressure on his scalp. His bowels were constipated, and his pulse “oppressed.” Cupping and blistering, the blue pill, drastic purgatives, and ordinary diuretics, tried in combination and succession, gave him temporary relief; but no permanent benefit was obtained. Dr. Gower then suggested a plan which he had himself found successful in such cases, and which had first been used by Dr. Carmichael Smith, who had recorded ten cases of recovery under its adoption. Dr. Gower’s plan was to rub down ten grains of crude mercury with about a scruple of manna, and five grains of *fresh* squills: this was to be one dose: and it was to be repeated every eight hours.

My informant rubbed the quicksilver down with conserve of roses, and then added the fresh squills, making the whole into the consistence proper for pills with liquorice powder. The patient took this dose three times a day, for nearly three weeks, without any pytalism being produced. Its effects were great prostration of strength, and loss of flesh, with gradual relief of all the boy’s sufferings. It operated profusely by the kidneys. The medicine was continued twice a day, and at length once, for another fortnight; when all the symptoms of the disease had disappeared. The boy was greatly emaciated. He was then ordered an ounce and a half of Griffith’s mix-

ture thrice daily; and soon regained his health and strength, and got quite well. He remained well eight years afterwards.

The success obtained in this case led to the pursuance of a similar course in that of the son of a well-known fish-monger in Old Bond Street. He was about twelve years old, and afflicted in nearly the same manner as Scott, except that the pain in his head was more acute, and caused violent screaming: *relief* had been repeatedly given, for a time, by cupping. The physician in attendance was unwilling to try the plan, when it was proposed to him, but said that he would give what was equivalent—small doses of blue pill, with squills in powder. The result was salivation in a few days, without any amendment. In about three weeks, the local effects of the mercury having subsided, and the patient then suffering extreme pain in the head, loss of sight, and want of power over the lower extremities, my informant was desired to adopt any measures he thought fitting. The medicine was given as in the former case, and with the same happy consequence. It acted, as before, without producing ptialism, but with a great reduction of strength and flesh. Health was restored by steel, after the symptoms of hydrocephalus had disappeared. This cure also was permanent.

I think you will give me credit for not being over fond of recommending what may be called *conundrums*, instead of well-tryed and approved means of cure; but I say that in such a complaint as chronic hydrocephalus, we have generally the opportunity of testing the virtues of many reputed remedies, one after another; and we are not to despise or neglect any measures that have been found beneficial, merely because they are out of the way, or because we cannot see in what manner they can excel the more common formulæ.

You will observe that these were cases in which the disease came on some time after the sutures of the skull had closed.

[Dr. Hannay relates a case of chronic hydrocephalus, in the Edinburgh Med. and Surg. Journal, in which he attributes the recovery of the patient mainly to the application to the scalp of a liniment composed of powdered ipecacuanha and olive oil, each two drachms, and half an ounce of suet. Dr. Hannay remarks, that the application of this liniment, three or four times a day, is followed in about thirty-six hours, by a papular and vesicular eruption; and he is of the opinion, that as chronic hydrocephalus often succeeds to the suppression of eruptions on the scalp, the use of this counter-irritant will prove in many cases extremely useful—its effects are much more manageable than those of the tartar emetic ointment, which, in this disease, has been found advantageous. — C.]

The mechanical remedies of chronic hydrocephalus are two: and they have a totally opposite mode of action. By the one, the brain is compressed; by the other, it is lightened of its pressure: yet both of them have proved successful. What does this show? what, but a confirmation of the doctrine that there are different states of the encephalon, very dissimilar in their essential character, yet having some symptoms in common; and those the most likely of all to catch our attention? Such common symptoms resemble an algebraical symbol, which derives its value from the plus or minus sign prefixed. Surely it is of vital importance to study, and if we can to settle, the differences whereby these inverse conditions, requiring contrary remedies, may be discriminated.

Bandaging the head is one of these two expedients; *puncturing* it the other. Neither of them is practically applicable after the bones of the skull have united.

Bandages appear to have been suggested by the notion that the increase of the fluid within the head, and probably some of the symptoms too, might depend, more or less, upon the want of firmness and proper resistance in the outer containing parts; in the feeble and half solid skull. A certain amount of support and pressure is requisite for the due exercise of the cerebral functions. Beyond this amount all increase of pressure is hurtful. The middle point of safety it may be hard to hit. It is certain that the easy yielding of the bony walls of the head, by reason of the membranous interspaces that exist in the early periods of life, proves oftentimes the safety of these patients. If the skull did not expand as the water gathered, morbid symptoms would ensue. Great nicety must therefore be requisite in the use of this

remedy. While the head is palpably enlarging, compression by means of plasters or bandages would probably be mischievous. When the disease is stationary, and the unconnected bones of the skull are loose and fluctuating, and the child is pale and languid, much benefit may be expected from moderate and well-regulated support. The late Sir Gilbert Blane was the first, I believe, to suggest this mode of treatment; but its safety and efficacy have been more recently demonstrated by Mr. Barnard, who has related several examples of complete success from the employment of bandages. In these cases the children were pale, bloated, and feeble, with flabby muscles; the bones of their heads were moveable and floating, and the functions of the brain more or less impaired. Mr. Barnard applies strips of adhesive plaster, about three-quarters of an inch wide, completely round the head from before backwards; covering the forehead from the eyebrows to the hair of the head, as low down on the sides as the ears will permit, and lapping over each other behind. Then, cross-strips are carried from one side of the head to the other, over the crown; and lastly, one long slip, reaching from the forehead, within half an inch of the root of the nose, over the vertex to the nape of the neck. In his first trial of this plan, but never afterwards, Mr. Barnard laid pieces of linen, wetted with cold water, over the plasters. The only internal medicine given was castor-oil, to regulate the bowels. The effects, in all these gentleman's cases, were these: a gradual diminution of the size of the head; mitigation, and ultimate disappearance, of all head symptoms, such as strabismus, rolling of the eyes, starting of the muscles, and convulsions: and at the same time, increased tone of the muscular system, with an improved appearance of the skin, and of the secretions from the bowels. These are striking results. They show that, in certain conditions of chronic hydrocephalus, a part of the danger arises from a lack of due support and confinement of the brain; and they prove that compression alone may be equal to the cure. To such cases, Dr. Arnott's air-press would seem, from the facility with which its equable compressing force may be regulated, to be especially adapted.

But in children who are not of this pale and feeble habit, and in whom ossification of the skull goes on, the period when the walls cease to yield is the period of danger. The water continuing to accumulate, inordinate pressure begins to take place. Under these circumstances, the application of bandages or plasters must, if nothing else be done, be insufficient or unsafe. The brain-case being no longer capable of expansion, there remains to be attempted a reduction of the quantity of the liquid which it contains.

Now, any considerable diminution of the accumulated fluid, through the agency of mere absorption, is scarcely to be expected; even although we endeavour to aid that process by applying leeches and cold to the head, and by purgatives, or diuretics, or diaphoretics. Some mode, more certain and effectual, of emptying the distended cavity, has therefore been earnestly sought after; and the second mechanical expedient of which I have spoken offers a very sure method of attaining this object. He must have been a bold physician who first proposed to decant the water from the brain, by means of a perforation, made with a trocar, through the membrane of the fontanelle, through the membranes of the brain, and through even the expanded cerebral substance itself. But the success of the project has amply vindicated his happy audacity. It is not a very new suggestion, but it has received particular attention in this country of late years; and though tapping the brain in chronic hydrocephalus has been denounced as useless and cruel by some high continental authorities, by Gölis and Richter especially, it furnishes one of the best of the few chances of safety to the patient. Of course I mean ultimate safety, for the operation itself is attended with the present risk of accelerating the patient's death. Other means, however, failing, we are justified in advising that hazard. We have to consider, that by performing the operation, we incur the danger of abbreviating the existence of a being, whose life, without it, could scarcely be long continued, or capable of enjoyment: but then we afford *some* chance of a perfect cure. A speedy death, or an uncertain life of mental and bodily imbecility, or complete restoration: these are the three events to be looked at. Of the three, the second is, in my judgment, incomparably the worst; and if the case were my own, if I had to decide the painful question in reference to one of my own children, I would accept the alternative of probable speedy death on the one hand; possible complete recovery on the other.

To say the truth, the *immediate* danger is not so very great as you might suppose; provided that the operation be skilfully and cautiously performed, and only a moderate quantity of water drawn off at a time. That even a very rough operation is not necessarily fatal we learn from a singular case related by Mr. Greatwood. A child, fifteen months old, afflicted with chronic hydrocephalus, fell down, and struck the back part of its head against a nail, which penetrated the skull. Above three pints of water gradually flowed out at the orifice thus made, and the child was cured.

In some rare cases the imprisoned liquid has found a natural vent, and dribbled away, through foramina in the bones of the skull communicating with the nostrils. In this manner injurious pressure has been relieved, or for a while staved off.

I will mention a few instances in which tapping the brain has been performed; for I know no better mode of showing you the manner in which the operation should be done, the cautions to be attended to in doing it, and what kind of success it has had.

There is an account of the performance of this operation by Leeat, in the *Philosophical Transactions* for the year 1751. This date is subsequent to the period when the Rev. Mr. Stevens suggested the propriety of trepanning Dean Swift's cranium. In 1778, Dr. Remmet, of Plymouth, punctured the head of a hydrocephalic child on five several occasions, with a lancet, and took away, in all, no less than eighty ounces of fluid; five pints, as pints were measured in that day. The child died seventeen days after the last tapping. A very interesting case of the same kind is related by Dr. Vose, of Liverpool. His patient was an infant seven months old. Its head was more than twice the ordinary size. Three operations were performed; the first with a couching needle. Upwards of three ounces were on that occasion evacuated; and it was estimated that about the same quantity dribbled away afterwards. The child thereupon became very weak, but was presently revived by some cordial medicine. About six weeks afterwards, the liquid having collected again, an opening was made with a bistoury, and eight ounces were removed; and nine days after that, twelve ounces more, without any bad consequences. The head diminished in size, the patient got apparently well, and the case was published as a successful one. Unfortunately, however, upon the closure of the sutures by ossification, the complaint returned, and the child died of it.

Mr. Lizars, of Edinburgh, operated upon a little patient of his twenty times in the course of three months; using a small trocar. Dilatation of the pupils, and squinting, which had previously existed, ceased immediately upon the escape of the water. The child recovered. But in this case also, as in Dr. Vose's, the success was temporary only. The head at the period of teething again enlarged, and again the tapping was performed; but the little patient sunk. Another very striking and instructive instance is recorded by Mr. Russell, of Edinburgh. The patient was an infant three months old, with an enormous head; twenty-three inches in circumference, and fifteen inches and a half from one ear to the other. The child was affected with strabismus, and a perpetual rolling of the eyes. The usual routine measures, compression among the rest, had been employed without any success. By four operations, performed at intervals of about ten days, the size of the head was considerably reduced; but, the fluid continuing to collect, calomel was given in small and frequent doses, and the gums became sore, and the child got well. At eight months old the dimensions of the head were less, by four inches in circumference, and by two inches and a half across the vertex, than they had been before the first tapping; and the sutures had entirely closed.

But Dr. Conquest, of Finsbury Square, has, more than any other person, given authority to these operations. In a paper published in the *Medical Gazette*, in March, 1838, he tells us that he had then tapped the heads of nineteen children for this complaint, and in ten of the nineteen cases the children survived. Very little is known of the subsequent fate of these ten cases. Of the condition of three only, at a later period, is there any record. Not one of the three was in a very satisfactory state. Dr. Conquest introduces a small trocar through the coronal suture below the anterior fontanelle, and cautiously makes a pressure upon the head afterwards by means of strips of adhesive plaster; and he closes the wound in the integuments carefully after each time of puncturing. The greatest quantity of liquid withdrawn by him, at any one time, has been twenty ounces and a half; and the greatest number of operations on any one child has been five, performed at intervals of from two to six

weeks. The largest total quantity of water removed was fifty-seven or fifty-eight ounces, by five successiveappings.

This expedient, though doubtless hazardous, must be deemed to possess a certain value. The rules relating to its performance may be briefly summed up. The operation should scarcely be had recourse to until other means have failed. The trocar should be small, and it should be introduced perpendicularly to the surface, at the edge of the anterior fontanelle; so as to be as much as possible out of the way of the longitudinal sinus, and of the great veins that empty themselves therein. The fluid should be allowed to issue very slowly; and a part only of it should be evacuated at once. The instant that the pulse becomes weak, or the dilated pupil contracts, or the expression of the child's countenance manifestly alters, the canula should be withdrawn, and the aperture in the skull closed. Gentle compression should be carefully made to compensate, in some degree at least, the pressure that has been removed with the fluid. Should the infant become pale and faint, it must be placed in the horizontal posture; and a few drops of sal volatile, or of brandy, mixed with water, may be given. Sometimes slight inflammatory action comes on in the course of a day or two after the tapping. When this happens, we must apply cold lotions, or leeches, and use the other remedies which I mentioned before, as proper to subdue such inflammation.

It has been thought that the operation is more likely to succeed in the rarer case of arachnoidean than in the more common case of ventricular hydrocephalus. But supposing this to be so (which, however, is very questionable) how are we to discriminate between these two conditions?

Partly, we are told, by the character of the accumulated fluid; and this may be ascertained by introducing a grooved needle through the membrane. Serous fluid in the cavity of the arachnoid is sometimes a consequence of a previous extravasation of blood in the same part; and, thus arising, it is apt to be tinged with blood and to contain a sensible proportion of albumen. Whereas in dropsy of the ventricles the liquid is almost as limpid as water, and holds little or no albumen in solution.

This criterion cannot, however, be implicitly relied on. The liquid withdrawn from the cerebral ventricles is sometimes red and albuminous.

Partly, again, we may judge by the depth to which the needle penetrates before reaching the fluid. But this, also, is an ambiguous test, for not seldom the liquid in the distended ventricles comes so near to the surface as to seem to be immediately beneath the dura mater.

There is one positive indication, when it occurs, that the dropsy is ventricular, which I have learned from the comprehensive lectures on this subject, just delivered before the College of Surgeons by Professor Prescott Hewett. He points out, what I mentioned a few minutes since, the effect of the pressure of the fluid collected within the skull, in flattening or pushing outwards the orbital plates of the frontal bone. This happens in ventricular hydrocephalus alone; but it does not always happen. Now this change in the orbital plates narrows the dimensions of the orbits, thrusts the eyeballs more or less out of their sockets, and gives them a downward direction; so that a great part of the pupil is hidden behind the lower lid, and the white of the eye is more uncovered and visible than is usual. When this condition of the eyeballs is noticeable in a hydrocephalic patient, you may be sure that the dropsy is ventricular. But there may be ventricular hydrocephalus without this outward token of its situation. The orbital plates are not always warped. Whether they are so or not will depend upon circumstances, and one of the determining circumstances probably is the period at which the dropsical accumulation and pressure commence.

I once got a surgeon to perform the operation of tapping upon the infant of a poor woman, after I had tried in vain all the other measures that I have spoken of. To our horror, when the trocar was withdrawn from the canula, instead of clear serosity, a fine stream of purple blood spouted forth. The opening was at a considerable distance from the longitudinal sinus; but the trocar was not so delicate as it might have been, and I presume that one of the larger superficial veins had been pierced. I do not think, either, that the instrument was introduced in a sufficiently perpendicular direction. Of course the risk of hitting a vein is increased when the trocar is carried obliquely inwards: and a large portion of the cerebral mass is also wounded.

We naturally thought it was all over with the child, which presently became deadly pale and faint. A verdict of *infanticide by misadventure* stared us in the face. But under the use of stimulants the infant revived again; no hæmorrhage went on internally, as we apprehended it would; but the child, after a day or two, seemed very much the better for the loss of blood. This amendment, however, did not last; and the mother, who had been terrified by the immediate consequences of the operation, feared to come near me, lest I should wish to have it repeated; and at length our patient died. I was very desirous to examine the interior of the head; but this was not permitted.

On one subsequent occasion I have witnessed the operation. The subject of it was an infant about eight months old. Four months after its birth, its head was observed to grow inordinately large. At the time of the operation the fontanelles were exceedingly tense; the child screamed frequently, occasionally vomited, and was slightly convulsed; the features were pinched, and the eyeballs distorted downwards; but the pupils were not dilated. Four ounces of transparent liquid were let out by puncturing the anterior fontanelle. A few hours afterwards the child was tranquil, and much improved in aspect; the distortion of the eyeballs had disappeared. Three ounces more were taken away the next day. For two days thereafter all the symptoms appeared to be mitigated; but the skull was flaccid; yielding, like a broken egg, to the gentlest pressure. On the evening of the fourth day after the first tapping, the respiration became hurried, the child grew dull, and, before midnight, expired. In this case it appeared to me that the chance of success was baulked by the want of external support subsequently to the tapping.

[Dr. Whitney relates, in the *Edinburgh Medical and Surgical Journal*, an instance of the successful puncture of the brain in a case of chronic hydrocephalus. By the first operation nine ounces of fluid were drawn off, and in three weeks subsequently, by a second operation, five ounces more. Neither operation was succeeded by any bad symptom, and the recovery of the child appears to have been complete. Two cases are related by Professor Wutzer, and Dr. Butcher, in the *Austrian Medical Journal*, in which the puncture of the brain was unsuccessfully employed. In the first, a child seven months old, death occurred in six days after the first operation; in the second case, of a child sixteen months old, the operation was repeated, after an interval of four weeks, and, seven weeks after the second puncture, the child died in convulsions. Dr. West (*Report for 1844-45*) states that of *sixty* recorded cases in which puncture of the brain was performed, *seventeen*, or one in $8\frac{1}{2}$, had a favourable termination; or, in other words, the recoveries have been to the deaths in the proportion of 28 per cent. — C.]

You will not expect me to draw any comparison between the merits of compression and of paracentesis, as substantive remedies. They are opposite measures, and adapted to different and opposite conditions of the brain. The one repairs defect of pressure; the other relieves its excess. To hold the balance even requires much care, a steady and gentle hand, an accurate judgment, and incessant vigilance. Either expedient may suffice, alone. Both may be (and have been) profitably employed in the same case, in succession, according to its varying circumstances. If the walls of the head be tight and firm, the trocar should precede the bandage; if lax and moveable, compression should be cautiously tried, and followed, if need be, by the puncture.

When advertng, in a previous lecture, to the radical cure of hydrocele, I remarked that in other forms of dropsy we scarcely dared to employ, with the same view, the injection of irritating substances into the emptied cavity: but, in so saying, I underrated the hardihood of operative surgery. To cure chronic hydrocephalus in a radical manner, preparations of iodine have been thrown into the tapped skull. In one instance, not only was such fluid injected, but in order that it might be brought into contact with the whole internal surface, the poor child's head was shaken, as one might shake a phial. In another case no fewer than twenty-one injections were practised; and from first to last, not less than a drachm and a-half of iodine, and four drachms and a-half of the iodide of potassium, were thrown in upon the brain. Both arachnoidean and ventricular hydrocephalus have been thus treated. It is very sur-

prising that these rude handlings of one of the most delicate textures of the body should have been apparently so harmless. If, however, they have hitherto inflicted no palpable injury, neither have they effected any permanent good. In one example the head was tympanitic after the operation; in more than one, the dimensions of the skull diminished somewhat for a time; in none did any signs of inflammation ensue.

All this (as Mr. Hewett observed) merely shows what some children will bear.

I have now done with the *inflammatory* affections of the *brain*: in conjunction with which I have also considered some other morbid conditions, that are either connected with inflammation of the contents of the cranium, or resemble it in some of their phenomena. Thus, I have spoken of *delirium tremens*, which is apt to be mistaken for inflammation of the brain: of *softening from disease of the cerebral arteries*, which is liable to be confounded with inflammatory softening: of *tumours* of different kinds, which tend to produce inflammation, or symptoms like those belonging to inflammation: and of *chronic hydrocephalus*, which sometimes is the sequel, sometimes the precursor, of acute hydrocephalus; and has other points of analogy with that disease, the encephalitis of strumous children.

[*Meningitis encephalica*.—Under this name, Dr. Brockman has recently described a peculiar form of cerebral disease incidental to childhood, in which the membranes of the medulla oblongata and pons varolii are chiefly affected.

Dr. B. has met with fourteen cases of this affection. It was, at first, observed by him as a sequel of scarlatina, but subsequently, he has seen it to occur most frequently as an idiopathic affection.

It is sometimes associated with general disease of the brain; at others, it is uncomplicated. Notwithstanding it is unattended, in its earlier stages, by any serious symptoms, it is an affection fully as dangerous as cerebral meningitis. The first stage, or that of simple hyperæmia, generally continues for one or two days. The child is dull and heavy, and the occiput is often hot; the bowels, however, are regular; there is no vomiting, no intolerance of light, nor any disturbance of sleep. The general dulness of the patient, and vague complaints of some uneasy sensation in the head, increase as the inflammatory stage sets in; the heat of the occiput is augmented; the head becomes retracted, as in the ordinary cases of acute hydrocephalus; and convulsive twitchings of the limbs occur, similar to the effects of light electric shocks, which recur every few minutes while the patient is awake, but cease during sleep. The general febrile symptoms continue during the third stage; the pulse, however, diminishes in frequency and fulness, but does not become either irregular or intermittent. The general disquietude of the child subsides, by degrees, into a comatose condition, in which the head becomes still more retracted, but unattended with strabismus, or any morbid condition of the pupil; the peculiar air of stupidity that characterizes hydrocephalic patients is wanting. Two pathognomonic symptoms, however, indicate the occurrence of the stage of effusion. One of these is deafness; the other difficult articulation, and difficulty in moving the tongue—both of which occur at the same time, probably from paralysis of the motor nerves of the tongue. The deafness and affection of the tongue usually occur suddenly; sometimes they are first observed upon the child awaking from a quiet sleep. They are, according to Dr. Brockman, the earliest and most certain indications of the occurrence of effusion. This stage continues, sometimes, for three, and sometimes for fourteen days. Its termination is in fatal paralysis, the occurrence of which is often preceded by various singular nervous phenomena—as, sudden pauses in the respiration, or equally sudden syncope. In some cases, however, the paralysis does not follow, but the anomalous symptoms subside, and the patients gradually recover; until, indeed, the paralytic stage is fully established, the recovery of the patient is still possible.

In the uncomplicated cases of the disease, upon examination after death, the cerebrum in general, presents an extremely pallid and anæmic condition, in striking contrast with the cerebellum, the vessels of which are turgid with blood, while its substance, also, is often in a state of marked hyperæmia. The hyperæmia increases in intensity towards the central portions of the encephalon; and the membranes covering the pons varolii and medulla oblongata are found in a most decided state of

inflammation. The portion of inflamed membrane is perfectly isolated, and not more, usually, than a square inch in extent—the membrane of the cerebellum being entirely free from any indications of inflammation. There is ordinarily an effusion of a serous fluid into the sub-arachnoidal tissue; sometimes to the extent of several ounces; occasionally a gelatinous matter is effused, and, in some cases, the effusion is of a purulent character.

This form of the disease is most frequently observed in children from three to ten years of age, and who had previously enjoyed good health.

The treatment recommended by Dr. Brockman is, in its first two stages, depletion, by leeches to the posterior part of the head, cold applications to the scalp, and the free administration of calomel, which latter may be continued during the stage of effusion. Here, however, it becomes necessary to support the strength of the patient; for this purpose ammonia is directed by Dr. B., but he remarks that, in some cases, the administration of wine may be required. According to his experience, powerful counter-irritants, as a large blister, or the actual cautery, prove, also, sometimes beneficial.—C.]

Before I take up the subject of apoplexy, and of palsy, I wish to direct your attention to the inflammatory conditions of the *spinal cord*.

The whole pathology of this portion of the nervous system is extremely interesting; but it has not yet been so thoroughly made out as to enable any one to give a very systematic or satisfactory account of it. In addition to those numerous difficulties with which I showed you in a former lecture that the entire subject of the diseases of the nervous apparatus is beset, there is this further obstacle to our studying diligently the structural changes of the spinal marrow—that much labour and expense of time are required for exposing the interior of the vertebral canal; which is, therefore, too often neglected in examining the dead body.

There are certain points in the anatomy and physiology of the spinal cord which it is necessary that you should bear in mind, if you would have any clear notions even of what has been learned in respect to its pathology.

1. In the first place, the spinal cord (including the medulla oblongata) is the seat and centre of that remarkable property, the reflex function; by which so many of the automatic movements of the body are regulated.

2. In order that we may feel, or be conscious of, what occurs in any part of the trunk or limbs, and in order that our will to move any such part should be obeyed, it is necessary that there should be a continuity of nervous matter between the part in question and the brain. If the cord be cut across at any point, or so crushed as to be thoroughly disorganized at that point, a complete abolition of sensation and of *voluntary* motion ensues in all those parts of the body that receive their sentient and motor nerves from that portion of the cord which lies beyond the place of the injury, reckoning from the brain. What is true in this respect of the mechanical division of the cord, is equally true of such disease as pervades and spoils the nervous matter composing it.

Now it follows from this, that the effects of disorganizing forms of disease—as well as the effects of injury—must vary greatly according to the part of the cord they occupy.

Thus any such disease or injury affecting the whole thickness of that portion of the spinal marrow which is contained within the upper cervical vertebræ, is inevitably fatal at once; producing suffocation by paralyzing those muscles through the play of which the motions of respiration are performed. You know that the intercostal muscles and the diaphragm have at all times the main share in carrying on the mechanical actions of respiration; and probably they execute the whole action in every case of ordinary breathing. Now the intercostal muscles are furnished with motor nerves from the spinal cord, all along the dorsal vertebræ; and the diaphragm is principally supplied by the phrenic nerves, which are chiefly derived from the third and fourth cervical nerves. These muscles obey the will; but they act also independently of the will. The pneumogastric and trifacial nerves, with respect to them, are excito-motory nerves, and call into play a reflex power which is transmitted from the medulla oblongata. Hence any profound injury of the spinal cord, above the origin of the phrenic nerves, stops both the voluntary and the involuntary movements of the respiratory

muscles, and the individual perishes by apnoea, in as strict a sense, as though the access of air to the lungs had been suddenly prevented by a ligature drawn tightly round his wind-pipe.

Again, when a segment of the cord, however small, is disorganized in its cervical part, between the origin of the phrenic and the origin of the upper intercostal nerves, the breathing is not instantly suspended; but is performed entirely by means of the diaphragm, the intercostal muscles having no share in it. The ribs cease to rise and fall; and the abdomen is alternately protruded, and sinks back again. In each case I suppose the disease of the cord to be such as suffices to paralyse the parts supplied with nerves from it, beyond the seat of the disease. If disease of this kind occur below the giving out of the intercostal nerves, the breathing is not affected; we have *paraplegia* only, palsy and loss of feeling in the lower extremities, and perhaps in the hips, or even higher. Now a person in this condition *may* live a long time. When the disease is situated between the origin of the intercostal nerves and the origin of the phrenic, he may live a few days, but he seldom lives a week, and never survives a month; and when the disease is higher still, in the very upper part of the cord, above the origin of the cervical nerves, he perishes outright. The *kind* and *degree* of disease, therefore, being the same, the character of the symptoms, and the amount of danger, differ remarkably according to the *seat* of the disease.

3. Although sense and voluntary motion cease upon the disruption of the communication with the brain, the excito-motory functions of the separated portion of the cord are not necessarily suspended. On the contrary, they seem to acquire increased activity. The automatic power is apt to run riot, as it were, when the controlling influence of the sensorium is withdrawn. All of you probably have seen the limbs of a recently decapitated frog thrown into violent action by the stimulus of galvanism. I have witnessed the same thing in the human body after death by hanging. What is still more curious, you may have unequivocal manifestations of similar phenomena in the *living* body. I have lately been informed, by Dr. William Budd, of a case in which a man was afflicted with paraplegia, in consequence of disease of the vertebral column. He was totally deprived of the power of moving his lower extremities. Sensation in them was almost, yet not entirely, extinct. A sharp pinch, or the prick of a pin, he could feel; but slight friction he was quite unconscious of: yet (as he himself said) his limbs were not; for when the inner edge of the foot was brushed or tickled by the hand of another person, the corresponding leg, over which he had no voluntary control, would start up, and be briskly convulsed. The same thing took place, in both limbs, whenever he passed his urine or fæces; so that he was obliged to have an apparatus of straps and ligatures to keep the legs down on such occasions. I have seen something like this myself, in several instances.¹

4. Under the sagacious researches of Dr. Marshall Hall, the physiology, and with it the whole pathology, of the spinal cord is undergoing, at this very time, a complete reformation. I know of no modern discovery so fruitful of important practical consequences, or so likely to improve our remedial management of nervous disorders, as the singularly interesting truths which he and others are even now engaged in demonstrating and enforcing. I do not profess to teach you this new physiology. I touch only, as I pass along, upon some of its cardinal points, to which I may have occasion to refer in future. We are considering how the signs of spinal disease may vary according to the particular location of that disease; and I would have you remark, here, that inasmuch as all the acts of ingestion and expulsion, all the inlets and outlets of the body, are governed by the spinal marrow, with its corresponding apparatus of incident and motor nerves—it is to be expected that disease in the upper part of the true spinal system should affect the orifices which answer to that part, and which are principally inlets—the larynx, the gullet, the cardia; while disease in its lower portion will be likely to disturb the natural functions of the lower orifices—the rectum and anus, the bladder and urethra, the os uteri—which are chiefly outlets.

5. You must bear in mind also the grand discovery of Sir Charles Bell, that the two roots by which each spinal nerve arises have distinct and different functions; the anterior roots being composed of motor fibrils, the posterior of sensiferous.

¹ This very interesting case has since been published, in detail, with several others resembling it, in the 22d volume of the *Mémo-Chirurgical Transactions*.

It was a natural inference, from this discovery, that the anterior *columns* of the spinal *cord* were subservient to the purposes of motion, and the posterior to the faculty of sensation. But this was an erroneous inference. Later anatomical researches, those especially of Mr. Lockhart Clarke; and experiments made upon living animals, especially the experiments of Dr. Brown Séquard, concur with the results of clinical observation to prove that the posterior white columns are *not* the channels of sensation. Sensibility has remained perfect when these columns were thoroughly disorganized. It is probable, from their relations with the cerebellum, that a part at least of their office concerns the co-ordination and regulation of the muscular movements of the body.

The anterior, or the antero-lateral columns, with the grey matter of the cord, are doubtless the seat and channels of the motory power, while they also minister to the transmission of sensations.

It is a curious fact, elicited by clinical experience, that in disease of the cord, not involving the roots of the nerves, the power of moving the limbs is commonly earlier diminished than their sensibility.

Sometimes it happens that sensibility, or voluntary motion, or both, are impaired in the upper extremities, while the same functions remain perfect in the lower and more distant limbs. In explanation of this phenomenon it has been supposed that distinct and different filaments of the spinal cord, extending from its junction with the brain, connect themselves with or help to form the several nerves which emerge from the cerebro-spinal axis; and that disease of the brain, or of the cord, has spared those strands and fibres which pass down to the nerves given off at the inferior part of the spine, while it has affected those strands only which belong to certain nerves from the superior part.

But a similar limitation of paralysis to the upper limbs may result from the implication of the *roots* of the spinal nerves, in disease of the upper segments of the cord itself or of its membranes. And it is a curious fact, worth remembering, and certified both by physiological and pathological observation, that irritation or disease affecting the roots of the upper cervical nerves may cause inequality in the pupils of the eyes, and suggest suspicion of mischief within the head, when the disorder is purely spinal.

6. We must not forget that the brain, and the spinal cord, which are distinct from, but yet continuous with, each other, sympathize largely and mutually under disease. This circumstance throws an additional obscurity over the study of their morbid conditions. It is one, however, which we cannot avoid, but which we must estimate and allow for, in our observation of diseases, as we best may.

7. There are a few remarks made by Dr. Abercrombie, in relation to some of the anatomical dispositions of the cord and its investing membranes, which may help us to comprehend better some of their morbid contingencies. Thus, with respect to the dura mater of the cord, it is practically of importance to recollect "that it adheres very slightly to the canal of the vertebræ by a very loose cellular texture: and that it adheres very intimately to the margin of the foramen magnum. In this manner a cavity is produced betwixt the membrane and the inner surface of the spinal canal (external, *i. e.*, to the membrane), which cavity may be the seat of effusion, and which has no communication with the cavity of the cranium. On the other hand, the space between the dura mater and the pia mater (or membrane immediately covering the cord) communicates freely with the cavity of the cranium; so that fluid may pass easily from one to the other, according to the position of the body."

I shall pursue the same order, in speaking of the inflammatory affections of the spinal cord, as I followed in respect to the analogous conditions of the encephalon. And, first, let us inquire what has been noticed of inflammation of the *membranes* of the cord. They may undergo inflammation, independently of the substance of the cord, and independently of the brain; but this is not very common. Usually, when we have meningitis of the cord, we have the same disease also within the cranium: usually too, with meningitis of the cord, we have more or less inflammation of the nervous matter composing it. The commonest symptoms of inflammation of the meninges of the cord (for I do not pretend to speak of the several membranes separately) appear to be pains, often intense, extending along the spine, and stretching

into the limbs, and aggravated usually by motion, and simulating therefore rheumatic pains: rigidity or tetanic contraction, and sometimes violent spasms, of the muscles of the back and neck, amounting in some instances to perfect opisthotonos: a similar affection of other muscles also, as those of the upper or lower extremities: a sense of constriction in various parts, in the neck, back, and abdomen, as if those parts were girt by a tight string: feelings of suffocation: retention of urine: priapism: obstinate constipation: and with these symptoms, rigors often.

You are not to expect in every case all the symptoms which I have been enumerating: they will vary according to the seat and extent of the inflammation. We need not wonder at the spasmodic symptoms, when we recollect that the nerves which issue from the body of the cord receive a covering from its pia mater. The pain felt along the course of the spine itself is said to be aggravated by the percussion of the spine, but not by simple pressure; and this seems very likely.

I know of no way in which I can so well hope to awaken an interest in you about these diseases, or to offer you instruction respecting them, as by instances. The following I take, abridging it somewhat, from Dr. Abercrombie. A man, twenty-six years old, had for several years been subject to suppuration of the left ear; suffering occasional attacks of pain on that side of the head, which were followed by a more copious discharge from the ear. In the first week of April he became ill, with pain of the forehead and occiput, disturbed sleep, and loss of appetite; but no fever. At the end of the week he complained of pain extending along the neck. This pain gradually passed downwards in the course of the spine, and deserted the head; and at last, after many days, it fixed itself with intense severity at the lower part of the spine; shooting thence round the body towards the crests of the ilia. He became affected also with great uneasiness over the whole of the abdomen, and had much pain and difficulty in passing his urine. About the end of the second week in April his sufferings had become extreme. He could not lie in bed for five minutes at a time, but was generally walking about the house in a state of great agitation, grasping the lower part of his back with both his hands, and gnashing his teeth with the intensity of his pain. He had no interval of ease, and was sometimes incoherent and unmanageable. On the 16th, he went to take a warm bath, walking down three stairs, and into an adjoining street, with little assistance. His speech afterwards became somewhat affected: there were convulsive twitches of his face, and difficulty of swallowing. Some transient squinting also was observed. The pulse was now very frequent. On the 18th, while sitting in a chair, he suddenly threw his head backwards with great violence, fell immediately into a state of coma, in which he remained for two hours, and then died. During the whole disease, there had been no paralysis, except the slight affection of his speech; no difficulty of breathing; no vomiting; and no convulsion except the twitching of his face the day before his death. The pulse was small and irregular. The bowels were easily kept open, but the pain in his back was much increased by going to stool. Two days before his death he had several attacks of shivering; and much purulent matter was discharged from his left ear during his illness.

Upon a very careful examination of his body, every part of the brain was found to be in a most healthy state. Some gelatinous deposit was found under the *medulla oblongata*; and purulent matter flowed, in considerable quantity, out of the spinal canal. The spine being entirely laid open, the cord was seen covered with a coating of purulent matter, which lay betwixt it and its membranes. The matter was most abundant in three places; at the upper part, near the foramen magnum—about the middle of the dorsal region—and at the top of the sacrum: but it was also distributed over the other parts with much uniformity. The substance of the cord was soft, and separated in some places into filaments. All the other viscera were healthy.

You may find several interesting examples of this form of disease in OLLIVIER's *Treatise on the Spinal Marrow*. The prominent symptom was generally *pain*, referred to some part of the spine, and *increased by motion*; and what is curious, sometimes little complained of except upon motion. In general, also, it extended along some of the limbs, and was accompanied by muscular rigidity, or tetanic spasms. Palsy occurred in one case; but this seemed to have been owing to *softening* of the cord itself. Constantly there was increased sensibility; a circumstance which Ollivier thinks cal-

culated to distinguish inflammation of the membranes from inflammation of the substance of the cord; the latter being usually attended with *diminished* sensibility. In the case that I have quoted from Dr. Abercrombie, the intense pain underwent no remission or abatement. In one of Ollivier's examples, there was, at the commencement of the disease, a striking intermittence of the pain; it came on with intense severity at ten at night, and lasted till three in the morning.

The causes of spinal meningitis are not always to be discovered. It sometimes extends from within the cranium. It may be excited by external violence to the spine, of which a good specimen has been recorded by Sir Charles Bell:—A wagoner sitting on the shafts of his cart, was thrown off by a sudden jerk, and pitched upon the back of his neck and shoulders. He was taken to the Middlesex Hospital, where he lay for a week, without complaining of anything except stiffness of the back part of the neck. He could move all his limbs with freedom. On the eighth day after his admission he was seized with general convulsions and locked jaw. He then became affected with a singular convulsive motion of the jaw, which continued in violent and incessant movement for about five minutes. This was followed by maniacal delirium. He then sank into a state resembling typhus fever; and after four days was found to be palsied and insensible in his lower extremities. The day before his death he recovered sensation in his legs.

On dissection, a great quantity of purulent matter was found within the spinal canal. It appeared to have formed about the last cervical and the first dorsal vertebrae, and to have dropped down, by its own gravity, to the lower part of the canal; where it produced palsy and anæsthesia of the inferior limbs by the pressure it occasioned.

Inflammation of the *substance* of the spinal cord leads to the same changes in its texture which have been already spoken of as being often the results, in the brain, of inflammation of the *cerebral* matter. Softening—induration—suppuration. I need not, therefore, again describe the physical characters of these alterations.

The symptoms which flow from inflammation of the nervous pulp of which the spinal marrow is composed, are by no means uniform; nor can we expect that they should be so, when we call to mind what has been already stated of the different effects that must ensue according as different parts of the cord happen to be implicated. The phenomena will vary likewise, according as the inflammation is acute or chronic. If we recollect how many parts of the body depend for their power of motion, and for their sensibility, upon the integrity of the spinal cord, we shall not be surprised at the diversity and multiplicity of the symptoms produced by disease of the cord. Tracking inflammation and its events from the upper portion of the spinal marrow downwards, we should expect to find, and we actually do find, some such an arrangement of symptoms as the following:—Convulsive affections of the head and face, inarticulate speech, loss of voice, trismus, difficult deglutition, spasmodic breathing, irregular action of the heart, constriction of the chest, vomiting, pain of the belly, sensation of a cord tied round the abdomen, dysuria, retention of urine, incontinence of urine, constipation, tenesmus, involuntary stools: and with respect to the voluntary muscles corresponding to these parts of the spinal marrow, convulsions, or palsy; or palsy succeeding to convulsions.

I must again have recourse to examples, to put you, more fully than any attempted abstract picture could put you, in possession of such forms of inflammation of the cord as you may expect to meet with in practice.

A man, fifty-six years old, was exposed to severe cold, while travelling on the outside of a coach. After this he was attacked with pain in the right arm and leg, most severe about the shoulder, but affecting the whole side, and he had also considerable headache. He soon perceived some loss of power in the affected limbs; and the progress of this was very curious. It began at the upper part of the arm, and extended downwards so gradually, that he was able to *write* distinctly, after he had lost the power of raising the arm, or bending the elbow. Then the leg became affected in the same gradual manner, and after ten or twelve days from the commencement of the disease, the whole leg and arm had become completely paralytic. Some pain continued in the parts, and it was occasionally severe, especially in the leg. Repeated blood-letting, and purgatives, and blistering, were employed. His

mind remained quite entire. His pulse was 84, and rather weak. After some time the *left* arm became paralytic, rather suddenly; but it was not so completely motionless as the limb on the right side: the left leg was not at all affected. Slight delirium occurred, but passed off again. At the end of two months after the exposure to cold, he again became delirious, and his pulse got feebler and rapid: he then fell into a state of stupor, muttering incoherently, but answering questions distinctly when he was roused. He lost his speech a few hours before death. For the last eight or ten days there had been considerable sloughing of the sacrum.

The brain was found to be healthy throughout. Much bloody fluid was discharged from the spinal canal into the cavity of the cranium before the spine was laid open. On displaying the spinal cavity itself, the cord was found in a state of complete softening, from the second to the last cervical vertebra. The portions above and below that part were quite healthy. (ABERCROMBIE.)

Comparing this case with the one I detailed of meningitis, we find that pain was present in both, but more severely so in the case of inflammation of the membranes: we find also, that stiffness and spasm of the muscles marked the *meningitis*; palsy, the *inflammation of the substance of the spinal cord*. In neither of them were the intellectual functions disturbed till towards the last. I believe that the characters now pointed out belong to these forms of disease respectively.

I borrow the following example (abbreviating it) from an interesting collection of *Cases of Paraplegia*, recently published by Dr. Gull.

A healthy brickmaker, twenty years old, walked twenty-eight miles on the 18th of July, 1855, in search of work, and slept in a brickfield. On the next day, which was close and wet, he walked thirty-two miles, and allowed his wet clothes to dry upon his body. On the 20th his legs suddenly gave way under him, and he fell down; but he got up again, and walked from his garden into his house, and two hours afterwards up stairs to his bed, feeling all the time "pins and needles" from the thighs to the feet. Retention of urine then came on, and his bowels ceased to act. On the 26th there was complete paraplegia, with involuntary twitchings and spasms of the legs, and gradually increasing anaesthesia below the navel, but nowhere complete. The motions of the lower ribs were imperfect. He had no priapism, no sense of tightness round the waist, and scarcely any pain. Slight movements of the legs could be produced by tickling the soles of his feet, and these movements were more readily excited as the case advanced. The retained urine became ammoniacal; sloughs formed over the sacrum; and he died exhausted, without any delirium, on the 20th of August.

There was no adhesion between nor effusion of lymph upon the spinal membranes, which had an anæmic appearance. At the middle of the dorsal region there was marked softening of the cord with slight enlargement for two or three inches. The posterior columns were quite diffuent, and contained exudation granules, to an extent much greater than was indicated by the softening visible to the naked eye. The anterior columns were softened, but retained their form. Both columns were of an opaque white colour; the grey matter was mottled by injection of its vessels.

Here again, the cord being affected and the membranes untouched, we have palsy, with scarcely any pain, and no disturbance of the mental functions.

Much may be learned in regard to the effects of inflammation, or any other cause of disorganization, *confined to a limited portion of the cord*, by observing what takes place in those injuries in which the bones of the vertebral column are broken or displaced. Of course I do not dwell upon these accidents, for they belong to surgery: but I have seen a good many of them, and watched them with deep interest. The symptoms are much more uniform than when inflammation occurs within the vertebral canal independently of external injury; simply because the injury to the cord is more definite and local. But such cases are very valuable objects of study to the physician. I remember several that occurred when I was a dresser in St. Bartholomew's Hospital; and I will state very briefly the particulars of one as an exemplar. In the year 1820, a man was brought there who had been thrown out of a tilt cart, in consequence of a dray's running foul of it. He had pitched upon his head, which showed, however, no trace of injury. When picked up he was found to be powerless, both in the upper and lower extremities. His stools passed from him without his being aware of it, and it was necessary to use the catheter to empty his bladder. He breathed

entirely by the diaphragm — that is, his thorax was motionless, and his abdomen rose and fell with every alternate act of inspiration and expiration. These symptoms are perfectly distinctive of injury to the cord between the origins of the phrenic and intercostal nerves. He suffered pain about the middle part of the neck behind. He went on exceedingly well for four or five days, and then the nurse very foolishly acceded to his request to be turned on his side, which caused his death in a very few minutes. This is not the only instance that I have known, in which life has been suddenly extinguished by similar imprudence. The lesson may be useful. There was another patient in the same hospital, who had fractured the cervical portion of the spinal column. Among other remedial measures, the surgeon had directed that his head should be shaved. The barber had performed half his task, and was turning, with his hands, the unfortunate man's head into a more convenient position for completing it, when he suddenly expired. The twist was fatal to him.

On the examination of the body of the patient whose case I was mentioning, a very remarkable state of the spinal column was found. The fifth and sixth cervical vertebræ were dislocated from each other *without any fracture*; a thing which has sometimes been pronounced impossible. The articular processes were fairly separated; and the vertebræ were also forced asunder, by the detachment of one of them from the intervertebral substance. The nervous matter of the cord opposite the point of dislocation was quite soft.

There is one very common and distressing consequence of such disease of the spinal marrow as produces paraplegia, not particularly noted in any of the cases which I have related, but always to be looked for. The muscles by means of which the bladder empties itself, are liable to participate in the palsy; and then the bladder empties itself no longer. The urine accumulates in it, and distends it, and even the ureter becomes dilated; and in this way not only the present but the prospective danger is increased. For the foundation of future disease in the kidneys is often thus laid, even when such distension of the bladder by its retained contents occurs independently of any disease of the spine; as it may do from stricture; from enlargement of the prostate; or even from the voluntary retention of the urine beyond a certain period, through feelings of delicacy. You are to look out, I say, for this distension of the bladder, and relieve or prevent it by the introduction of a catheter through the urethra. You must not be deceived by being told that the patient passes plenty of water; that it even runs from him. Incontinence of urine is, in fact, in these cases, though it may sound paradoxical, a sign of retention of urine. The urine dribbles away because the bladder admits of no further distension; it overflows, and runs out at the natural orifice, but the bladder remains constantly full and stretched. You must make an examination, therefore, of the hypogastric region with your hand. If you find that part of the belly hard and resisting, and giving out a dull sound on percussion, you may be sure, in these cases (where there is paralysis of the lower extremities, and the water dribbles away), that the bladder is full, and has lost the power of expelling its contents. Sometimes you may recognise the fluctuation of the urine in the distended bladder, and ascertain the globular shape of that organ. It may rise even beyond the umbilicus. But what I chiefly wished to point out to you is the circumstance that the bladder becomes diseased, and the urine altered in quality, under this state of palsy. The urine grows alkaline, turbid, and ropy, and exhales a very offensive ammoniacal smell; and the inner surface of the bladder is found, after death, to be thickened, red, and smeared with adhesive puriform mucus — in a state of chronic inflammation, in short.

LECTURE XXVIII.

Inflammatory and Structural Diseases of the Spinal Cord, continued. Treatment.

Apoplexy. Its General Symptoms and Diagnosis. Symptoms characterizing the Apoplectic State. Pressure the ordinary Physical Cause. Hemiplegia. The Palsied Muscles Rigid, or Limber.

ALLOW me to repeat that the structural diseases of the spinal cord will reveal themselves, by their symptoms, most clearly to him who most distinctly perceives, and most accurately bears in mind, the *physiology* of that part of the nervous system. But to the best informed, and the most sagacious, they are too frequently obscure and perplexing.

Disease occupying a portion only of the cord, but affecting the *whole thickness* of that portion, from centre to circumference, will be likely to disturb, or suspend, the functions of sensation and voluntary motion in all the parts supplied with motor or sentient nerves from that portion of the cord, and from the portions beyond it. So that a great variety of symptoms depend, when the *amount* of disease is the same, upon the *place* of the disease. A total interruption of the conducting function of the cord, in the neck, above the origin of the phrenic nerves, extinguishes life by stopping the actions of respiration. A similar interruption in the cervical part of the cord, above the origin of the intercostals, but below the origin of the phrenic nerves, destroys life as certainly, but not so rapidly, nor in exactly the same manner. We find the lungs loaded with frothy serous fluid in such cases; we find the bladder inflamed; and, often, sloughing of the integuments and muscles of the nates and hips. A similar interruption below the dorsal vertebræ is not *necessarily* fatal, even when it is attended with permanent paralysis: but it *usually* is so, sooner or later.

When the interruption of function is not total and complete, disease of the cord will be likely to produce rigidity, convulsions, tremors, or simple weakness, of the corresponding muscles; pain, tingling, numbness, of the corresponding limbs and surfaces.

The spinal cord is a symmetrically double organ, and disease limited to one of its lateral halves will derange or abolish the power of movement in the corresponding muscles *on the same side of the body alone*; and perhaps disturb somewhat the sensations of that side. But the experiments of Dr. Brown Séquard appear to prove that there are sensiferous fibres proceeding upwards and downwards from the posterior roots of the nerves; and that these sensiferous fibres decussate each other within the cord. If this be admitted as an anatomical truth, it explains what was formerly a perplexity, namely, that paralysis of one side of the body is sometimes associated with anæsthesia of the other side. In this severance of palsy and defective sensibility, may we not find a clue to the special diagnosis of hemiplegia that is strictly spinal, from hemiplegia that is cerebral in its origin?

If you impress upon your recollection the facts thus summarily stated, you will find in them, I think, a key to many of the phenomena which accompany, and denote, more or less plainly, disease of the spinal marrow.

Inflammation of the *membranes* of the spine is most apt to declare itself by *pain*, increased on motion, of the spine and of the limbs; and by *rigidity* and *spasm* of the muscles of the neck and back. Inflammation of the *cord* itself, which readily passes into, or rather produces, softening of its substance, is most commonly marked, first, by convulsive movements of some parts of the body; secondly, by palsy of those parts, with or without anæsthesia. The same may be said of *suppuration* when it occurs as an event of inflammation; and the pus may be collected into an abscess in the nervous matter of the cord, or it may be diffused and mixed with softening.

Now I need not dwell upon the *treatment* proper to be adopted in inflammation of the spinal cord and its membranes. *Mutatis mutandis*, it is the treatment already recommended in inflammation of the *brain* and its membranes. When the inflammation is acute, we must take blood from the arm, or by cupping-glasses along the sides

of the spine. Blood enough may be taken by cupping along this tract to produce the effect of general bleeding as well as of local. Perfect rest in the horizontal posture must be strictly enjoined. Mercury will generally be proper.

In more chronic forms of inflammation within the spinal canal, we have still a capital remedy in cupping: and counter-irritation in various ways, but more especially by means of issues made on one or both sides of the spinous ridge, is also, in many cases, of most essential and unquestionable service.

Great care must be taken, when there is palsy of the bladder, not only (as I admonished you in the last lecture) that the urine be regularly drawn off, but also that the patient be kept *dry* and *clean*; for if great attention be not paid to this point, sores will form where the urine remains in contact with the skin, to the great increase of his suffering, and of his danger. Indeed, take what pains we may, there is generally a strong disposition to the formation of sloughs upon the sacrum and hips in cases of paraplegia. They result from the perpetual pressure made upon those projecting points; from the feeble state of the circulation in the palsied parts; and (often) from the irritation of the urine and fæces, which are passed without the sufferer's consciousness.

When the patient is kept clean and dry, and the surfaces on which the weight of his body has been supported begin to be red and angry, you may protect them by a plaster: or by rubbing them with brandy, you may sometimes prevent the skin from breaking: or what is best of all, you may put your patient upon one of Dr. Arnott's hydrostatic beds; and then the pressure will be equally distributed over all that portion of the body which comes in contact with the waterproof material of the bed.

To bring this outline of the diseased states of the spinal cord up to that point in which we left those of the encephalon, I may state that, like the brain, the spinal marrow may become hardened by chronic inflammation; and, like the brain, it may be enroached upon by tumours; fibrous, scrofulous, or malignant. With respect to these, all that I can now say likely to be of any practical benefit to you, is that the symptoms they occasion are those of slowly increasing paralysis, or of slowly increasing rigidity of the muscles, without fever or what is called reaction; and that the locality, and extent, and effects of the paralysis, or of the rigidity, will vary according to the part of the cord in which these morbid conditions occur, and the depth to which they affect it.

[CEREBRO SPINAL MENINGITIS has, of late years, attracted considerable attention from the circumstance of its having occurred as an epidemic in different parts of Europe and the United States.]

According to M. Rollet, who describes the disease as it occurred at Nancy, (*Bulletin de l'Acad. Roy. de Méd.*, viii. 43,) it occurs under two forms; in the one, there are no signs of lesion of the nervous centres themselves, no affection of sensation or motion, though there are all the symptoms of inflammation of the membranes; at first, rigors, then malaise, tinnitus aurium, vertigo, violent pain in the head, extending along the vertebral column; agitation or restlessness, slight delirium, and moderate fever, or absence of fever. In the second form there is affection of the intellectual faculties, and also of the functions of sensation and motion, with more or less complete abolition of all the senses. In this latter form the appearances upon dissection were, great vascularity of the cerebral arachnoid; a layer of plastic purulent matter covering the whole inner surface of the pia mater and the brain, and a considerable collection of the same matter at the base of the brain, about the pons varolii and medulla oblongata. The cerebrum was slightly punctated, but not softened. The choroid plexus was injected; the cerebellum softened; and the arbor vitæ of a blood-red colour. Beneath the spinal arachnoid there was the same kind of purulent matter as beneath the cerebral arachnoid; and opposite the third dorsal vertebra, a considerable collection of pus, as well as opposite the last dorsal vertebra. The substance of the spinal cord appeared healthy. The lesions here described exactly correspond with those described by MM. Faure-Villar, Chauffard, and Forget, by whom accounts of the disease have been given as it prevailed in Versailles, Avignon, and Strasburg.

Morbid changes from inflammation have also been noticed in the alimentary canal, but these M. Rollet regards as merely accidental coincidences. M. Forget, however, attaches great importance to them.

The appearances alluded to are slight redness of different portions of the gastro-intestinal mucous membrane, in the form of patches, arborizations, or dots: in some instances a diseased condition of the follicles; in others, thickening, or softening, to a greater or less extent, of portions of the mucous membrane of the stomach and ileum; and in other cases, again, enlargement and even ulceration of the agminated and solitary glands of the lower portion of the ileum, with enlargement, reddening, or softening of the mesenteric glands. These lesions of the alimentary canal have almost invariably been observed in patients who survived the first few days of the attack, from which circumstance and their infrequency, they can be viewed only as the result of an accidental or secondary affection.

M. Rollet remarks, that, in those cases in which the substance of the brain is affected, there is an almost continual tendency to intermission, or, at least, remission, which alternates about every three hours with an exacerbation; this he regards as merely characteristic of the encephalo-meningitis, and not as an evidence of the disease partaking of the nature of remittent fever, which is the view taken by M. Gassaud.

According to Dr. Mayne, by whom an interesting account is given of the disease as it occurred in Ireland during the year 1846, (*Dublin Quarterly Journal of Medical Sciences*, for August, 1846,) its general pathological characters were nearly uniformly the same wherever examined. The serous membrane covering the brain and spinal marrow was invariably found to be the seat of extensive inflammation, and unlike the more ordinary forms of arachnitis, the *spinal* arachnoid always suffered much more severely than the cerebral. In the post-mortem examinations made by Dr. Mayne, the scalp and dura mater exhibited but little undue vascularity; the pia mater covering the hemispheres of the brain was congested, and the large veins, in their way to the several sinuses, appeared remarkably turgid. The free surface of the cranial arachnoid felt dry and clammy, and had lost its transparency in many places, particularly at the base of the brain, but there was no lymph or other inflammatory effusion in the sac of the arachnoid. Lymph of a yellowish or greenish hue appeared on the surface of the encephalon, *beneath* the serous tunic: this occurred sparingly on the upper surface of the hemispheres, and there only along the sulci; but at the base of the brain it was found in greater quantities, especially in the sub-arachnoid space corresponding to the circle of Willis, where many of the cerebral nerves at their origin were fairly imbedded in it. In the spinal canal, a similar exudation filled the sub-arachnoid space; it there existed in sufficient abundance to envelop the cord completely; it also extended down to the lowest extremity of the cauda equina, investing each of the spinal nerves at its source; but in the vertebral canal, as in the cranium, the cavity of the arachnoid contained none of this morbid secretion. The substance of the brain and spinal marrow appeared remarkably free from lesion; there was no unusual vascularity, induration, or softening apparent, nor did the ventricles betray any evidence of inflammation.

In many of the cases reported, however, the brain and spinal marrow are stated to have been occasionally implicated; in some, the ventricles of the brain contained inflammatory effusions, and the choroid plexus appeared unusually vascular; in others, more or less of the substance of the brain and spinal marrow was found in a state of softening; in a certain number, sero-purulent effusion was detected at the base of the brain, and in the theca vertebralis; but in every instance the serous membrane was the part essentially engaged, whilst the nervous material seldom suffered, and when affected it was only accidentally involved, the disease having been, in such cases, evidently propagated to the substance of the cerebro-spinal axis from its membranous investments.

In the post mortem examinations made at Versailles, in 1839, the left cavities of the heart were found to be almost entirely empty, while those of the right side were filled with large fibrinous coagula, of a yellow colour and some consistence. The same thing was observed by the physicians in other parts of France, especially in cases in which the blood drawn during the lifetime of the patient was buffy and contained but little serosity. Dr. Ames, of Alabama, found the blood drawn from the arm, and by cups, to form large, loose coagula, in which all the red globules were rarely included. The serum separated slowly, and in small quantity. The colour was in general bright—in a few cases approaching to that of arterial blood. In four only,

out of thirty cases, it was buffed. It presented indications of an excess of fibrine. In four analyses of the blood, procured, in two cases, at the first venesection, in one at the second, and in another at the third, M. Tourdes states, that the principal alteration detected was an increase of the red globules and of the fibrine, but especially of the former.

The symptoms by which the disease commences are, in general, of a very formidable character, and its accession is often sudden and quite unexpected; in a large number of cases the patient is in his ordinary health and spirits up to the very moment of the seizure, and experiencing no premonitory symptoms to warn him of his danger. In four of the cases at the South Dublin Union, the boys had eaten a hearty dinner, and retired to bed in apparent health, when the disease, all at once, declared itself.

Very generally, however, the attack is preceded by more or less pain of the head, especially of the forehead, temples, or occiput. The pain is usually constant, but sometimes remittent, or even intermittent. Pain is, also, sometimes experienced in the back of the neck and along the course of the spine, with a sense of soreness in the limbs and joints. In a few cases the attack is preceded by a sense of giddiness, with or without dimness of vision.

Occasionally, the attack commences with a feeling of chilliness, succeeded by a slight increase of the heat of the surface, and pain, extending from between the shoulders to the occiput, with stiffness, to a greater or less extent, of the posterior cervical muscles. In other cases, the patient may be attacked by chilliness, pallor of countenance, coldness of the extremities, low moaning, or muttering delirium, quickly succeeded by restlessness, flushing of the face, a frequent pulse, a wild expression of the eyes, and a hot and dry skin. In other cases, again, the disease may be ushered in by a sense of lassitude and uneasiness, considerable prostration, and a dull heavy pain of the head, with more or less vertigo, especially when an attempt is made to assume the erect position; the eyes are languid and half closed, the speech laborious and indistinct. Occasionally the patient is suddenly attacked with deep coma, or with more or less stupor, attended by a sense of extreme debility, giddiness, dimness of sight, or double vision. Or, finally, the attack may commence with severe pain of the abdomen, immediately succeeded by nausea, and perhaps vomiting. In violent attacks of this character, the extremities become, at the same time, cold and of a bluish colour, and the pulse is reduced to a mere thread. After a few hours, reaction, more or less complete, generally ensues.

Whatever may be the character of the initiatory symptoms, they are replaced, sooner or later, by a state of violent agitation, or by a state of stupor more or less decided, with a slow, occasionally full, pulse, and dilated and immovable pupils. When in this condition, touching any portion of the patient's body will sometimes cause him to emit a short plaintive cry; at others, the patient utters, from time to time, acute cries, and carries his hand frequently to his head. When spoken to, he will, in general, exhibit a degree of consciousness by a motion of the head, by an attempt to articulate, or by opening his eyes for a moment.

Pain, more or less intense, of the head, and along the spine, is present in the early stage of nearly all cases. Pressure applied to the cervical portion of the spine will often produce pain of the head, darting to the forehead, eyes, and temples, as well as pain at the top of the sternum; while pressure on the dorsal vertebræ will cause pain at the middle of the sternum, or about the umbilicus, according as it is made higher or lower. The pain is frequently severe, and continues for some time after the pressure is removed.

Delirium is very commonly present from an early period of the attack. It is often attended with contraction of the pupils; occasionally with dilatation of one pupil and contraction of the other; sometimes with ptosis of the eyelids, and ecchymosis under the eyes. The delirium ordinarily lasts only a short period, but quickly returns. In most cases, the mind of the patient is desponding and apprehensive.

In the majority of cases there is more or less intolerance of light and sound; in some, to such an extent, that the slightest ray of light, or the least unusual sound, is apt to excite convulsive movements. Imperfect vision has been occasionally noticed in the first period of the attack—the patient seeing objects double, or only one half of them, or they appear to him as if enveloped in a mist. The conjunctivæ are often

injected, and the eyes of a glittering and watery aspect. The insensibility of the eyes to light, and complete blindness of one or both eyes, are noticed as having been present in many cases. Violent inflammation of one or other eye is described as being of frequent occurrence in some epidemics.

In some cases there is partial or complete deafness; in others a constant ringing in the ears is complained of from an early period of the attack.

An exalted sensibility of the entire surface of the body is very generally present. The patient winces upon the slightest touch, even of the bed-clothing, and refuses to change his position, from the pain consequent upon every attempt at motion. This exalted sensibility of the cutaneous surface is often manifested only towards the close of fatal cases. Diminution of tactile sensibility and confirmed stupor, when they occur, are always indications of imminent danger.

In very violent cases, petechiæ occur upon the extremities and over the eyelids, within a few hours after the attack. An exanthematous eruption, also, occasionally makes its appearance.

The respiration is sometimes irregular and laboured—a difficulty would appear to be experienced in some cases in expanding the lungs—with respiration chiefly through the nostrils. Stertorous respiration is not a frequent symptom.

There is often continued irritability of the stomach, with insatiable thirst, and tenderness of the epigastrium upon pressure. These symptoms are entirely independent of disease of the abdominal viscera. In two cases in which they persisted in a very marked degree to the close, Dr. Mayne, upon examining the abdomen after death, found the stomach, intestines, and other organs, without any appreciable lesion.

Constipation and suspended secretions are common symptoms of the disease. The tongue is usually more or less coated with a pale ash, white, or yellowish fur. In the more grave and malignant forms of the disease, it has been observed to be broad and flabby—sometimes so enlarged as to impede articulation, and indented around its edges by pressing upon the teeth. An increased flow of saliva is commonly present.

The pulse, during the period of excitement, is usually full and frequent—from 120 to 140 in a minute—often, however, it is very slow—sinking, sometimes, to 48 or 50 in the minute. The pulse has been observed to vary in the number of its beats at different periods of the day.

The most striking characteristic of cerebro-spinal meningitis is that presented by the condition of the muscular system. The muscles of the neck, in particular, become rigidly contracted, drawing back the head upon the vertebral column, and firmly fixing it in that position, so that the patient is unable to move it forwards; neither can this be done by the attendants with the employment of any justifiable degree of force. The countenance, at the same time, assumes very much the tetanic expression. In some cases, the contraction is confined to the sterno-mastoid muscle of one or both sides; in others, again, it is the extensors that are principally affected, the head being retained permanently in its natural erect position. Rigidity is very commonly observed, also, in the muscles of the extremities. The patient loses the power of moving his limbs and of assuming the erect posture. In some instances there is a quivering motion of the muscles of the face, with tremors of the hands, and embarrassment of the movements of the extremities, or spasmodic twitchings in the flexors of the limbs, with a disposition to a constant movement of the legs from side to side, alternately. In some epidemics, rigidity of all the spinal muscles was a common symptom—occasionally, the whole spine, from the occiput to the sacrum, being bent forcibly backwards, like a well-strung bow, so as to prevent the patient from lying flat upon his back. Contraction of the recti muscles of the abdomen is often present.

In many cases there is a difficulty of prehension, it being with great difficulty that the patient can take and drink water from any vessel without assistance. In some cases involuntary twitchings of the muscles are produced whenever the patient attempts to move or seize any thing, as if he were under the influence of strychnia. In others, violent convulsions are induced the moment the inferior extremities are raised up, or merely touched. There is great irregularity as to the period when the tetanic symptoms occur. They may set in as early as the first day of the attack, or not until after the lapse of several days.

Cerebro-spinal meningitis, although it is generally marked by pain in the head,

more or less intense, rachialgia, heat of the scalp, congestion of the conjunctivæ, some degree of intolerance of light and noise, exalted sensibility of the cutaneous surface generally, tendency to coma, and a tetanic affection of the muscles of the neck, and perhaps extremities, may, nevertheless, in many instances present no symptoms of so decided a character as to lead us to suspect the existence of serious disease of the brain and spinal marrow, until the labored pulse, the dilated pupil, the profound coma, or the severe spasmodic or convulsive attacks indicate but too plainly the near approach of death. In other cases, again, and those by no means of rare occurrence, symptoms of a most formidable character may present themselves at the very outset of the disease. Thus, the patient may be attacked at once with violent paroxysms of general convulsions, requiring manual restraint to protect him from injury; or, he may suddenly, without any striking premonitory symptoms, sink into a state of coma almost apoplectic in its character, or, into a half-unconscious condition, with constant moaning or plaintive cries, and grinding of the teeth.

Intermissions of a periodic character are not uncommon in the course of the disease. So complete are these, in some cases, as to lead to the hope of a speedy recovery of the patient, the fallacy of which is shown by the return of the symptoms, in perhaps an aggravated degree, on the following day.

When death is not early induced by the violence of the attack, the patient sinks, more or less rapidly, into a state of profound coma, his pulse becomes slow and laboring, his powers of speech and deglutition entirely fail, his tongue becomes dry, and, together with his lips, encrusted with dark sordes; his stools are passed involuntarily, while his bladder becomes distended with urine, or allows it constantly to dribble away: death finally closes the scene, often preceded by paralysis of one side of the body, or of one or other extremity.

The duration of the disease is very variable. Death may occur within a few hours from the commencement of the attack. The generality of cases terminate about the fourth day, some, however, are protracted to over fourteen, twenty, or even fifty days. Convalescence is usually slow and lingering. Even after an apparently perfect recovery, secondary diseases are apt to occur, and sooner or later destroy the patient.

The diagnosis in cerebro-spinal meningitis is somewhat obscure. There is no symptom or series of symptoms which can be considered as strictly pathognomonic. The disease is in general characterized by acute and fixed pain of the head, rachialgia, aversion from light, injection of the conjunctivæ, increased sensibility of the surface, acute cries, low, muttering delirium, or coma, pain, and stiffness of the posterior-cervical muscles, with permanent retraction of the head, often rigidity of the large extensors of the spine, spasmodic tremors or twitchings of the muscles, particularly of the face, and tetanic convulsions of the limbs. When a disease, marked by several or all of the above symptoms, occurs, especially as an epidemic, we may pretty confidently pronounce it to be cerebro-spinal meningitis.

The prognosis is for the most part unfavourable—sporadic cases, it is true, frequently do well under an appropriate treatment, but in its epidemic form, it has been found to terminate fatally in the great majority of cases. When the attack commences with great prostration, coma, and general symptoms of collapse, death often ensues very speedily without the occurrence of reaction. Few cases recover after severe tetanic symptoms make their appearance. Irregularity of respiration, difficulty of swallowing, great enlargement of the tongue, extensive petechiæ, violent general convulsions, and deep persistent coma, are all unfavourable symptoms.

As already remarked, it is chiefly from the occurrence of cerebro-spinal meningitis as an epidemic, that the disease has of late years attracted the attention of physicians. These epidemic visitations are occasionally confined within very narrow limits, while, at others, as was the case in France, between the years 1837 and 1842, they spread successively over extensive regions. Their occurrence would appear to be altogether independent of any morbid agency referable to peculiarities of climate, season, or locality. Age, and to a certain extent sex, would appear to rank as predisposing causes of the disease, whatever may be the nature of the epidemic agent by which it is produced. Its subjects, wherever it has occurred, have been young persons, generally of the male sex. In Ireland, boys under twelve years of age were those almost exclusively attacked. In Gibraltar, in the great majority of cases, it occurred in males between two and fifteen years of age. In Tennessee, its principal victims were

children between the ages of six and fifteen years. In Missouri, between ten and fifteen years. In St. Augustine, Texas, the patients were generally under fifteen years; in but two or three instances did the disease attack those over eighteen years of age, and not in a single instance a female. In Alabama, however, the majority of those attacked — over fifty per cent — were beyond twenty years of age. Fifty-four per cent. were males. In Texas there was not an instance of the disease occurring among the negroes, who were probably more exposed to morbid agencies than the whites. In France, the disease occurred, for the most part, among the young conscripts who had lately joined their regiments.

With respect to the treatment of cerebro-spinal meningitis but little can be said of a very positive or satisfactory character. The rapid march of the disease in the larger number of cases, allows but a short interval for the application of appropriate remedies. At the height of the epidemic, especially in cases where the attack commences with symptoms of extreme violence, or in which a state of extreme collapse is present from the very onset, the most judicious and best directed treatment will very generally fail to arrest a fatal termination.

In the commencement of the attack, when symptoms of prostration and of deep stupor are absent, as well as during the early period of the stage of excitement, there can be no doubt of the propriety and efficacy of direct depletion. The amount of blood to be drawn is to be measured by the age and condition of the patient and the effect produced. If a weak pulse rise, or a strong one retain its character during the flow of blood, this may be allowed to continue; but if the pulse becomes weak, a moisture breaks out upon the surface, and the face becomes pallid, indicating approaching syncope, the flow of blood should be instantly arrested, even though we may find it necessary, from the rising of the pulse, and the renewed flushing of the face, to re-open the vein a few hours afterwards. The extent of our bleeding should never be proportionate to the degree of restlessness and delirium with which the patient may be affected: these states of violent nervous erethism quickly exhaust the powers of life, and when present, a too copious venesection would be liable to induce a sudden and speedily fatal collapse.

Subsequent to general bleeding, cups should be applied to the back of the neck, and along the spine, and leeches to the temples and neck, and behind the ears, and repeated at short intervals, so long as any indication for direct depletion remains.

After the first bleeding an active mercurial purgative should be administered, and cold applied to the shaved scalp, by means of a bladder half filled with powdered ice, or cloths wet with iced water, or iced water and vinegar. At the same time the feet and legs should be immersed in hot water, followed by sinapisms to the feet and ankles.

In conjunction with direct depletion by the lancet, active purgation cannot fail to prove an important remedy, by producing a determination from the diseased organs. There will scarcely be met with a case in which the presence of gastro-enteric inflammation will contraindicate its employment.

Tartar emetic in divided doses, combined with some one of the saline diaphoretics, will no doubt prove beneficial in the early period of the stage of excitement. In the epidemic which occurred at Vicksburg, Miss., Dr. Hicks gave it, in combination with camphor, in the following prescription, and, as he states, with the best effects: R. Antimon. tart. gr. ij, Pulv. camphor. ʒij., Mucilag. g. acaciæ ʒvj. Mix. Dose, a tablespoonful every two hours.

After direct depletion has been carried as far as, under the circumstances of the case, is judged advisable, especially if the patient falls into a state of coma, with feeble pulse and deficient reaction, sinapisms or blisters along the whole course of the spine will often be found of advantage. Blisters to the upper portion of the spine very generally had the effect, Dr. Ames informs us, of removing or greatly relieving the cephalalgia, even when bleeding had failed to do so. The relief afforded by them in the malignant forms of the disease is, we are informed, very decided. Blisters to the scalp have been advised; we much doubt, however, the propriety of their application to this part.

When the attack commences with symptoms of collapse, or when a state of collapse ensues after a short and imperfect reaction, the most powerful excitants — mustard, ammonia, or turpentine — aided by heat and friction, should, without delay, be ap-

plied externally along the spine and to the extremities, and perseveringly continued, at short intervals, until the torpid sensibility is aroused. It is probable that, in such cases, the actual cautery, as employed by M. Rollet, will be found of advantage. This gentleman passes the iron, at a white heat, six, eight, or more times, upon as many different points, along each side of the spinal processes. He states that, in the worst cases, the first application of the actual cautery does not elicit from the patient any indication of sensibility; it is only at the third, fourth, or even fifth application that a slight muscular movement proves that pain is experienced. Some patients utter cries during the last applications, but immediately relapse again into a comatose condition.

Should we succeed in establishing permanent reaction, the patient must be carefully watched, and if the reaction transcend the proper grade, resort should be immediately had to general and local blood-letting, to an extent proportioned to the violence of the symptoms, and the age and strength of the patient; at the same time, cold applications should be made to the head, and the other means of keeping down excessive reaction employed.

The early and free exhibition of mercury, both by the skin and mouth, with the view of producing promptly its specific action, is favourably spoken of by several of the American writers on the disease. Dr. Ames, of Alabama, considers it a more efficient remedy than blood-letting, as well in the promptness as in the permanence of its beneficial effects. The French physicians condemn mercurial frictions; more, however, we suspect, from theoretical views, than from any actual experience of their bad effects. It is proper to remark that, in many cases, mercury, even when its specific effects have been induced early in the attack, has failed to exert any perceptible influence in retarding the fatal march of the disease. The same is true, however, in reference to every other remedy that has been resorted to in this disease.

Subsequent to the employment of venesection and the other antiphlogistic remedies, the administration of opium has been recommended by several of the French physicians. Forget commenced its use between the fifth and seventh days of the disease, in the form of a syrup containing half a grain of opium as a dose for an adult. This he found to relieve the pain of the head, and to calm the delirium and muscular spasms. M. Chauvart states that the early employment of the most energetic antiphlogistic means failed in his hands to cure the disease, but he found it to be promptly arrested by opium given in large doses: in many cases it was advantageously combined with quinine. It is stated that before this plan was adopted, only one case was cured out of thirty, but subsequently the disease was even less fatal than in its sporadic form. M. Tourdes admits the inefficacy of the usual antiphlogistic remedies, but cannot agree with all M. Chauvart has asserted in favour of the curative effects of opium. Dr. Ames, of Alabama, does not consider opium as generally a safe remedy in the more violent inflammatory cases, or as beneficial in the congestive malignant cases. In other forms of the disease, he speaks of it as a safe and very valuable remedy. We are informed by Dr. Roberts that, at St. Augustine, Texas, opium and morphia were tried in a few cases, but without any good result; they appeared rather to increase the stupor, without relieving the pain and restlessness. The water of the cherry laurel and of valerian, combined with mucilage, were employed by some of the French practitioners with a view to their sedative operation, subsequent to antiphlogistics and revulsives. The distilled water of bitter almonds is preferred by M. Maible, as furnishing more definite proportions of hydrocyanic acid.

By certain of the French army physicians quinine is recommended as a most efficacious remedy in cerebro-spinal meningitis; by the majority of them, however, it is denounced as positively injurious. Dr. Ames employed it frequently in the graver forms of the disease, and sometimes with partial success. He found it occasionally to arrest the paroxysms when the disease was attended by fever of a regular remittent character. In other forms of the disease, if not absolutely injurious, its effects were not such as to encourage a continuance in its use.

Dr. Ames speaks highly of the effects of potass in this disease. It was given to children in doses of from three to five grains, every two hours. He states that no case proved fatal, so far as he could learn, in which the potass was freely and continuously employed. Under the use of the remedy, in many cases unattended with true febrile symptoms, the cephalalgia was speedily and permanently relieved, and in

others, its use was followed by a prompt reduction of arterial excitement, delirium, and the intense pain of the head.

Ethereal inspiration, it is said, was practised, with the best effects, by M. Basseron, physician-in-chief to the Military Hospital of Mustapha, in Algeria.

During the period of excitement, cooling drinks should be allowed, and a strictly antiphlogistic diet enjoined. Absolute rest and quiet, with the seclusion of light, as far as it is consistent with due ventilation, are all-important. In the comatose cases, and during the stage of collapse, care should be taken to prevent an accumulation of urine in the bladder.

Convalescence from epidemic cerebro-spinal meningitis is usually protracted, and relapses are liable to occur from slight errors in diet and regimen, hence the greatest watchfulness is to be observed until the general health and strength of the patient are fully re-established.

Dr. Hicks found the annexed prescription to act as a most admirable tonic, after the violence of the disease had been subdued, for relieving the inertia of the nervous system that remained in every instance in which recovery took place: R. Iod. ferri, ℥j; iod. potass. ℥ij; iodini, gr. viij; syr. sarsaparil. ℥iv. Mix. Given in teaspoonful doses every four hours, in an equal quantity of pure water.—C.]

I proceed, in the next place, to a perfectly distinct class of diseases of the brain and spinal cord; to the apoplectic and the paralytic affections, arising independently of inflammation. It will be convenient to consider these disorders together. Apoplexy may indeed occur without paralysis, paralysis without apoplexy; but the two so frequently coexist, or happen in such immediate sequence and connexion, and in their history and pathology they have so much in common, that we shall best understand the whole subject by comprehending them in the same review.

When a person falls down suddenly, and lies without sense or motion, except that his pulse keeps beating, and his breathing continues, he is said to have been attacked with *apoplexy*. He appears to be in a deep sleep; but this is not all, for you cannot awaken him by the same means which would rouse a healthy man. He is not in a state of syncope, for his pulse beats, perhaps with unnatural force; and often his face, instead of being pale, is flushed and turgid, and his respiration goes on, though it may be laboured and stertorous. What I now denominate apoplexy, is the very same state which has so frequently been mentioned already in these lectures; it is *coma* occurring suddenly, or coming on (at least) with rapidity. What is coma? It is that condition in which the functions of animal life are suspended, with the exception of the mixed function of respiration; while the functions of organic life, and especially of the circulation, continue in action. There is neither thought, nor the power of voluntary motion, nor sensation: but the pulmonary branches of the par vagum continue to excite, through the medulla oblongata, the involuntary movements of the thorax. When this upper part of the cranio-spinal axis becomes involved in the disease, and its reflex power ceases, the breathing ceases also, and the patient is presently dead.

It is a common question—how would you distinguish apoplexy from the effects of a narcotic poison? If you were summoned to a person in the state I have been describing, how could you tell whether he was afflicted with apoplexy, or stupefied by a large dose of opium, or merely dead drunk? Why, so far as the condition of the cerebral functions is concerned, you cannot discriminate the one from the other. In each case there is profound coma: but the cause of the coma is different in each, and you must seek to ascertain that cause in the history and other circumstances of the patient: you inquire whether he is known to have been drinking, you try if you can perceive the odour of alcohol, or of wine, in his breath; or you endeavour to make out whether he has been low-spirited, or in known difficulties; in short, whether it is likely that he may have swallowed poison. But from the actual condition of his sensorial functions, you cannot solve the question.

Yet let me say, thus in the outset of our remarks upon apoplexy, that it is often of great importance that the diagnosis should be determined. A man was found lying in Smithfield in a state of total insensibility, and motionless, except that he still breathed. He was carried into St. Bartholomew's Hospital. The house-surgeon thought he smelt the smell of gin in his mouth; and thereupon very properly made

use of the stomach-pump. By means of it he discharged a large quantity of ardent spirit; and in the course of a few minutes the man revived, shook his ears, and walked away. If the gin had been suffered to remain in the stomach, and if the remedies of apoplexy had been vigorously put in force, the absorption of the poison would have been thereby accelerated; and the debauch would probably have had a fatal termination. The same remarks apply still more urgently to the case in which opium, or any other strong narcotic poison, is lying in the stomach. Even when there is no great danger, either in the person's condition, or in the remedies used for it, it is not a very pleasant or creditable thing to make a false point of this kind. If we do err, however, we had better err on the safe side. The father of the late Professor James Gregory, of Edinburgh, (who used to relate the case in his lectures), was once called out very late in the evening to visit an old gentleman of that city. He found him in a completely comatose state; his wife crying, and all his household plunged in grief and distress. They told him that the patient, whom he now saw in a fit, had come home, and upon the servant's opening the door to him, had fallen into the passage, on his back, in a state of insensibility. Dr. Gregory learned, however, that he had been at the "Club," and he knew well enough that this club was composed of choice spirits, fond of their cups; although the gentleman's wife did not know so much. Therefore he ventured to express his "hopes" to the wife that her husband was drunk: a charitable view of the case, at which she was extremely affronted and indignant. He persisted, however, in his opinion, and not long afterwards the patient began to recover his senses. It turned out that he had partaken more liberally than the rest of the club, and was the *first* to be intoxicated. Two of his companions carried him home quite incapable of motion; but not liking to introduce him themselves to his wife in that predicament, they placed him with his back against the door, rang the bell, and decamped. Of course when the servant came to open the door, his master tumbled senseless on the floor. I need not point out to you what ridicule the physician would have brought upon himself, and what damage he might have inflicted upon his patient, had he busily applied, in this case, the ordinary remedies of apoplexy.

The state of coma, such as I have described as being characteristic of apoplexy, may terminate in one of three ways. It may cease, more or less rapidly, and leave the patient to all appearance in perfect health. What may be the exact condition of the encephalon during the continuance of the coma, in such cases, no one can positively tell. But the occurrence of temporary coma, under the influence of a narcotic poison, and the perfect disappearance of the coma as the effects of the drug pass off, teach us that the functions of the brain may be almost totally suspended for a time by causes which do not injure its texture. I think it probable that, when there is no poison at work, a temporary stress upon the cerebral blood-vessels (produced by a determination of blood towards the head, through the arteries, or by a detention of blood in the obstructed veins) may exercise a sufficient degree of pressure upon the central parts of the brain to cause transient coma.

But often there is poison at work, an inbred poison, of which the agency was not recognised until a recent period; the poison of unpurified blood. When speaking of the general pathology of dropsy, I mentioned a peculiar renal disease — first detected and described by our distinguished countryman, Dr. Richard Bright — which unfits the kidney for what is probably its most important office; that of removing *urea* from the system. When this excrement, thus retained, accumulates in the blood beyond a certain amount, it is very apt, among other injurious tendencies, to cause death in the way of coma. The retention of bile, or of some of its principles, has occasionally, as it would seem, a similar consequence. In all cases, therefore, of apoplectic stupor, you must not omit to search for evidence of this source of the coma.

In the second place, the apoplectic coma may terminate, more or less quickly, in *death*. And on examining the brain we may find a large quantity of extravasated blood spread over its surface, or lying within its broken substance: or a considerable effusion of *serous fluid* collected within its ventricles: or we may detect *no* deviation whatever from the healthy structure and natural appearance of the organ. The congestive pressure (if it indeed existed) has left no prints of its action.

Dr. Abercrombie has given to that form of apoplexy, which destroying life, leaves no traces behind it, the name of *simple* apoplexy. And this name, for its conve-

nience, I shall retain. Of the other two kinds of quickly fatal apoplexy, that in which blood is found extravasated is more common than that in which there is effusion of serum only. The one has been called *sanguinous apoplexy*; a better term is *cerebral hæmorrhage*: the other has been named *serous apoplexy*.

Thirdly, the apoplectic coma may terminate in *partial* or *imperfect* recovery. One, or all of the cerebral functions may be left impaired; the mind enfeebled; the power of motion limited, or lost, in some parts of the frame; the faculty of sensation benumbed or extinguished: the unhappy subject of the attack remaining more or less crippled in body, and more or less maimed in intellect. In these cases, when at length we have an opportunity of examining the brain, we almost always find traces of damage inflicted upon its texture at the period of the attack. A part of this damage has usually consisted in the extravasation of blood to a small or moderate amount: sometimes, with or without extravasated blood, there is softening or disruption of the nervous substance. I shall return to these points presently.

The attack of apoplexy does not always occur in the same manner; and Dr. Abercrombie has pointed out three several ways in which it is apt to come on. I am confident, from the result of my own observation, that the distinctions laid down by Dr. Abercrombie are just and true: and it is of importance that you should be aware of them. "In the first form of the attack, the patient falls down suddenly, deprived of sense and motion, and lies like a person in deep sleep; his face generally flushed, his breathing stertorous, his pulse full and not frequent, sometimes below the natural standard. In some of these cases, convulsions occur; in others rigidity and contraction of the muscles of the limbs, sometimes on one side only."

Now respecting persons seen in this condition, the immediate prognosis is *uncertain*. Some die in a short time, and much blood is found extravasated within the cranium. Some die after a rather longer interval, and then we often find serous effusion only. And in some that die early, no effusion either of blood or of serum can be detected. Some recover altogether, without any ill effect of the attack remaining. Others recover from the coma, but are left paralytic of one side, and with some imperfection of speech, or of one or more of the senses. And this paralysis and imperfection may disappear in a few days, or gradually depart, or remain for life.

In the second form of attack, the coma is not the earliest symptom. The disease generally begins with sudden and sharp pain in the head. The patient becomes pale, faint, and sick, and usually vomits; and sometimes, but not always, falls down in a state of syncope, or resembling syncope, with a bloodless and cold skin, and a feeble pulse. This also is occasionally accompanied by some degree of convulsion. Sometimes he does not fall down, the sudden attack of pain being accompanied only by slight and transient confusion. In either case he commonly recovers in a short time from these symptoms, and is quite sensible, and able to walk; but the headache does not leave him. After a certain interval, which may vary from a few minutes to several hours—and Dr. Abercrombie records cases in which it was even much longer—the patient becomes heavy, forgetful, incoherent, and sinks into coma, from which he never rises again. In some instances, paralysis of one side occurs; but perhaps more often, there is no palsy observed.

The disease, when it comes on in this way, is much more uniform, and of much worse omen, than when it commences after the former fashion. It is of great use to know this; for to an inexperienced eye the cases do not *seem* so terrible as those in which the patient becomes profoundly comatose from the very first. The apparent amendment is fallacious, and apt to lead one into giving a false prognosis. Very few persons come out of the coma, and a large quantity of blood is usually found extravasated in the brain. These cases are not, as Dr. Abercrombie well observes, apoplectic in the outset. They differ remarkably from the first set of cases. If there be at the very beginning some loss of sense or motion, it goes off again in a very few minutes, or perhaps in a few seconds: the prominent symptom, at the commencement, is sudden and violent pain of the head, with faintness, sickness, and often with vomiting. The pain continues, and is sometimes confined to one side of the head; the face is pale and ghastly, the pulse weak, and often frequent or irregular; but the patient is quite conscious, and in full possession of his intellect. At length he recovers his natural temperature, his countenance improves, and the pulse becomes stronger and steadier: then his face gets flushed, he feels oppressed, answers questions

slowly, and at last sinks into stupor and fatal coma. The period between the first attack and the commencement of the coma is variable. Sometimes the stupor succeeds the pain and faintness so rapidly, that the case comes greatly to resemble those in which coma is the first symptom, and takes place suddenly; but still a short period of sense, commonly with complaint of great pain, may be observed. But the interval may be a quarter of an hour, or many hours, or even two or three days. "Upon inspection," says Dr. Abercrombie, "we find none of those varieties and ambiguities, which occur in the apoplectic cases, but uniform and extensive extravasation of blood." [I should state that he calls the first class of cases *apoplectic* cases, the coma being present from the first: and the second class, which we are now considering, he calls cases *not primarily apoplectic*.] The symptoms in this form of attack depend, no doubt, upon the giving way of some one of the cerebral vessels. At the moment when the vessel is ruptured, a shock is given to the brain; a temporary derangement of its functions occurs; but this passes off. The circulation then goes on as before, until such a quantity of blood has escaped from the ruptured vessel as is sufficient to produce coma. There is no part of Dr. Abercrombie's book more admirable, and clearly put, than that which is occupied with these important distinctions, which I give you very much in his own words. He points out the close analogy which exists between this variety of apoplexy, and the result of external injuries, when they occasion extravasation of blood on the surface of the brain. The hurt person recovers from the immediate effects of the accident, walks home perhaps, and after some time becomes stupid, and at last comatose. The surgeon trephines the skull, and discovers blood upon the dura mater; and the blood being removed, the coma goes off. We cannot help *our* patients by a similar expedient; though the opinion has been broached that trepanning the skull will, at some future period, be a common practice in apoplexy. Dr. Abercrombie conjectures that after the rupture has taken place, the hæmorrhage is sometimes stopped by the formation of a clot at the orifice in the vessel, but at length the blood bursts out again, and proves fatal. He relates two cases in which this probably happened; in one of them an interval of three days, and in the other an interval of a fortnight, elapsed between the first attack, and the super-vention of coma. The portions of blood extravasated at the two separate periods may sometimes be distinguished by their appearance — their colour and consistence.

The *third* form of attack described by Dr. Abercrombie can scarcely be said to be an *apoplectic* attack at all; indeed he himself includes this form in the class of *paralytic* cases. It is characterized by sudden loss of power on one side of the body, and frequently by loss or impairment of speech, without loss of consciousness. The patient is sensible, listens to and comprehends your questions, and answers them as well as he is able, either by words, which in most cases he articulates imperfectly, or by gestures. The further progress of the cases that commence in this way is marked by considerable variety. Sometimes the hemiplegia passes gradually in a short time into apoplexy. Sometimes the patient soon gets well, the palsy leaving him entirely. Or a gradual recovery takes place, which is not complete for some weeks or months. Or the patient rallies up to a certain point, and there the improvement stops; he regains the power of moving his leg, but it drags somewhat after him; or the leg recovers, but the arm remains feeble, or his speech continues to be inarticulate. And in another variety of this form the patient neither improves on the one hand, nor becomes apoplectic on the other, but is confined to his bed, paralytic, and perhaps speechless, though in possession of his faculties in other respects, and dies at last worn out and exhausted, some weeks, or months it may be, after the attack. In the outset of these cases there is not always complete hemiplegia, sometimes the arm only is affected, sometimes (but much more rarely) the leg only. Or some other voluntary muscles are the first to lose their power.

Dr. Abercrombie speaks of the conditions discovered after death in cases that have thus commenced, as being *inconstant*; but I believe that in a vast majority of instances they will be found to consist of *softening* of the brain and its consequences, whereby the communication between the centre of volition and the paralysed muscles has been suddenly cut off. With this softening there may, or there may not, be extravasation of blood.

You will find, I think, that most cases of apoplexy, or of palsy akin to apoplexy, range themselves with more or less exactness in one or the other of the three classes

which I have been describing. Let me briefly recapitulate them. In the first, the coma is sudden and deep; the condition of the patient thus struck in an instant senseless and motionless, warranting those epithets which the ancients applied to the victims of this disease, of *attoniti* and *siderati*, as if they were thunder-smitten, or planet-struck. In the second form of the attack, the earliest symptom is acute pain of the head, with sickness and faintness; coma *supervening* usually in no long time. The third form is ushered in by sudden hemiplegia, which may or may not lead to loss of consciousness, or stupor.

These broad lines of distinction, being kept in mind, will both furnish assistance towards the diagnosis and the prognosis of this multiform disorder, and also give to your study of it, as it occurs to you in practice, a higher degree of interest than it would possess if all the forms of attack had been jumbled together in one common description.

When the *apoplectic* state is *fully* formed, in what manner soever the attack may have commenced, it is marked by most or all of the following circumstances. The patient lies totally unconscious of all that may be going on about him. He replies to no questions, he is unmoved by the cries and lamentations of his family; in fact, he does not hear them. His pulse is infrequent, often full, perhaps intermitting. His breathing is peculiar, being slow, sometimes interrupted or irregular, attended with snoring or stertor during *inspiration*, and a puffing out of the cheeks, like the action of one who smokes a pipe, during *expiration*. Both these peculiarities are referable to the same principle, and both denote a profound insensibility to all external impressions. There is no longer any voluntary attempt to breathe, yet the involuntary movements of respiration subsist: the medulla oblongata still responds to the impressions which reach it from the lungs and from the skin, still prompts contraction of the muscles that enlarge the capacity of the thorax; but the loose curtain of the palate, and the lips and cheeks, are passive. By the vibrations of the one the stertor is occasioned; the mouth is closed by the mere elasticity of the others, and the flaccid cheeks flap outwards with the explosion of the air, as it escapes when the chest again collapses. The countenance is frequently turgid, and livid; the blood which tinges it is already but half arterialized; the pupils are commonly contracted; sometimes they are of unequal size. The limbs lie motionless: either they are all absolutely palsied; or (what probably is often the case) the capacity of motion remaining, the *will* to move them is wanting. If you raise one of them it falls passively down again, when you leave hold of it, like a *dead* limb. Sometimes, however, they are rigid and stiff. Sometimes one is stiff, and the others limber. And sometimes one or more of them, or those of one side, tremble, or are distinctly convulsed. You find perhaps that the patient is unable to swallow. If you put fluids into his mouth, they appear to choke him, or they run out again at the corners of his lips. His bowels are usually torpid; but if they act, the evacuations are passed in the bed without his knowledge or concern. His urine also flows involuntarily; or is retained in the distended bladder until it fairly *overflows*, and dribbles away perpetually.

When the attack terminates in death, that event is preceded, I believe in almost every case, by profuse perspiration, which bursts forth from every part of the surface, and is often cold and clammy. The pupils are sometimes at this period dilated, one perhaps more so than the other. The pulse becomes more frequent, the breathing more rare, and at last it ceases altogether.

In this description you will perceive that something more is included than pure coma. The absence of consciousness—implying the suspension of thought, of sensation, and of volition—marks plainly the affection of the cerebrum. The symptoms which diversify the apoplectic state, and distinguish one case of the disease from another, proceed from an associated or consecutive affection of the spinal cord. There may, indeed, be merely coma; profound and invincible sopor only. In this condition a morsel of food, or a spoonful of drink, passed far back into the pharynx, is instantly carried onwards by an act of deglutition: the excrements are duly retained, and duly voided: the limbs are simply passive and motionless; neither stiff, nor convulsed. But in the severe cases, inability to swallow, laxity of the sphincters, spasms, rigidity, tremors of the voluntary muscles—more or fewer of these adjuncts to the coma—are very apt to present themselves: and they denote, I say, the direct

or indirect extension of the morbid influence on which the apoplexy depends, to the cranio-spinal axis.

An easy and interesting criterion of the degree in which the reflex apparatus may be concerned has been pointed out by Dr. Hall. The orbicularis is the sphincter muscle of the eyelid. Touch the eye-lashes, and the lids involuntarily close; even during sleep the movements of the shut lids are apparent. If, in apoplexy, they do not respond to this mechanical stimulus, we know that the true spinal functions are gravely implicated.

On the other hand, many of the morbid phenomena just mentioned may occur, without any affection, from first to last, of the intelligence. But to these forms of disease, although their nature and essence may be the same, the term apoplexy cannot properly be applied.

This state, so appalling and painful to look upon, but fortunately so devoid of suffering for the patient — this suspension of the functions of animal life — depends, we have reason to believe, upon *pressure* applied to the brain, the organ subservient to those functions.

That excess of pressure is a *vera causa* is obvious, and that it is adequate to the production of coma is capable of demonstrative proof. It is not enough to show that they often exist together, for the coincidence might be casual. Neither does their occasional disjunction, real or apparent, furnish any conclusive argument against the general proposition, that coma, in many and in most cases, is the result of pressure upon the encephalon.

Coma may exist without pressure. In other words, coma acknowledges *other* causes also, *besides* pressure. It is produced by many narcotic poisons; by the circulation of venous blood through the arteries. In these cases we have no proof of any compression of the cerebral substance.

The other disjunctive condition is much more puzzling, and has led some persons to question or deny the general proposition. Can there be unnatural pressure, yet no coma? It would seem so. Serum, pus, blood, have been met with in the brain, foreign matters have penetrated the cranium, and coma has not ensued.

The force of this difficulty is lessened by the consideration that foreign substances may be present within the skull, without occasioning any preternatural degree of pressure. We read of bullets being carried about for some time in the brain. In such instances it is probable that a portion of the contents of the skull was forced out at the time of the injury: or that coma has come on, and gone off again, in consequence of the gradual absorption of the cerebral matter to make room for the foreign body. The same explanation may be applied to the chronic accumulation of water within the cranium, and to the slow growth of tumours.

Further, there is reason to believe that it is not on every part of the brain that the same degree of pressure made will produce the effect ascribed to it. It is stated in Mr. Mayo's Physiology, as the result of actual experiments on animals, that *lateral* pressure against the hemispheres of the brain produces no observable ill consequence; but that vertical pressure, pressure downwards, occasions stupor; which is probably attributable to compression of the central or deep-seated ganglia. Now it is obvious that some injuries of the brain may tend more than others to cause pressure in that direction.

The difficulty may not be wholly relieved by these considerations. But it is a difficulty which cannot invalidate the evidence of numerous facts that attest the agency of pressure, as, at least, one cause of coma. The presumption of such agency arises whenever coma immediately succeeds to pressure; and it is converted into certainty if, upon the removal of the pressure, the coma immediately departs. Now the annals of physic are full of instances of that kind. In experiments upon animals, stupor has been brought on, and made to cease, at the pleasure of the operator, by applying pressure to the exposed brain, and by remitting that pressure. Nay, the experiment has been tried on the human brain itself. A man who had undergone the operation of trepanning, and had recovered, was in the habit of exhibiting himself for money in Paris, where Haller saw him. He suffered the spectators to make pressure upon his brain, where it was covered by the integuments only. This always put him into a state of coma or deep sleep; but sensibility and the power of voluntary motion returned at once when the pressure was taken off.

A most remarkable example of the concurrence of coma from pressure upon the brain, and of the removal of the coma by removing the pressure, was afforded by a patient who was in St. Thomas's Hospital under the care of Mr. Cline. Mr. Green, who was Mr. Cline's nephew, was in the habit of relating the case in his lectures here. It is quite pertinent to my present purpose. One of Mr. Cline's apprentices was visiting the dépôt at Deptford, and discovered there a man who had been for some time in a state of unconsciousness : and he had him removed to St. Thomas's. His main symptoms were apparent insensibility to all surrounding objects, and a total incapacity to make any communication to those about him ; except that his attendants learned to infer, from certain instinctive movements or gestures, that he felt hunger, or thirst, or a want to relieve his bowels. His fingers were permanently bent towards the palm of the hand, and his eyes were turned upwards, so that the corneæ were completely concealed beneath the upper lids.

Upon examining this man's head, Mr. Cline found that there had been fracture with depression of one of the parietal bones. He trepanned that part, and elevated the bone. The patient seemed to feel the operation ; and as soon as it was concluded, his eyes and fingers were restored to their natural position. On the evening of the same day he sat up in bed, and though at first stupid and incoherent, soon became rational and well.

When he had entirely recovered his senses, it was ascertained that the last thing he remembered was his serving on board a vessel which made a capture off Minorca. He was wounded in the engagement, and carried afterwards to the hospital at Gibraltar. All this happened upwards of twelve months before the operation. So that one whole year of this patient's life was a complete blank, because, during that period, a little piece of bone was pressing upon his brain.

Cases of this kind show, very convincingly, the connexion that subsists between pressure on the brain and coma, and their relation to each other as cause and effect. The pressure and the coma begin together ; the coma continues as long as the pressure continues ; and it ceases when the pressure is removed. The old definition of the *cause* of a morbid condition is completely satisfied : "*Præsens morbum facit, mutata mutat, sublata tollit.*"

Observe, in passing, that in the case just described there was not only coma, but a permanent contraction also of certain muscles ; that these muscles did not through that long period of time become unfit for the exercise of their proper function ; and that the pressure was applied to the *periphery* of the brain.

From this digression—not altogether foreign to our subject—I return to the consideration of the pathology of apoplexy.

If the patient recover from the coma, he may live a few hours, or days, or he may live for many years. Sometimes, as the coma departs, all the natural functions are gradually restored ; but much more commonly paralysis remains. You already know that it is apt to affect one moiety of the body only. If a line be drawn from the vertex to the perinæum, dividing the body into two halves, which, so far as the exterior is concerned, are symmetrical, all the *voluntary* muscles that lie on one side will be found powerless ; or if they are not *all* so, those which are palsied are situated on the same side of the line. And this state of things is called *hemiplegia*. *Paraplegia*, that condition in which all the parts below a transverse line are palsied, though it sometimes results from cerebral disease, is much more commonly the consequence of mischief in the spine.

Hemiplegia, I say, may remain after the coma of apoplexy has passed away. But I have already told you that hemiplegia may occur without antecedent coma, and as a primary affection. The actual phenomena are the same in either case ; and a careful study of these phenomena you will find to abound with interest.

Hemiplegia, then, may be complete or incomplete. By complete hemiplegia I mean total palsy of most of the voluntary muscles on one side of the body, and especially of the muscles of the limbs of one side. The patient may *will* the motion of his leg, or arm, but they no longer obey the act of volition ; if they are lifted by another, and then let go, they drop down like logs of wood. You will find that, in well-marked cases, the *intercostal* muscles of the palsied side do not contract. Dr Todd makes this remark concerning the muscles of the thorax and abdomen, that they are seldom palsied in hemiplegia depending on disease of the *brain* ; but almost

always so in the rarer form of hemiplegia which sometimes results from mischief in the upper part of the spinal cord. The muscles of the face, also, are some of them inert on the same side. I have known many persons who have thought that the muscles of the *face*, in hemiplegia, when they were affected at all, were affected on the opposite side of the body from that to which the palsied *limbs* belonged. But they never could have examined actual cases of hemiplegia with any attention. How the error arose I cannot tell, but I have known a professed anatomist make it. I guess that it may have arisen from one of two causes. An anatomist who had not looked closely upon disease, would expect, and not unnaturally, that the face and limbs would be affected on opposite sides of the body; seeing that the nerves which supply the muscles of the face are given off above the place where those fasciculi of nervous matter which are called the anterior pyramids, decussate each other. And a common observer, who was not an anatomist, would be apt to conclude that the side towards which the mouth was drawn was the affected side: whereas it is just the reverse. The face is drawn to the healthy side, because the muscles on that side are no longer counteracted and balanced by the corresponding muscles of the palsied side. The *blank* half of the face is that which answers to the paralysed limbs. The muscles which cease to obey the will are generally those that are supplied by the third, or by motor branches of the fifth nerve: and the *rule* in hemiplegia is that these muscles are palsied on the same side with the muscles of the leg and arm. But there is no rule, they say, without an exception: certainly the exceptions to this rule are very uncommon. I have not had leisure to look over the records of the very many cases of this disease which my position as physician to a hospital has brought under my observation; but I do not recollect more than two exceptions; and one of them, as it happens, is now exhibited in the person of one of my patients in the Middlesex Hospital. Some of you have seen the woman. It is a well-marked exception: but in this instance the hemiplegia followed a blow on the head, and I suspect that a double injury was inflicted; that the palsy of the face results from mischief on one side of the brain, and the palsy of the limbs from mischief on the other. This I only conjecture, because the phenomenon is so rare.¹

Then, again, with respect to the *tongue*: when put out beyond the lips, its point is commonly turned to one side. To which side? Why *towards* the palsied side. For what reason? Clearly because the muscles that protrude the tongue are powerless on that side, and in full vigour on the other. That half of the tongue which corresponds with the sound side is pushed further out than the other half, and therefore the tongue bends to the palsied side. Such is the usual fact, and such the explanation of it. But there are more numerous exceptions to this than to the correspondence of the paralysis in the external facial muscles. Gaps in the row of teeth may regulate the direction of the protruded tongue. Sometimes it comes out straight; sometimes the patient cannot thrust it forth at all; and sometimes, even, it deviates towards the sound side. But the *rule* is as I have stated it.

This also has been noticed of the tongue in such cases; that the patient has been able, after some effort, to thrust it suddenly out, and then has required a certain interval of time before he could do so again; as if the spent nervous power were slowly regenerated. With these different affections of the tongue, the patient's speech is variously altered. His voice is thick, muttering, inarticulate, or unintelligible. Sometimes, even though he may be quite conscious and rational, he is unable to utter a syllable: or his efforts result in the constant use and repetition of some one inappropriate word or phrase; and he seems vexed at finding that his attempts to converse are fruitless.

Supposing the patient to recover, wholly or partially, from the paralysis, the *leg*, the face, and the tongue, in nine cases out of ten, ay, and in a much larger proportion than that, recover first and fastest: sooner and quicker I mean than the *arm*. And another fact, quite analogous with this, is that when one of the extremities alone is affected with paralysis, it is, in nineteen cases out of twenty, the *arm* that is so affected. I give you again the *rules*; they are liable to occasional exceptions. It is stated by Romberg that hemiplegic paralysis dependent upon *spinal*, and not upon *cerebral* mischief, is more persistent in the leg than in the arm.

¹ This patient died afterwards, at her own home; and no opportunity was given of inspecting her body.

This, then, is one way in which the hemiplegia may become, or be from the first, incomplete: viz., in *extent*. One limb may be powerless and the other strong. But the palsy may also be incomplete in *degree*. The patient may be able to move and use his limbs, but they are feeble. Such movements as he is capable of are commenced slowly; as though the effort of volition were not obeyed, as it is in health, on the instant. He cannot bend his fist firmly; nor lift his arm beyond a certain height. Or his leg feels heavy to him, and trails a little behind as he walks; he is unable to stand upon that limb; or to plant his foot securely, or with the usual precision. In short, there are innumerable gradations of paralysis, from slight weakness of the affected muscles to perfect immobility.

I have stated that the actual condition of the palsied muscles differs in different cases. Sometimes the limbs are loose and supple; bending backwards or forwards readily when moved by the hand of another, or by the sound hand of the patient himself. Sometimes, on the contrary, they are more or less stiff and contracted. They resist the attempted movement. You cannot extend or bend them much; and your endeavour to do so gives the patient pain. The rigidity may affect both the flexor and the extensor muscles, but it is principally seen in the flexor. It may present itself in both limbs, but it is most often manifest in the arm.

These facts have long been recognised. The light which they shed on the pathology of brain disease, and the guidance which they furnish in its treatment, have been but recently pointed out. We owe this valuable addition to our knowledge on these subjects, to your distinguished teacher, Dr. Todd. His doctrines are fully expounded in his volume of *Clinical Lectures on Paralysis, Disease of the Brain, and other Affections of the Nervous System*; from which I gather very much of what I am now about to tell you.

Rigidity of the limbs in hemiplegia may occur simultaneously with, or presently after, the palsy; and then it indicates "irritative disease" within the cranium. It may come on slowly, late in the course of the disorder, after perfect resolution and flaccidity of the muscles in the first instance; and then it shows that there has been "loss of substance of the brain, and that a cicatrix is undergoing contraction."

The rigidity that happens early in the disease may be slight and partial, one or two only of the palsied muscles being stiff while the others are flaccid; or it may be considerable, and affect nearly all the muscles. If you try to extend the patient's arm, or to straighten his fingers, you find that the biceps in the one case, and the flexores digitorum in the other, are, or at once become, stiff, and resist your efforts: and in like manner, though less strikingly, the triceps of the arm may forbid its flexion. The same may be said, *mutatis mutandis*, of the leg, when it presents similar phenomena. Sometimes, but not often, the muscles concerned in mastication are implicated. The patient, though insensible, resists powerfully any attempt to pull open his mouth.

This early rigidity is occasionally preceded or accompanied by convulsive movements. It is attended with little or no wasting of the affected muscles.

The rigidity that remains late in the disease, may follow either of the two earlier conditions—the lax or the stiff condition—of the palsied muscles. Like the former rigid state, it is sometimes slightly marked; while sometimes it is extreme in extent and in degree, the hand being carried up to the shoulder, the fingers bent into the palm, which is hurt by the growth of the nails, and even the heel fixed firmly against the buttock. Whatever its degree, this form of rigidity is *permanent*; and the rigid muscles, and the limbs to which they belong, gradually dwindle. Inaction of the muscles, according to the principle which I explained to you in a very early part of these lectures, leads to lessened nutrition, and a consequent diminution of bulk; in one word, to atrophy. Sometimes, however, the size of the helpless limb is maintained, or even augmented, by the supervention of œdema. The motion of the blood in its veins not being aided by the play of its muscles, the areolar tissue becomes infiltrated with serous liquid.

When in the outset of hemiplegia the palsied limbs are lax and flabby, there is both early and rapid wasting of their muscles.

Dr. Todd's theory, I repeat, with respect to the early rigidity, occurring with or without convulsive movements, is that it is connected with *irritation* within the cranium: with laceration of the brain, for instance, by a clot of extravasated blood—

with an inflammatory condition of its hurt, or around its softened, substance — with the mechanical irritation produced by depressed bone in fracture of the skull. And the occurrence of this early rigidity warrants the opinion that the cerebral mischief or irritation lies on, or not far beneath, the *surface* of the brain. The late rigidity he believes to accompany a gradual shrinking and contraction of the cerebral matter, during the process of repair set up in the torn or otherwise injured brain.

There is another mode of discriminating different conditions of hemiplegia, which Dr. Todd has, to the best of my belief, put upon its right footing. It is furnished by the results of the application of the stimulus of electricity to the paralysed muscles. He makes three classes of cases. In the first, and vastly most numerous class, he found the palsied limb to be affected slightly, or not at all, and always less than the sound limb. In the second there was no perceptible difference in the effect of electricity upon the two limbs. These were “cases of recent paralysis, the cause of which was not of a very depressing nature.” In the third class the electricity had a greater influence (yet never a much greater) upon the palsied than upon the sound limb. In nearly all these last cases the paralysis was accompanied by early rigidity of the muscles.

Dr. Todd's conclusions on this matter, which I give you in his own words, are “that when the paralysed limbs exhibit an early spastic or rigid state of the muscles, they will be more excitable by electricity than the sound limbs; but if the paralysis be accompanied by a state of complete resolution of the muscles, the sound limb is most excitable to the galvanic stimulus, and the paralysed limb is sometimes scarcely at all to be excited. In the latter case, the nerves of the paralytic limb are in a depressed condition; in the former they are in an irritated condition; and the different effects of electricity in the two cases will depend on the difference of cause of the paralysis. If the paralysing lesion be irritative, the paralytic limb will be more excitable by the galvanic stimulus; if, on the other hand, it be depressing, the paralytic limb will be less excitable; and thus this difference in the effect of electricity on the two limbs may serve to guide us in our diagnosis, and we may conclude that the lesion is irritative or depressing, according as the paralytic limb is more or less excitable by the galvanic stimulus.”

Besides the *palsy*, there is often *anæsthesia* also. But this is by no means so constant a symptom as the paralysis. The function of sensation (wherefore I cannot tell) is less frequently abolished or perverted than the function of voluntary motion. When the sensibility is lost, or blunted, or any how modified, it is so, commonly, in the same parts that are affected with paralysis. But sometimes there is anæsthesia, and no palsy; and, more strange still, there has been sometimes anæsthesia of one side and palsy of the other. As a general rule, the anæsthesia is less common, and less intense than the palsy; and is much sooner recovered from.

Tracing these cases onwards — such cases, I mean, as do not perfectly recover, — we find that the palsied limbs are usually colder than their fellows. This probably is owing to the diminished circulation of blood through the capillaries: there is not so much blood converted into venous from arterial; and less animal heat is developed. This has been observed even when the main artery of the part has beat as forcibly as in the corresponding part on the other side.

It is necessary to be aware that these palsied parts do not resist the influence of cold or of heat so well as the sound parts. When the sensibility is blunted, we can readily understand how the limb may become burned, from the absence of any warning pain that an injurious degree of heat is applied: but this is not all. A lower degree of temperature than would injure a sound part has often been found prejudicial to a palsied part: and if these palsied parts get chilled by frost, they more readily vesicate and inflame, on the return of heat, than other parts: merely *warm* water will sometimes act upon them like *scalding* water. I say a knowledge of this fact is of practical moment. That degree of warmth which the palsied limb fails to generate for itself, we must accumulate for it by warm clothing; and we must take care that it is never exposed to any artificial temperature which exceeds a certain point. We sometimes see mischief done by applying hot bottles or bricks — *too* hot — to such limbs.

The mental faculties are, in some few instances, quite unhurt by the attack: too frequently, however, they suffer irreparable damage. Of many persons, a striking

alteration is evident in the whole character and temper. The brave man has become timid; the prudent man foolish; the calm and cheerful man peevish and impatient. There is no longer the same power of attention, the same capacity for business, the same clearness and comprehension of thought. And whatever other changes may be observable, there are two ways, especially, in which the patient, after he has emerged from the coma, is very apt to be affected: viz., by a defection of memory, more or less partial; and by a peculiar tendency to emotion, particularly the emotion of grief: he will weep for very slight causes, sometimes long after the attack of apoplexy has passed over. This is very curious. I may tell you that the same readiness to shed tears, and to be immoderately affected by trifling causes of emotion, is sometimes noticed among the *precursory* symptoms of apoplexy.

LECTURE XXIX.

Apoplexy and Palsy, continued. Conditions of the Brain left visible after Death. Cerebral Hæmorrhage. Changes of and around the extravasated Blood. White Softening of the Brain. How produced. Parts of the Brain most commonly implicated.

WE were engaged with the subject of apoplexy. I requested your particular attention to the threefold mode in which that fearful disorder has been observed to make its attack. I described the apoplectic state itself, and endeavoured to elucidate the manner of its production. Lastly, I brought under your notice the varied phenomena of hemiplegia, and pointed out to you the inferences which have recently been drawn from the pliant or the stiff condition of the paralysed limbs.

Let us next inquire what morbid conditions have been disclosed by dissection of the brain after death from apoplexy and cerebral palsy; and what relations these conditions bear to the different outward manifestations of disease which were recounted in the last lecture.

I shall pass over those cases in which *no* morbid condition is detected, simply reminding you that a determination of blood towards the head, or a detention of blood within the head, sufficient, by tightening the full vessels, to occasion extraordinary pressure upon the nervous pulp, may account for the symptoms, and for the extinction of life: or the presence of some poisonous substance in the circulating blood (such as urea) may account for them.

I pass over likewise those cases in which serum only is found effused within the cerebral ventricles. A moderate quantity of serous fluid poured out rapidly during life would certainly exert a degree of pressure adequate to the production of fatal coma. How the serum comes to be so effused, it is not always easy to say. Yet there is one condition of the blood-vessels of the brain which, when it can be proved to exist in a given case, is sufficient to account for the effusion. Any real or virtual retardation of the blood in the cerebral veins would lead to what is tantamount to dropsy, *there*, as well as in any other part of the body; and intelligible causes of such retardation are known sometimes to be in operation.

A very common morbid condition in these cases is a quantity of extravasated blood — *cerebral hæmorrhage*. The amount, as well as the situation of the blood, varies greatly. Sometimes it is spread over the surface of the brain, on or between the membranes. Sometimes it is collected in one or more of the ventricles: but much more often it occupies the broken substance of the brain itself; and then, if its quantity be considerable, it generally forces its way either into the ventricles, or (less frequently) to the surface, or even in both these directions at once. In such cases, *apoplectic* symptoms, and death, are the invariable consequences of the hæmorrhage.

But when blood effused into the substance of the brain does not break a passage

out, either in the one direction or the other, its pressure is not necessarily or immediately mortal. The patient may survive for weeks, or months, or years; and the clot of blood may, in the meantime, undergo remarkable changes.

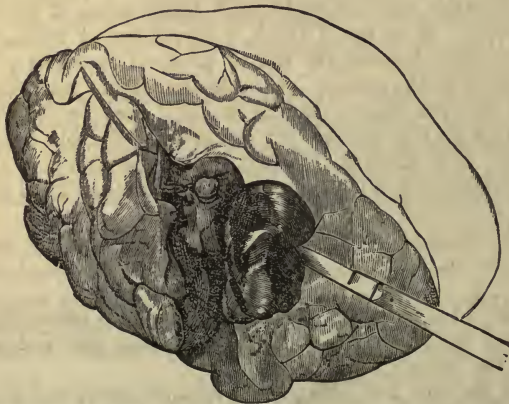
FIG. 27.



Apoplectic effusion upon the left side of the pons varolii.

Of course the cavity in which it lies varies in magnitude in different cases. It may be barely big enough to receive a large pea: it may be capable of containing a hen's egg. When examined not long after its formation, the clot is soft and of a dark colour; much like black-currant jelly. The sides of the cavity are irregular and

FIG. 28.



Hæmorrhage into the right lateral ventricle and right hemisphere, in a man aged 65. He was brought into St. Mary's Hospital in a state of profound coma, and died two hours after admission. There was a large ragged cavity in the hemisphere, communicating with the ventricle, from which about 4 oz. of black fluid blood escaped. The corpus striatum and thalamus opticus of right side were much softened. There was no apparent disease of the arteries.

ragged; and the cerebral substance of which they are formed is generally, to the depth of a line or two, moist, soft, and as if stained of a reddish or yellowish colour, which is fainter in proportion as it is more distant from the coagulum, and gradually loses itself in the natural tint of the surrounding parts. By degrees this stain disappears. The clot becomes more and more firm; assumes first a brownish, and subsequently a pale red, or even yellowish hue; diminishes continually in bulk; and at length may, I believe, be entirely re-absorbed. Meanwhile the walls of the cavity become less uneven, and clothe themselves by degrees, as they contract upon the shrinking coagulum, with a distinct membrane of a yellowish colour, sometimes of extreme delicacy, and resembling the serous membranes; sometimes thick and apparently fibrous. It is said that these cysts are capable of being obliterated by a process of cicatrization. This I have never myself seen. But they often remain filled

with a gelatinous or serous liquid; or traversed by threads, more or less numerous, of areolar tissue; or containing a small residue of the original clot of blood.

It is impossible to assign the precise period within which these remarkable changes may be accomplished. Dr. Abercrombie has detailed an instance in which a coagulum, that must have been of very considerable size, had entirely disappeared in less than five months. In another of his cases it was seen to be partially absorbed at the end of three months. "On the other hand, Moulin found a small coagulum not quite gone at the end of a year; and Riobé observed some of the blood still remaining in a cavity of small extent after twenty months. In two cases Serres found a hard coagulum of blood remaining; in one at the end of two, and in the other at the end of three years."

It frequently happens that a patient has suffered, during life, several distinct attacks of apoplexy or of cerebral hæmorrhage; and that as many cells are met with after death, exhibiting respectively various stages of that process of repair which has just been described.

These are the changes that mostly take place in the coagulum, and its containing cell, when the hæmorrhage does not prove fatal, and the patient recovers more or less completely. But the same changes do not always, or necessarily, occur. Instead of being gradually removed by absorption, the extravasated blood appears occasionally to become a solid, organized, and consequently living mass, deriving its nourishment from the arteries of the brain. A man, whose case is related by Andral, was smitten with apoplexy, and remained thenceforward, for many years, hemiplegic. At length he died, of some other complaint, in the wards of La Charité. When his brain was examined there was found, in one of the hemispheres, a mass of a pale red colour and fibrous appearance, traversed by numerous small blood-vessels which anastomosed with those of the brain: the surrounding nervous matter retained its natural aspect; and there was no appearance of any cyst.

I have yet to mention another, and a fatal consequence of hæmorrhage into the substance of the brain. It is not, I think, a very frequent consequence; yet it deserves attention the more, because the risk of its occurrence may perhaps be lessened by judicious treatment in the outset. The clot sometimes provokes suppurative inflammation of the cerebral matter around it: or it may be that the nervous pulp, being bruised or torn by the first violent irruption of the blood, suppurates spontaneously afterwards. It is affirmed (by what French author I forget) that the patient cannot be considered secure against this consecutive mischief until eight days of safety have elapsed from the period of the apoplectic seizure.

Instances of this result of cerebral hæmorrhage, according to my experience, are not, I say, very common. I have before me some memoranda of the last case of it that I saw.

I received, on the 3d of September, a note, written in a remarkably clear and neat hand, desiring that I would call upon the writer, as he had had a severe attack of apoplexy a day or two before.

I concluded that the note had been penned by some member of the patient's family, and I expected to see him in his bed, paralytic, probably, or manifestly ill. But I found a stout, active gentleman, walking about in his drawing-room, apparently in perfect health, and declaring that he felt so. He showed me, however, a paper written by a surgeon who on the previous day had brought him to town from a distance, and who had been obliged to return immediately. The paper stated that Mr. — had suffered a sudden and decided fit of apoplexy on the 30th of August; that he was then freely bled; that perfect consciousness was not restored, nor the force of the pulse subdued, till twenty ounces of blood had issued from his arm; and that on the evening of the same day sixteen ounces more were drawn.

My patient spoke of going down to his country-house, where he had, he said, "a good deal of shooting to do." I dissuaded him from this, and enjoined perfect quiet for at least a fortnight to come.

The next day, after a long and imprudent conversation with a friend, he suddenly lost the thread of his discourse, and could not recover it. Then he became confused and misapplied words. I asked him how he felt. He answered, "Not quite right," and this he repeated very many times, abbreviating it first into "not right," and at length into "n'ight." Wishing to mention "camphor," he called it "pamphlet."

I mention these as specimens. On the fifth it was evident that his right arm and leg were weak in comparison with the others; but their sensibility was unimpaired. By slow degrees the weakness degenerated into complete palsy, and the right side of the face became motionless. Gradually also he grew heavy, stupid, comatose, unable to swallow, with a fixed pupil; and so, on the morning of the 15th of September, he died.

We examined his head the next day. On the left side, the dura mater adhered to the skull-cap with morbid firmness. During the endeavours made to detach it, a table-spoonful, or more, of a dirty-looking, greenish, very offensive pus spurted forth. This was found to have proceeded from an abscess, which must have contained two ounces of pus, and which was situated in the upper part of the left hemisphere of the cerebrum. The walls of the abscess looked as if they were coated with a layer of yellowish plaster. In the centre of this cavity was a small, fibrous, tough mass of a dull red colour; the coagulum, doubtless, of blood effused on the 30th of August. In front of the abscess, the brain *seemed* natural, but its consistence was that of liquid custard.

Another very common condition met with after death, is that which is called *white softening* of the brain. The physical characters of such softening I have already described. Its *causes* remain to be considered. They may all be summed up under the head of defective nutrition: and in a great majority of instances the deficient nutrition is owing to disease in the cerebral arteries—to that fatty degeneration of which I spoke in a former lecture, and which begins, most probably, in the muscular element of those vessels. The change is not limited, in general, to the capillary vessels of the brain: but is distributed, under the shape of atheromatous patches, throughout various of the larger arteries of the body; in the aorta, in the iliacs, in the radial artery, where it may occasionally be *felt* in the living patient, in the carotid, vertebral, and basilar arteries, and in their ramifications. In consequence of these changes, parts of the brain are supplied insufficiently with nutrient blood, grow soft, and at length become utterly disorganized. The transmission of the nervous power is cut off by a breaking up of the road along which impressions leading to sensation travel in the one direction, and the mandates of the will in the other. *Paralysis* occurs: paralysis without loss of consciousness, for there is no necessary pressure.

This then is one mode in which white softening of the cerebral substance may happen, and give rise to palsy, and especially to hemiplegia. It is a gradual process, yet the palsy may be sudden. Degeneration of the cerebral substance precedes the final disruption of its fibres: and there are often symptoms of such gradual degeneration of the blood-vessels, and of the brain—premonitory symptoms of the palsy. Of these I shall say more as I go on.

But there is another way in which white softening of the brain, and its consequences, may be more rapidly produced. One or more of the larger cerebral blood-vessels may be plugged, or compressed, and the supply of blood to the corresponding portion of the brain suddenly shut off; and then that portion soon loses its consistency, and its organic structure, and liquefies into a mere pulp. Dr. Todd has recorded in the *Medico-Chirurgical Transactions* a case of this kind which I saw in consultation with him at Norwood. The common carotid on the right side was closed, and its channel rendered impervious, by blood which had dissected its way between the layers of the middle coat of the aorta, after passing through a transverse rent in that vessel. Paralysis of the left side of the body followed three days afterwards, with white softening of a large portion of the cerebral substance on the same side with the shut carotid. Similar mischief in the brain sometimes results from ligature of that artery for the cure of aneurism. And a very curious source of obstruction to the cerebral (or other) arteries, in consequence of disease existing in a distant organ, has recently been discovered and explained by Dr. Kirkes. I shall hereafter describe to you certain fibrinous excrescences to which the valves of the heart are liable. These are sometimes so soft that portions of them are broken off, and washed away by the circulating blood: and so may be carried in any direction till they enter some blood-vessel which they cannot pass through, and which they thus seal up. When this happens in one of the principal arteries of the brain, the necessary results are white softening and paralysis. The anatomical distribution of the arteries at the base of the brain explains why it is that a solid mass coming through the internal carotid is more frequently

arrested in the middle cerebral artery than elsewhere; and why the obstruction of that vessel at its origin shuts off nearly all their nutrient blood from those portions of the brain which are supplied by its branches. For reasons to be given hereafter, this accident is more likely to take place in the *early* periods of life: white softening from dilapidation of the blood-vessels in the *later*.

Hæmorrhage into the brain may occur with little or no previous alteration of the cerebral substance, which the blood then ploughs up and crushes; but it seldom does so occur except under external violence, or from the sudden spontaneous rupture of a *large* blood-vessel. I just now told you that the larger blood-vessels as well as the smaller are very subject to disease, and especially to that fatty and calcareous degeneration of their inner coats which is sometimes called atheroma, or, popularly, ossification. The disease shows itself in whitish patches of a round or oblong shape, which render the bore of the affected artery unequal in capacity, and its inner surface uneven. This variation of calibre impedes the free passage of the blood, and tends indirectly to increase its pressure upon the sides of the vessel. At the same time the coats of the artery lose their natural elasticity, become weak and frangible, and at length incapable of sustaining the impulse of the blood, and sudden hæmorrhage ensues. The arteries at the base of the brain are liable to *aneurism* also, and to consequent rupture. Morgagni has reported cases of aneurism affecting the internal carotid and basilar arteries. Serres has described a case of apoplexy resulting from perforation of the basilar artery, which was dilated, not far from its superior bifurcation, into an aneurismal pouch as big as a hen's egg. Dr. Baillie records an instance where both the internal carotids, on the side of the sella turcica, were distended into little aneurisms, one of the aneurisms being about the size of a cherry, the other somewhat smaller: and similar examples are related by other writers. I have seen two such myself: a beautiful preparation of one of them is preserved in the museum of the College of Physicians.

Again, white softening may take place without cerebral hæmorrhage: but neither is this very common, except from the sudden plugging or obstruction of a considerable blood-vessel. You will easily perceive how it is that the two conditions, the effusion of blood and the softening, are so commonly found co-existing. The softening, which is caused by the degeneration of the blood-vessels, combines with that degeneration in facilitating their rupture. The support which the healthy nervous substance afforded them is lessened and ultimately lost, till at length those fibres of the brain which hitherto had sufficed to convey, though perhaps imperfectly, its peculiar influences, either give way of themselves, or are broken down by a gush of blood from the fracture of minute blood-vessels. The character of the attack—the presence or absence of pressure upon the central parts of the brain, and of coma, the result of such pressure—depends much, as I have said before, upon the amount of blood poured out, and upon the place where it is shed.

No doubt softening of the brain may be produced by inflammation of its tissue: but the concomitant symptoms will generally enable you to discriminate between such cases and those of atrophic softening, which ensues from the defective supply of nutritive blood to portions of the brain.

It has long been known that hæmorrhage does not occur in all parts of the *substance* of the brain indifferently. Morgagni had remarked the frequency of sanguineous effusions in or near the corpora striata and optic thalami; and more extensive subsequent research has amply verified the general correctness of his observation. The same parts are also, I believe, more frequently than any others, the seat of atrophic softening. The explanation is that these are the most vascular parts of the brain. It has been noticed that the corpora striata are especially subject to laceration and sanguine effusion, while the surrounding parts remain unhurt, in violent concussions of the brain. And when injections are forced into the cerebral blood-vessels in the dead body, it is in the very same parts, the corpora striata above all others, that a sort of factitious hæmorrhage is produced by the rupture of vessels, and the escape of their contents.

LECTURE XXX.

Apoplexy continued. Relations between the Symptoms and the Appearances found in the Brain after Death. Special Diagnosis and Prognosis. Relations between Cerebral and Cardiac Disease. Predisposition to Apoplexy and Palsy—natural and accidental. Precursory Symptoms. Exciting Causes. Treatment.

I LEFT off in the last lecture, after having described the appearances which ordinarily present themselves within the skull, after attacks of apoplexy, and of cerebral palsy. In this description some account has been already given by anticipation of the connexion traceable between the physical injury sustained by the brain in such cases, and the outward symptoms. I proceed to touch upon certain points, relative to that connexion, which have not yet been noticed.

One of the most remarkable circumstances which dissection teaches us, when there has been partial palsy, is, that the palsy is on the *one* side of the body, and the hæmorrhage of the brain on the *other*. This is a very general law. But exceptions to it are said to have been observed. Morgagni mentions such. Dr. Bright has recorded a somewhat doubtful case of exception. I have never met with any: and I cannot help suspecting that in some of those which are said to have occurred, mistakes have been made: that either they have been incorrectly observed, or inaccurately described. You may consider the rule as almost, if not altogether, universal.

This crossing over of the morbid effect of the extravasated blood, or of any other diseased state, has long been attributed to that crossing over of nervous fibres which takes place at the upper part of the spinal cord. Just where the medulla oblongata and the medulla spinalis unite, the anterior pyramids decussate each other, and send their fibres mutually to the opposite side of the body. All this of course you know. The right anterior pyramid is continued into the centre of the left half of the spinal cord; and the left anterior pyramid into the centre of the right half of the cord. Now supposing, as we have every reason to suppose, that the nervous influence, whatever may be its nature, travels in the course of the fibres of the brain, we see in this decussation of the anterior pyramids an easy and pleasing solution of the phenomena in question. And Mr. Mayo, in his *Outlines of Pathology*, has made a happy use of two facts previously ascertained, which, taken together, afford a very neat *proof* that the transference of the morbid influence, or privation of influence, from one side to the other, actually takes place in that very part of the nervous system where the decussating fibres meet. The *facts* are stated by Dr. Yelloly, in the *Medico-Chirurgical Transactions*. Sir Astley Cooper divided the right half of the spinal cord of a dog, in the space between the occiput and the atlas; immediately, that is, after the cord has emerged from the skull through the foramen magnum: the result of this division of the cord was hemiplegia, paralysis of the limbs, on the *same side* with the injury. The bridge by which the morbid effect crosses over must therefore be *above* that point. We have got a limit on one side. And a case observed by Dr. Yelloly gives us a limit on the other. He examined the head of a man who had died hemiplegic; and he found a tumour, as big as a filbert, imbedded in and pressing upon the *right* side of the *annular protuberance*. The palsy had existed on the *left* side. The bridge of communication must consequently lie *below* that point. It must lie, therefore, between the two points now indicated; *i. e.*, it must be either in the medulla oblongata, or just at the junction of the medulla oblongata with the medulla spinalis. Now in this very interval, *and here alone*, a decussation of the nervous filaments is found to exist. There can be no doubt that the decussating fibres form the channel of communication.

But here we are met by a serious difficulty. How does it happen that the muscles of the *face*, and of the *tongue*—which are supplied by nerves that arise from the nervous centres *above* the place of decussation—how does it come to pass that these muscles sustain the same cross injury, and are paralysed on the same side on which the limbs are paralysed? And again, how does it happen (as it certainly does), that

hæmorrhage into the *cerebellum* should have a similar cross influence? These seeming anomalies have never been satisfactorily explained: but you must not forget that they exist.

The complex structure of the brain, and the dissimilar consequences that ensue, in different cases, from its injury or disease, lead directly to the belief not only that the organ subserves several distinct functions, but also that separate parts or sections of it hold peculiar and definite relations with other portions of the body. Ingenious men have even attempted to settle these points experimentally. By wounding or removing various portions in succession of the cerebral mass in living animals, and comparing the results, they have endeavoured to assign to each portion its particular province and function. But to say nothing of the remarkable differences which exist between the cerebral functions in man and in the inferior animals, there is an unavoidable source of fallacy common to all such experiments. We cannot reach the particular spot in the brain upon which the contrived injury is to be inflicted, without penetrating and hurting various other parts: and from these combined injuries (dangerous, indeed, and often fatal in themselves) arise symptoms which the experimenter may erroneously conclude to be characteristic of the lesion originally in his contemplation.

Much more accurate and satisfactory data for the determination of this interesting class of questions, would seem to be furnished by the spontaneous operation of disease, and especially of the diseases we are now considering. The injury done to the cerebral substance by the irruption of blood is often not less sudden, nor less mechanical, than in the experiments or contrived observations to which I have alluded. It is capable also, in many instances, of exact appreciation in regard to its extent; the parts which lie round the seat of the effusion remain undisturbed; and above all, the organ that is the subject of our observation is the *human* brain itself.

Attempts have accordingly been made to connect particular symptoms with the disorganization of particular parts of the brain. These attempts can boast, as yet, it must be confessed, but little success. Very few, if any, of the conclusions hitherto advanced upon this intricate subject can be relied on. Yet it is proper that you should be informed of them.

Because palsy of the arm is, in general, more complete, and more persistent, than palsy of the leg, it has been maintained that the former, the paralysis of the arm, is to be ascribed to hæmorrhage of the *corpus striatum*, which seems to be more common than any other; and upon similar grounds hæmorrhage of the *optic thalamus* has been supposed to determine paralysis of the *leg*. So much have these distinctions been confided in, that the honour of having first pointed them out has actually, in France, been made a subject of dispute. Now it is plain that one example of the contrary effect of these particular lesions, would suffice to upset the whole theory: but many such exceptions have, in fact, been noticed. With the view of settling this question, Andral collected and collated seventy-five cases of cerebral hæmorrhage, in each of which the clot of blood was sufficiently limited to allow of that case being applied towards the solution of the controverted points.

In forty of the seventy-five, both the leg and the arm were paralysed together. And where was the place of the hæmorrhage in these forty cases? Why, in twenty-one of them the *corpus striatum* was the only part injured; and in nineteen of them the *optic thalamus* was the only part injured. Thus you see, according to the theory just explained, in about one-half of the cases the arm alone *should* have been palsied; and in about half, the leg alone: whereas both leg and arm were palsied in them all.

Again, in twenty-three of the seventy-five cases the palsy was confined to the arm. Therefore, according to the theory, the injury should have been confined to the *corpus striatum*. What was the fact? Why, in this class of cases also there was as nearly as possible an equal sharing of the injury between the two parts. In eleven of the twenty-three the *corpus striatum* alone suffered; in ten the *optic thalamus* alone; in two the space between them.

Once more: there were twelve out of the seventy-five cases in which the leg alone was palsied. Consequently, in all of these twelve, if the theory were sound, there should have been damage of the *optic thalamus* only. But in ten of them the mischief was confined to the *corpus striatum*; in two only to the *optic thalamus*.

Gall had conjectured that the faculty of speech was placed under the governance of the anterior lobe of the brain: and Bouillaud has endeavoured to support that opinion by a number of facts observed in connexion with cerebral hæmorrhage; but Cruveilhier has brought forward several curious instances in which the loss of speech was a prominent symptom, while the disease was not found in the anterior lobe, but in some other part of the brain.

Andral, with his accustomed industry, has accumulated evidence upon this point also.

In thirty-seven cases of cerebral hæmorrhage observed by himself or by others, in which the morbid condition occupied one or both of the anterior lobes, the power of speech was abolished twenty-one times, and unaffected sixteen times.

On the other hand, he has collected fourteen cases, in which the power of speech was lost, yet no alteration had taken place in the anterior lobes. In seven of these fourteen cases the lesion was situated in the middle lobes; and in the other seven in the posterior lobes of the brain.

There can be no doubt that there *are* certain distinct parts of the brain which influence respectively the upper and lower limbs; inasmuch as they are often separately palsied: and since the loss of speech is occasionally the only, or the most prominent symptom, while in other cases the speech is not affected at all, we cannot but believe that this faculty is under the special guidance of some definite part within the cranium. But the facts that I have just been quoting, show, in the most convincing manner, that we are not able, as yet, to allot these separate functions to their proper spots in the cerebral mass.

In the account which I have endeavoured to give you of the symptoms of apoplexy, and of cerebral palsy, of the different modes in which the attack may commence, and of the various morbid appearances discovered within the cranium in the fatal cases, I have already embodied almost all that can be stated, with any confidence, respecting the special diagnosis and the prognosis of the disease. The one of these follows the other: the exact diagnosis being known, the prognosis is seldom difficult. By the diagnosis, however, I do not now mean simply the recognition of the disease as a case of apoplexy, or of hemiplegia. Of that *general* diagnosis, of the means of distinguishing the coma of apoplexy from the coma caused by opium or alcohol, I told you all that I know in a former lecture. Hemiplegia, when it exists, is incontestably evident to our senses. But I use the term diagnosis now in a stricter sense, and in reference to the distinctions that exist between *one case* and *another*; and I say that, in proportion to the accuracy with which we may be capable of determining the precise condition of the contents of the skull, will be the facility of predicting the issue of the complaint. Let me remind you, then, that when a patient suddenly becomes apoplectic, we cannot tell whether there be effusion of blood, or effusion of serum, or no effusion at all within the cranium: and therefore the prognosis must be precarious and uncertain. If, after the use of suitable remedies, the coma persist for many hours, the prognosis becomes worse. In those cases which begin with pain of head, faintness, and nausea, and which pass on to coma, the prognosis is positively bad; for the diagnosis is easy, and we are tolerably certain that a blood-vessel has given way, and that a large quantity of blood has ploughed up or compressed the substance of the brain. In the paralytic cases also, if coma supervene, the prognosis is gloomy: but frequently coma does not supervene, and then our prognosis, so far as life is concerned, may be pronounced favourable.

Among the symptoms that belong to the apoplectic condition itself, there are some which experience has selected as being most especially of evil omen; and it is well worth your while to remark that these discouraging signs relate, almost all of them, to the automatic functions of the cranio-spinal axis. The open, fixed, unwinking eye; the explosive flapping of the cheeks in expiration; the inability to swallow; the slow, sighing, interrupted breathing; the loosening of the sphincter muscles of the bladder and anus: these are fearful, and too often fatal symptoms, and they all belong to the excito-motory portion of the nervous system. Perhaps the profuse sweat that so often attends the process of dissolution may be referred to the same source; the whole tone of the various tissues being lost or relaxed. I would not say that no one of these

symptoms is ever recovered from : but I may say that of twenty patients in whom such phenomena occur, nineteen will die.

Now symptoms of this kind may be expected to arise, if there be hæmorrhage in or near the medulla oblongata ; or if there be mischief so extensive in the brain as to cause pressure upon the medulla oblongata. We should reason out the likelihood that such symptoms would be of bad augury. But the fact that they are so was ascertained long before the theory which accounts for them was devised. The fact is independent of the theory, and for that reason helps wonderfully to confirm it.

You cannot have failed to observe how closely, in the diseases which we have been considering, the pathology of the brain is interwoven with the pathology of the heart and blood-vessels. You must perceive how necessary it is, in every instance of apoplexy or of hemiplegia, to inquire into the condition of the apparatus of the circulation.

When sudden hemiplegia, with or without coma, occurs in advanced life—say after the age of fifty—in all probability evidence will present itself of disease in the heart, in the arteries, or in both. You may sometimes feel that the radial artery is unduly rigid, or of unequal calibre, in your living patient. If such evidence appears, you may conclude that there has been softening of the brain. Should there be early rigidity of some of the palsied muscles, you may infer that some irritative cause is in operation within the skull : and if the rigidity be extensive, and especially if it be attended with convulsive movements, the further inference will be warrantable that the cerebral mischief is not far from the surface.

Again, in the early periods of life, the sudden occurrence of hemiplegia or of apoplectic symptoms should suggest the suspicion of valvular disease in the heart ; and you would search accordingly for signs of such disease. Do not fall into the mistake, which has been made by pathologists of eminence, of assigning to apoplexy and hypertrophy of the left ventricle of the heart, when they meet in the same person, as they frequently do, the relation of effect and cause. It has been held that the powerful contractions of a ventricle thus morbidly strong may drive forwards the blood with such unusual force, as to strain and burst the cerebral arteries. Dr. Hope, in his elaborate work upon Diseases of the Heart, uses these words : — “ Instances of apoplexy supervening upon hypertrophy have been so frequently noticed, that the relation of the two, *as cause and effect*, is one of the best established doctrines of modern pathology.” Similar opinions have been expressed by the most distinguished of the French writers on this subject ; Andral, Bouillaud, Cruveilhier. I believe them to be entirely erroneous.

In the first place, hypertrophy of the left ventricle of the heart is very frequently, far more frequently than not, accompanied by other structural changes of that organ : changes which imply some impediment to the circulation : changes which involve or influence its right chambers also. In fact, disease of the right heart is not very often seen, without disease of the left : and one of the commonest forms of alteration to which the left side is liable, is hypertrophy of its ventricle. Now I have already pointed out to you the connexion which sometimes subsists between cerebral hæmorrhage and such disease of the heart as obstructs the ready and regular descent of the blood from the head through the *veins*. Many of the cases of apoplexy occurring in persons who have previously had cardiac hypertrophy are, I really believe, cases of this kind. The brain affection is dependent, in part, upon disease of the heart, but not upon the preternatural strength of its left ventricle. The heart acts morbidly upon the brain through the veins, and not through the arteries.

But there is another reason for the coincidence ; and here the arteries are concerned.

No one can doubt that the momentum, with which the blood reaches the cerebral arteries, in *healthy* persons, under violent bodily exercise or mental excitement, must often exceed the momentum produced by a hypertrophic heart in the cerebral arteries of persons who are tranquil and at rest. But apoplectic seizures are frequent under the latter circumstances, infrequent under the former. We must look, therefore, for something more than the mere hypertrophy to explain the coincidence. Now (supposing the absence of any check to the flow of blood from the head through the veins) that something is to be found in *disease of the arterial system*.

When the arteries of the brain are ossified, or changed, and rendered brittle in the way I yesterday described, the *commencement of the aorta* also is found, in a great

majority of cases, to be the seat of similar alterations; and, often, to be sensibly dilated. Now the fatty or calcareous deposit beneath its inner tunic must seriously impair the elasticity of the vessel; and in this way the free passage of the blood out of the heart will be impeded. Dilatation of the aorta at that part will produce the same hinderance more certainly and in a greater measure. Still more effectually and obviously will any narrowing of the outlet prove an impediment. It is in consequence of these mechanical obstacles to the free exit of the blood from the left ventricle, that the walls of that chamber, urged to more vigorous contraction, become thicker and more powerful. The hypertrophy is the natural compensation for the morbid state of the aorta; without it the heart would much sooner become unable to propel its contents at all: and the hypertrophy does not often, I fancy, become greater than is needful for its purpose. The strength of the left ventricle, therefore, in such cases, is not a true measure of the force with which the blood is driven into the distant arteries. Quite the contrary. It is a measure of the *difficulty* with which the blood is circulated through the *primary branches*, and therefore through the entire system of the arteries. It indicates the *diminished* force with which the blood is likely to reach the cerebral vessels. And in point of fact, you will find in *many* cases of hypertrophy of the left ventricle — I do not say in all, but certainly in very many — you will find the pulse at the wrist to be disproportionately small and feeble. So that, in these cases, instead of regarding the cerebral hæmorrhage as the *effect* of the hypertrophy (acknowledging, as I do, the frequent coexistence of these morbid conditions), I have been accustomed to look upon the apoplexy and the hypertrophy as *concomitant effects of the same cause*; viz., of disease pervading the arterial tree. The hypertrophy of the left ventricle is a consequence of the diseased condition of the aorta at its mouth; the cerebral hæmorrhage is a consequence of the same diseased condition of the arteries in the brain. When you find each of these lesions, and nothing to retard the venous current, you may, I believe, safely apply this explanation of the occurrence of apoplexy.

If you suspect, from the age and other circumstances of your patient, that he may be the subject of fatty degeneration of the blood-vessels, that suspicion will be strengthened by your finding, on inspection of his cornea, that it presents the *arous senilis*.

The classes of persons in whom, *cæteris paribus*, attacks of apoplexy are especially to be apprehended, are those whose *ancestors* have suffered the same disease; those who possess a *particular conformation of body*; and, above all, those who have reached a *certain period of life*. No doubt apoplexy may and does occur in persons whose progenitors have escaped it; in persons of every conceivable shape and make; and in persons of all ages. But it is *much more* frequent in the classes I have specified, than it is among persons not comprehended in those classes.

The first and the second class sometimes concur, *i. e.*, a particular conformation of the body is transmitted from parent to child, and with it is transmitted a proclivity to apoplectic disease. But even when there is nothing particular in their bodily form, or in their habits of life, old experience has clearly ascertained that they who come of an apoplectic stock are themselves more than ordinarily liable to apoplexy.

The pattern of body which is most prone to apoplexy is denoted by a large head and red face, shortness and thickness of the neck, and a short, stout, squat build. This remark is as old as the time of Hippocrates. However, apoplexy is common enough in men and women who are thin, and pale, and tall. *Cæteris paribus*, corpulent people are more in danger of apoplexy than spare people; but it attacks both the one and the other.

Advanced life is certainly a very strong predisposing cause, and the reason of this must be evident to you after what I have said respecting the dependence of these diseases of the brain upon previous disease of the blood-vessels. Earthy concretions in the coats of the arteries are so frequent in the later periods of existence, that they are met with, according to Bichat, in seven individuals out of ten who die beyond the age of 60; and Dr. Baillie considered ossification to be much more common in old persons than a healthy state of the arteries. Apoplexy and cerebral paralysis begin to be common after 50: but they do sometimes occur even in young children.

All these three kinds of predisposition are beyond our power. We cannot exterminate the hereditary tendency; nor remodel the plan upon which the body is con-

structed; nor arrest, or put back, the clockwork of human life. But we may guard and caution persons, thus predisposed by nature towards apoplexy, against many of its exciting causes.

A strong predisposition to apoplexy is, moreover, engendered by certain other diseased conditions; and over some of these conditions our art enables us to exercise more or less control.

One of these I referred to just now—the kidney disease discovered by Dr. Bright.

Diseases of the chest influence very materially and injuriously the circulation within the head. Without going into detail respecting complaints with which I am obliged to suppose that you are as yet unacquainted, I may state, by anticipation, that impediments to the free transmission of blood through the heart and lungs constitute the mode in which thoracic disorders predispose to apoplexy. The *plethora capitis* produced by such impediments is frequently visible in the turgid and livid features, and in the distended jugular veins.

The cessation of habitual discharges, of the catamenia, of bleeding piles; the drying up of old sores; the healing of long established issues and setons; all have an unquestionable tendency, by causing or augmenting plethora, to generate a predisposition to apoplexy.

And large observation of the habits of those who fall victims to this terrible malady, leaves no room for doubting that intemperance often paves the way for its invasion. The continued abuse of ardent spirits, in particular, lays the foundation of many of those morbid conditions of the sanguiferous system, and of the viscera, which constitute the predisposition we are now considering.

Among the *premonitory* symptoms *headache* is of frequent occurrence: but the same symptom is abundantly common in persons who are in no danger of apoplexy; it derives its minatory character from the concurrent circumstances. Headaches awaken our fears when they *begin* to be troublesome in advanced life. They are, then, still more formidable if they are accompanied by vertigo; or, without any other evidence of gastric derangement, by nausea and retching. Sometimes, as I just now told you, severe headache ushers in, and almost forms a part of, the apoplectic attack.

Vertigo itself, even without headache, is a very common precursor or warning of an approaching seizure. It is sometimes slight and transient; sometimes almost habitual. Although vertigo may depend upon other causes than mischief within the head, we cannot regard it without apprehension when it often occurs in old persons. It should teach us to obviate as entirely as we can all the known exciting causes of apoplexy. The principal of these I shall by-and-by describe to you.

Transient deafness, or transient blindness, blindness or deafness for a few seconds or minutes, is another of these warning symptoms. The late Dr. Gregory, of Edinburgh, used always to mention in his lectures the case of Dr. Adam Ferguson, the celebrated historian, as affording one of the strongest illustrations he ever met with of the benefit that may be derived from timely attention to the avoidance of those circumstances which tend to produce plethora and apoplexy. It is, perhaps, the most striking case of the kind on record. Dr. Ferguson experienced several attacks of temporary blindness some time before he had a stroke of palsy; and he did not take these hints so readily as he should have done. He observed that while he was delivering a lecture, his class, and the papers before him, would disappear, vanish from his sight, and reappear again in a few seconds. He was a man of full habit; at one time corpulent and very ruddy, and, though by no means intemperate, he lived fully. I say he did not attend to these admonitions: and at length, in the sixtieth year of his age, he suffered a decided shock of paralysis. He recovered, however, and from that period, under the advice of his friend, Dr. Black, became a strict Pythagorean in his diet, eating nothing but vegetables, and drinking only water or milk. He got rid of every paralytic symptom, became even robust and muscular for a man of his time of life, and died in full possession of his mental faculties at the advanced age of ninety-three; upwards of thirty years after his first attack. Sir Walter Scott describes him as having been, “long after his eightieth year, one of the most striking old men it was possible to look at. His firm step and ruddy cheek contrasted agreeably and unexpectedly with his silver locks; and the dress which he usually wore, much resembling that of the Flemish peasant, gave an air of peculiarity to his

whole figure. In his conversation, the mixture of original thinking with high moral feeling and extensive learning, his love of country, contempt of luxury, and especially the strong subjection of his passions and feelings to the dominion of his reason, made him, perhaps, the most striking example of the Stoic philosopher which could be seen in modern days."

This anecdote, which I have made use of as a wrapper for some medical instruction, will not be the less acceptable to you when I add that the remarkable man to whom it relates was the great-uncle of my friend and present colleague in this school, Dr. Robert Ferguson.

Very frequently slight and partial paralysis is the forerunner of an attack of apoplexy. Double vision is one form in which such limited palsy is apt to show itself. It is evidently connected with some degree of squinting; *i. e.*, some one or more of the muscles that move the eyeball are paralysed; the person cannot direct each eye to the same object at the same time. This is a very suspicious symptom. Dr. Gregory was acquainted with a sportsman who one day, when out shooting, disputed with his gamekeeper as to the number of dogs they had in the field. He asked how he came to bring so many as eight dogs with him. The servant assured him there were but four; and then the gentleman became at once aware of his situation, mounted his horse, and rode home. He had not been long in the house when he was attacked with apoplexy, and died.

Sometimes the slight and local paralysis shows itself in a faltering or inarticulate mode of speaking. The rapidity of the movements of the tongue requisite for distinct utterance is so great, that the slightest weakness of any one of its muscles is rendered obvious. We see this in one very common form of what may in truth be considered a kind of apoplexy; *viz.*, in drunkenness. In many persons the very first symptom of their becoming intoxicated is their inability to speak plainly. "Clipping the King's English," is the slang expression for it; and the same thing often takes place in respect to the more proper forms of apoplexy.

It is a curious circumstance, by the way, and one which is illustrative of what we meet with in disease, that different sets of muscles are chiefly affected by inebriation in different persons; the same set being always the first affected in the same person. Thus some men, when drunk, lose (as I have just stated) the proper command over the muscles of the tongue, and falter in speech, while they can walk very well: others reel and stagger, having lost, in a greater or less degree, the power of moving and governing their limbs, and of balancing themselves, who yet can speak quite fluently and plainly: and in a few cases, drunken persons become delirious, who still retain the power of distinct articulation, and of directing their steps aright. This being so, we need the less wonder at the variety in the nature of the warnings that precede the apoplectic attack.

In many instances there is numbness or debility, or total palsy of one limb, or of a single finger, or even of a solitary muscle, as of the levator palpebræ. The patient cannot grasp your hand with firmness, or sign his name in his usual way, or pick up a pin, or snuff a candle, or manage an obstinate button, or tie a knot in a thread cleverly: or, perhaps, one of his eyelids droops, and the eye is half closed. Sometimes, on the contrary, the patient stares at you frightfully, with one eye, which he cannot shut.

The numbness also assumes various characters, according to its place and degree. One patient will tell you that he feels as if one of his limbs were muffled in flannel; another, that he is uncertain whether, in walking, his foot has reached the ground or no. A gentleman, since dead of apoplexy, assured me that, when sitting, he did not know how far his breech covered the seat of the chair. With the numbness there is often associated a degree of tingling: that familiar sensation of "pins and needles," in a part which is recovering after being "asleep," from pressure upon the trunk of its ministering nerve.

All these symptoms are modifications of the function of voluntary motion; or of the function of sensation. Nor are manifestations wanting, among these precursory circumstances, of a derangement of the other and nobler function, of which the brain and nervous system form the material instrument. I mean the function of *thought*.

Thus one very deplorable warning is the loss of *memory*. All persons find, as they grow older, that they do not retain so tenaciously in their recollection things

which have recently occurred, as things which happened when they were young. This partly depends upon the degree of *attention* which we pay to different circumstances. Those events which strongly excite the curiosity, and rivet the attention of the boy, become familiar to the man, and he gives them but little notice, and is very apt to forget them. But the loss of memory that threatens apoplexy is something more than this. It is sometimes partial, and extends to certain sets of things only. For example, some persons entirely forget certain words, while they recollect others perfectly. Common words are often thus forgotten, while unusual or remarkable words are remembered; or a wrong word is chosen. One word is used for another that sounds something like it. Thus one of my patients, meaning to accuse a certain individual of *perjury*, always called it *purging*: and many other words he changed after the same fashion. But in truth the modifications of a partial loss of memory that have been known to precede apoplexy are both odd and endless: some people forget their own names, or the names of their children. Dr. Gregory, who had paid particular attention to these precursory symptoms, and who had a large practice for a great number of years to furnish them, used to mention a case of this kind. After some efforts his patient could recall to his recollection what his christian name was, but he could not think of his surname. About twelve months after his memory began to fail in this strange manner he was found dead in his bed. Another gentleman for some time before his death could never recollect the name of the street in which he lived. Upon one occasion of his visiting Edinburgh, he called on Dr. Gregory, and partook of a hearty breakfast, having forgotten that he had breakfasted before he came out. On the same day he attended, with Dr. Gregory, the funeral of a young lady who had been his ward; the funeral took place in the country; and when they returned together in the carriage it was evident that he had forgotten all that he had been doing. Next day he met the doctor in the street, and saluted him with all the kindness of an old acquaintance at first meeting; saying he was happy to have fallen in with him now that he was in town, and totally forgetful of their recent interviews.

Connected with this failure of memory, there is often an unnatural degree of drowsiness. Sometimes without any permanent affection of the memory, there is a temporary confusion or suspension of thought; the patient suddenly loses the train of ideas with which his mind had been occupied; stops short in the middle of a sentence, and endeavours in vain to recover the broken thread of his discourse.

Among the mental conditions that bespeak a tendency to apoplectic disease, I have several times noticed a strange and vague *dread*, of which the person can give no reasonable explanation; a sense of apprehension and insecurity not accounted for by the apparent state of his general powers and functions; a painful degree of indecision and irritability; with a dislike and fear of being left alone. One patient of mine described his "nervousness" of this kind, by telling me that in descending a staircase, especially a winding one, he was obliged to turn round, and come down backwards, as one descends a ladder; or even to sit down, and so slip, stair by stair, from the top to the bottom. Yet with the assurance given him by a friend's arm, or by a convenient baluster, he could walk down stairs without difficulty. He had no actual vertigo.

All these, and many other signs that are apt to precede and herald an attack of apoplexy, are well worth your study. They show that, even before the stroke descends, there is some morbid process going on within the head. The great use of being acquainted with these warning circumstances, and of looking out for them, consists in the opportunity and the *authority* which they furnish, for enforcing upon the person in whom they manifest themselves, the absolute necessity of avoiding all the avoidable *exciting* causes of the disease. But our means of advising him will be very imperfect if we have not carefully considered what these exciting causes are. I propose to devote a few minutes, therefore, to the consideration of the circumstances that are apt to *bring on* the attack. There are many cases of apoplexy and of cerebral hemiplegia, in which we cannot trace the operation of *any* such causes: but in many other cases their influence is decidedly marked; and the avoidance of them, while it is important to all who show a disposition to such diseases, is especially so to those who, having once suffered an attack, have reason to dread a *repetition* of it.

In the first place, anything which is calculated to hurry the circulation, and to

increase the force of the heart's action, is likely to operate as an exciting cause of apoplexy, or of cerebral palsy: simply by augmenting the momentum of the blood against the sides of the cerebral vessels, which in advanced life are so often diseased and weak. Strong bodily exercise, therefore, is a thing to be avoided by all persons in whom the predisposition to these disorders has declared itself. It is of much importance to make patients aware of this; for many persons think, when they labour under uncomfortable bodily feelings of any kind, they may get rid of them by a brisk walk; or by galloping some miles over the country on horseback.

Another dangerous state for such persons arises whenever the free escape of the blood from the head is suddenly obstructed. I have adverted to this before. Certain diseases, chiefly thoracic, which tend to keep the veins of the head inordinately full, rank among the *predisposing* causes of apoplexy. But, upon the very same principle, various conditions, which are temporary only, may operate as *exciting* causes. By what is called "holding the breath," whether upon an inspiration or an expiration, the transit of the blood through the lungs is impeded: and the check is felt (through the pulmonary artery, right chambers of the heart, and great veins) in the vessels of the head. And this effect is increased when *straining* is at the same time performed; that is, when a deep breath is taken and retained, while some muscular forcing effort is made.

Under this principle fall a number of bodily acts, which, however harmless in a healthy frame, are not without peril to a person having a predisposition to apoplexy. The motion of the blood in the lungs, and therefore in the head, is checked in the acts of coughing, vomiting, sneezing, laughing, crying, shouting, and so forth. You cannot have looked at a person in a violent paroxysm of coughing without seeing that it produced a determination of blood to the head, or rather a congested state of the veins of the head. The jarring pain in the head which is apt to follow each succession of the cough depends upon this principle: which is often strikingly illustrated in young children labouring under whooping-cough. They turn purple in the face, and become giddy; and not uncommonly ecchymosis of the conjunctiva occurs, giving fearful intimation of what might just as readily take place *within* the cranium. It is not very unusual for the whole of the white part of the eye to become suddenly bloodshot in these violent fits of coughing; and convulsions even have happened under the like circumstances.

Straining at stool is a common exciting cause of apoplexy in those who are predisposed to it. And this is one of the worst dangers attending costiveness of the bowels in old people: but it is one which it is often in our power effectually to obviate. It is more within our control than a bad cough could be. Any kind of straining indeed is equally perilous. A very good proof of this danger was recently afforded by a patient of my own. He was attacked with apoplexy on his way to Ascot races; and upon recovering somewhat, was found to be paralytic on one side of the body. He was brought back to town, where I saw him. After some time he regained the power of using the affected limbs to a very considerable extent; so as to be able to walk about, and follow his business, which was that of a job-master, or proprietor of a livery stable. I cautioned him seriously, *inter alia*, against straining: but I suppose he forgot my caution. For, while dressing one morning, he tugged violently in attempts to pull on a damp boot, and in the midst of his efforts fell back insensible: and from this relapse he never fairly recovered.

To the same principle are to be referred a variety of things from which a patient, in danger of this disease, must most carefully abstain. Lifting heavy weights; leaping; striking a hard blow; playing on wind instruments; even long and loud talking. Dr. Abercrombie relates two instances of fatal apoplexy brought on (as it would seem) by a sustained exertion of the voice: one of the attacks happened to a clergyman during the delivery of his sermon; the other to a literary man while speaking in a public assembly. In both cases a large quantity of blood was found extravasated within and upon the brain. Dr. James Gregory used to mention a patient of his, an officer in the army, who had apoplexy, and in whom the attack had been preceded by pains of the head, and giddiness, upon his giving the word of command, and particularly when dwelling upon the last sound; that is, when he made a long expiration. Precisely of the same kind is a case told by Van Swieten, of a singer who was obliged at length to abandon her vocation by reason of gradually increasing vertigo

whenever she had to hold a high note. Violent emotion is another exciting cause. Large fires, crowded rooms, the heat even of the sun, favour the access of apoplexy, and therefore ought to be shunned by those who have a tendency to that disease. The warm bath is not without hazard to such persons. This is so well known, I understand, at Bath, that the physicians there will not allow paralytic patients, in whom the paralysis has been connected with apoplexy—hemiplegic patients, for example—to go into their hot baths. The excitement of drunkenness, and the venereal excitement, are not uncommon causes of apoplexy, especially in old persons. I had a man of middle age under my care during the spring of 1837, in whom a most awful attack of apoplexy came on under circumstances such as I have just referred to. He had dined at a large festive party, and afterwards accompanied a woman with whom he was acquainted to a brothel; and he was struck with palsy during the act of intercourse. He was long unable to speak; and he still remains, and probably will ever remain, a cripple: incompletely hemiplegic.

I have been since consulted upon the case of an old gentleman residing in France, in whom an attempt at sexual connexion was attended with similar consequences:

“The Gods are just, and of our pleasant vices
Make instruments to scourge us.”

Posture again has no small effect upon apoplectic people. Giddiness, and some degree of confusion of thought, are apt to be occasioned, in most persons, by long stooping. There is one peculiar posture or position mentioned by Dr. Fothergill as being very unsafe, especially for short-necked persons—viz., that position which is assumed when we turn the head to look backwards for any length of time without turning the rest of the body; in fact, a twisting of the neck. In this attitude the jugular veins are more or less obstructed. He gives an account of a man who was seized with apoplexy as he was crossing the Thames in an open boat: he having kept his eye fixed upon a particular ship until, and after, he had been rowed past her. On the very same principle tight ligatures worn about the neck, and compressing the jugular veins, may bring on apoplexy; the wearing a tight neckcloth, for example. A continental writer informs us that a Swedish officer, who was desirous that his men should look well in the face, required them to wear tight stocks; and the consequence was that in a short time a great many in that regiment died of apoplexy. A similar unwise requirement in our own army regulations has of late, I believe, been abolished. Dr. Abercrombie quotes from Zitzilius the case of a boy who had drawn his neckcloth very tight, and was whipping a top, stooping and rising alternately. After a short time he fell down apoplectic. The neckcloth being loosened, and blood drawn from the jugular vein, he speedily recovered.

There is one very powerful exciting cause of apoplexy, in those predisposed to it, which I need only refer to now, because the facts that have been observed in proof of its agency were fully detailed in a former part of the course; I mean exposure to cold. You will recollect my telling you that the number of deaths in London from apoplexy and palsy in the month of January, 1795, which was a bitterly cold month, very much exceeded the number in the month of January, 1796, which was a remarkably mild month. The cold operates in two ways, in the production of apoplexy. In the first place, it drives the blood from the surface, and accumulates it in the large vessels of the interior of the body, and so increases the stress upon the cerebral arteries. And in the second place, the cold has a great influence in causing or aggravating affections of the *chest*; and the return of the venous blood from the head is impeded, in the manner just now explained, by fits of coughing and obstructed respiration.

This influence of external cold, and probably certain barometric conditions also of the atmosphere, help to explain, what I am sure I have several times had experience of, namely, the epidemic prevalence, now and then, of apoplectic seizures.

The older writers entertained some very false notions in respect to the distinction between sanguineous and serous apoplexy. They laid it down that apoplexy resulting from extravasation of *blood* within the cranium was denoted by flushing of the face, and strength of the pulse; and that it was a disease of persons in the vigour of life: while apoplexy resulting from the effusion of serum was marked by paleness of the countenance, and weakness of the pulse; and occurred in the old and infirm: and

they directed their practice according to this distinction. After what has already been said, I need not tell you that this classification of apoplexies could not have been founded upon the actual observation of disease : and that our treatment, now-a-days, is not regulated by any such erroneous theory.

Nevertheless, I do not mean altogether to praise the modern practice in apoplexy; for it is often one of mere routine. Practitioners are too apt, in this as in other instances, to be guided in their choice of remedies by the *name* of the disease, and to treat all cases of apoplexy alike. I remember being much amused by the perplexity which a friend of mine once told me he had felt on being summoned by letter many miles into the country to see a gentleman who had been struck with apoplexy. As he posted down he earnestly revolved in his mind what he might be able to advise when he should reach the house of sickness. He felt confident that the patient must already have been copiously bled; cupped, or leeches; blistered; and thoroughly dosed with calomel, senna, and croton-oil. Mustard poultices had doubtless been applied to his legs. My friend was distressed to think that while much would be expected, nothing would be left for him to do worthy of so long a journey, and so heavy an expense to his client. A clyster of turpentine might yet, perhaps, be an untried expedient. His cogitations were cut short, however, and his cares relieved, by an express which met him half-way on the road, to announce that the patient was dead. Now this is the routine of which I speak: most proper in some cases; unnecessary in others; pernicious in many. There are persons who seem to think that they have not done their patient justice if any part of this active intermeddling has been omitted. Others regard depletion as being always worse than useless, and trust entirely to stimulants and cordials. These are quite as dangerous *routiniers* as the others; but they are fewer in number.

Our practice would indeed be much easier than it is, if we could thus make one plan fit all cases which are, nominally, the same. But I need not, now, tell you that diseases alike in name—aye, and alike in their essential nature—are often widely different in their circumstances. I formerly explained to you that certain symptoms tell us what the disease is; but that we are often obliged to look to other symptoms, which may inform us what we are to do. I know of no rule so likely to guide you aright as that laid down generally by Cullen, of *obviating the tendency to death*. You must examine and judge to which of the several modes of dying there may be any manifest approach. If the tendency be, as in cases of apoplexy it mostly is, to death by *coma*, then blood-letting and the evacuating plan will often be requisite. If, on the other hand, the tendency be to death by *syncope*, you must withhold the lancet, and even have recourse to stimulating and restorative measures. The question is of the last importance; involving often (as Celsus taught) the alternative of life and death: “*sanguinis detractio vel occidit, vel liberat.*” Now the distinction between these modes of dying is to be made by attending to the state, not so much of the nervous, as of the sanguiferous system. Insensibility and unconsciousness are common both to syncope and to coma: and cases which fall under the class of apoplexies, and which we cannot separate from that class, are sometimes really more like cases of concussion than anything else; the shock having been of internal instead of external origin. If the pulse be full, or hard, or thrilling (sometimes it feels like a tense vibrating rope), or if there be obvious external signs of plethora of the head, you must abstract blood. You are not to refrain from bleeding the patient because he is pale, if his pulse warrant it; nor may you omit taking blood if the head and face be turgid, although the pulse be small; for that smallness may depend upon organic disease of the heart.

On the contrary, if his skin be pale and cold, and his pulse feeble and flickering, you would probably ensure your patient's death, or determine the accession of palsy, if you withdrew from the failing heart and blood-vessels a portion of their natural stimulus. I can only invite your attention to these broad features of distinction. Being once taught to look for and attend to them, your own judgment must instruct you as to what may be needful in particular cases. To this, as to most other diseases, the remark of Boerhaave is strictly applicable, who declares that he knows of nothing which can be called a *remedy*, “*quin solo tempestivo usu tale fiat.*”

Having made up your mind as to the general indications of treatment, you will pursue them steadily in detail. If the patient to whom you are summoned be stupid

and drowsy rather than faint, and his pulse and appearance warrant the conclusion of *plethora capitis*, the first thing to be done is to place him in a semi-recumbent position, with his head and shoulders raised; to loosen any tight parts of his dress, especially his neckcloth and shirt-collar, and whatever might press upon the *neck*; and then as quickly as possible to bleed him from the arm. We know that in some cases the apoplectic state occurs, when as yet no injury has been done to the brain; no effusion, no laceration of its texture; and we may hope, by timely and active measures, to *prevent* these terrible evils. We never can be sure that there is blood extravasated in such cases, and we must act, in the first instance, upon the presumption that there is not. We are especially encouraged to take away a considerable quantity of blood by venæsection when we perceive external signs that the vessels of the head are full: redness and turgescence of the face, throbbing and prominence of the temporal arteries, distension of the superficial veins of the neck and forehead. Our object is to take off the strain upon the internal vessels by bleeding in such a manner and to such an amount as shall produce a decided effect upon the general circulation. Sometimes the good consequence of the bleeding is very marked indeed, so that no doubt of its propriety can be entertained; the patient being so insensible as not to feel the puncture of the lancet, and yet emerging from his coma while the blood is still flowing. It is seldom, however, that we can expect such immediate and manifest melioration as this.

After one *sufficient* bleeding from the arm, the vessels of the head may be further relieved by cupping the nape of the neck, or the temples; and venæsection may be repeated if the condition of the pulse, and the symptoms generally, should require its repetition. It is seldomer, however, in cases of apoplexy than in cases of acute inflammation, that a second or third recourse to the lancet becomes advisable, unless, indeed, the first blood-letting has been mismanaged. Enough blood must be taken, in the first instance, to produce some evident effect; and therefore no precise rules can be laid down respecting the absolute quantity to be drawn; nor can we make any sure estimate beforehand as to the whole amount of blood which it may be necessary to remove.

Even if we could be certain that a blood-vessel had given way, and that blood was already poured out upon the brain, there are good reasons why (no adverse circumstances withstanding) we ought at once to bleed our patient. I will enumerate briefly the benefits we seek to obtain by the abstraction of blood in such cases.

1. The effusion from the ruptured artery may be slowly going on. Bleeding from a vein, so as to make a sensible impression on the general circulation, will diminish the stress upon the cerebral blood-vessels, and so tend to put a stop to the hæmorrhage. Both of these two objects are of primary importance.

2. By early and free bleeding we lessen the hazard of inflammation supervening upon the mechanical injury done to the brain by the sudden tearing and contusion of its texture by the effused blood; and

3. We thereby bring the system into the most favourable condition for the rapid absorption of the extravasated blood, and for expediting the patient's recovery from those symptoms which depend upon the presence of the clot in the brain.

But although, in that form of disease which we are now considering, bleeding is our sheet-anchor, it may be carried too far, or repeated too often. We must not lose sight of the fact that many of these patients are old, and will not survive undue depletion; and that if they survive at all, they will need all the strength that we dare suffer them to retain, for carrying on the vital actions, when the chief instrument of the most important of the animal functions is so greatly damaged: nor of the fact that if there be blood extravasated, we cannot touch it, except indirectly, by the abstraction of more blood from the arm: nor of the fact that a patient may be bled into convulsions, and fatal syncope. In short, after the first bleeding, you must be guided by the special circumstances of the case, and particularly by the pulse. The woman at present in the Middlesex Hospital, with paralysis of the limbs on one side, and of the face on the other, attributes her palsy (erroneously most likely) to her having been cupped. She had had a blow some weeks before, and suffered head-ache from that time. At length she was cupped, from the neighbourhood of the head: and the next morning she was paralytic. This might have been an accidental coincidence. But I remember being sent for a few years ago to see a patient at Greenwich,

who had already three physicians about him, and was apparently in danger of apoplexy, of which he had for some time experienced distinct warnings. The three physicians had agreed that he ought to be cupped from the back of the neck; to which I assented; and while blood was being rapidly extracted in that manner, he became all at once hemiplegic. Similar cases have been noticed by other persons. Therefore we are not to bleed without measure or discretion.

The pulse may be small, and the arterial action feeble, while yet the veins are turgid, and the capillaries of the head and face loaded with blood. Changes may have occurred in the heart, such as to obstruct the stream which it is its healthy office to transmit. These are cases to which the local abstraction of blood from the head by leeches and cupping-glasses is peculiarly adapted.

Again, the whole state of the patient may approximate more or less nearly to the state of syncope; the pulse being weak, the aspect pinched and bloodless, and the skin cool. In this condition, no good, but the contrary, is to be expected from blood-letting of any kind. You will do better to apply warmth, cautiously, to the surface, and cautiously to administer what are called diffusible stimuli, of which the preparations of ammonia afford the most eligible forms. Five grains of the sesquicarbonate, or half a drachm of sal volatile, mixed with camphor julep, are ordinary doses. Stand by till the first stunning effect of the internal shock passes off; and carefully watch meanwhile for symptoms of reaction.

When hemiplegia happens without loss of consciousness or coma, it is most probably the result of white softening and disruption of the fibres of the brain, with or without a *small* effusion of blood. In such cases I quite agree with Dr. Todd, that abstraction of blood from the arm is requisite and justifiable only when there is also early rigidity of the palsied muscles, betokening irritation, and threatening therefore inflammation of the cerebral substance.

In more ambiguous cases, when you scarcely can tell which way the balance inclines, I would advise you to wait the effect of the next remedies I have to mention; viz. purgatives, about giving which you need not entertain the same doubt and hesitation.

Purgative medicines are of signal service in apoplexy. They empty the intestines, which are oftentimes loaded, and which by distending the abdomen have occasioned, perhaps, undue pressure against the diaphragm, embarrassed the breathing, and through it the cerebral circulation. Another very important purpose of hard purging, which I have frequently pointed out before, is the producing of copious watery discharges from the bowels; whereby the blood-vessels are drained, and the tendency of blood to the head especially relieved. If the patient can still swallow, you may give him half a scruple of calomel, and follow it up by a black dose. If the power of deglutition be lost, the croton oil becomes a most valuable remedy. Dr. Abercrombie suggests that it may be conveniently introduced into the stomach, suspended in thick gruel or mucilage, by means of an elastic gum tube. But really this is not necessary. If two or three drops of the oil be put upon the tongue, as far back as is possible, it will produce its specific effect very readily and well. But we are not to wait for the operation of aperients given by the mouth. Strong purgative and stimulating enemata must be thrown into the rectum: half an ounce, or six drachms, of turpentine, suspended, by the help of the yolk of an egg, in gruel or warm water. We very often witness decided signs of amendment upon the free operation of a purgative. I may mention one instance of this while it is fresh in my recollection. I was asked a few evenings ago by a medical friend, to see an old general, a patient of his. I found him in bed, comatose, though capable of being roused when loudly spoken to; but he presently fell off again into stupor. His respiration was peculiar. For a minute or two he would breathe, snoring strongly; then the breathing would cease altogether for half a minute or thereabouts; and then the stertorous respiration recommenced: and so on alternately.

He had been found by his servant on the floor, nearly insensible, in the morning, having fallen either out of, or upon rising from, his bed. He had very properly been cupped; and calomel and aperient medicine had been given: but the coma had been growing more profound all the afternoon. His bowels had been but scantily moved; and the fæces and urine were passed as he lay. His extremities were coldish. The pulse was neither full nor strong.

I learned that for four or five years he had had some very significant warnings; and within that period had suffered one or two slight apoplectic seizures, which had left him with impaired mind and memory.

I recommended blisters behind the ears, and two drops of croton oil with two drachms of castor oil, in a draught. The next morning I expected to hear that he was dead; but I found him quite conscious, speaking somewhat inarticulately, with the right side of his face chopfallen and inexpressive. There seemed no particular weakness of the corresponding extremities. The oils had been followed by copious evacuations from the bowels. The day afterwards he was sitting up, and so well, that I took my leave.

In combination with blood-letting and purgatives, cold lotions to the head are often found useful in this disease, especially if its surface be hot. I need not trouble you by rehearsing the modes in which the application of this remedy may be managed. Blisters near or upon the head, are also frequently of service, after due abstraction of blood, in rousing the patient from his state of coma.

Formerly, at the suggestion, I fancy, of Dr. Fothergill, it was much the fashion to give an *emetic* in the outset of the treatment of apoplexy. But this also is a ticklish remedy, capable of doing good or harm according as it is well or ill timed. If there be already extravasation of blood, or even plethora capitis, the act of vomiting will be likely to increase the existing mischief, and to enhance the danger. On the other hand, it may rouse and rally the nervous power when the patient is pale, and cold, and faint. Yet this can never be regarded as a legitimate purpose of emetics in apoplexy. They can safely be recommended in those cases only, in which the coma may appear to depend, wholly or in part, upon a loaded stomach. Hence the propriety of giving an emetic will deserve consideration whenever an attack of apoplexy follows close upon a heavy meal.

When the immediate danger has passed by, and paralysis remains, or when hemiplegia alone has befallen the patient, we are not to be over busy. If the palsy is to get gradually well, it must be by virtue of time, and the resources of nature. To young and strong persons I should, under such circumstances, give small and repeated doses of mercury: and in all cases I should prescribe aperient medicines, so as to keep the bowels freely open once or twice a day; enjoin perfect quiet; and put the patient upon very short commons. Diuretics are also proper when the urine is not plentiful without them.

You will often have to contend against the ignorance and impatience of the sick, or of their friends, on these occasions. They think that *weakness* is to be remedied by *strengthening* food; by meat and drink, and tonic medicines; or if they are not so foolish as this, they will want to be electrified, or to be put into a warm bath. Certainly in the earlier states of the palsy that remains after apoplexy, none of those measures ought to be permitted. Attempts to urge the hurt brain into action by such means, would be both vain and unsafe. But a secondary evil is apt to ensue, which may in some degree be obviated. During the period in which the moving power is dormant, the machinery of motion may fall from disuse into decay. Muscles that remain long unexercised, wither; and wither for that reason. They undergo the one, or the other, or both, of the two species of atrophy so well described by Mr. Paget. Either they simply dwindle in size, or dwindling they degenerate also in texture. This last is the more common change. The muscle is then spoiled for its purpose; and no longer capable of resuming its contractions, upon the restoration of the nervous influence. It is probable, as Mr. Paget ingeniously suggests, that in some, at least, of the cases in which the paralysis abides after every other indication of disease in the nervous centres has passed away, the residual fault is really in the instruments of motion, the muscles. He adverts to the experiments of Dr. John Reid, which show that the loss of contractile power in a palsied muscle is owing, directly, to its imperfect nutrition, and only indirectly to the severance of its connexion with the nervous centres. Dr. Reid divided the nerves of a frog's hind legs—and leaving one limb inactive gave the muscles of the other frequent exercise by galvanizing the lower end of its nerve. The result was, that at the end of two months the exercised muscles retained their weight and texture and their capacity of contraction; while the inactive ones had lost half their bulk, were degenerated in texture, and had also lost some of their power of contracting. In other cases also he found, that degeneration of texture

in the unused muscles always preceded the loss of their contractile power. It will be proper, therefore, in cases of protracted paralysis, to promote, and if possible maintain, the nutrition of the idle muscles, by friction and pressure, by shampooing, by calling them repeatedly into artificial exercise through the stimulus of galvanism or of electricity. Our aim must be to preserve the muscular part of the locomotive apparatus in a state of health and readiness, until, peradventure—that portion of the brain from which volition proceeds having recovered its functions, or the road by which its messages travel having been repaired—the influence of the will shall again reach and reanimate the palsied limbs. If, however, no such repair or recovery should ensue, then, at length (as Mr. Paget tells us, on the authority of Dr. Turek), “those tracts or columns of the cerebro-spinal axis through which in health impressions were habitually conveyed from the diseased part” will themselves slowly and gradually undergo a softening, as by atrophy. In the more chronic cases we may sometimes benefit our patient’s general condition by the cautious exhibition of some of the preparations of iron.

LECTURE XXXI.

Spinal Hæmorrhage. Paraplegia. Facial Palsy and Facial Anæsthesia; their Symptoms, Prognosis, and Treatment. Other Forms of Local Paralysis, and Local Anæsthesia.

I HAVE done with apoplexy as it respects the brain; which is the same thing as to say that I have done with apoplexy. You will find the same thing applied, indeed, to effusions of blood in other organs of the body; but this use of the word is a perversion of language. Apoplexy, as I have frequently observed before, is the abolition of the functions proper to the *brain*; of sensation, voluntary motion, and thought. In short, it is coma, coming on under certain circumstances.

I shall not speak therefore of *spinal apoplexy* (though that would be less improper than *pulmonary* apoplexy, or *hepatic* apoplexy), but of *spinal hæmorrhage*. Of this I really have little to say, except that it is well known occasionally to occur; and that the symptoms to which it gives rise are by no means peculiar or distinctive. They consist of *pain* in some part of the spine; *convulsions*; *palsy*: that is, they are the very same symptoms which inflammation, softening, mechanical injury, and other disorders of the same part may produce. Spinal hæmorrhage is much more rare than cerebral hæmorrhage. Dr. Abercrombie had met with only one case of it. He gives the heads of seven others which have been recorded by different authors. Dr. Bright has never seen it: but he publishes the particulars of one case, which were communicated to him by Dr. Stroud.

I will read you one or two short examples of spinal hæmorrhage, as specimens. A girl, fourteen years old, was attacked with headache, *pain in the back*, and a tendency to sickness when in the erect posture. At the end of a week the pain in the back became suddenly and very greatly aggravated; and this was followed by general convulsions, which proved fatal in five or six hours. The spinal canal was found filled with extravasated blood, in the lumbar region, where she had felt the pain. The brain and all the other viscera were sound. The case is detailed by Mr. Chevalier in the third volume of the *Medico-Chirurgical Transactions*.

Take one more instance from Ollivier, whose work on the spinal marrow you may read hereafter, when you have leisure, with advantage.

A gentleman, aged sixty-one, had just arrived in Paris after a long journey, when he was seized with *pain in the back*, all the way down from the cervical vertebræ to the sacrum. In the course of a few hours he became paraplegic, and was unable to retain his urine or feces. He then sent for a physician, and died while talking to him. A very extensive extravasation of blood was found in the spinal canal, beneath the membranes of the cord. The lower part of the canal was filled with a bloody

mass, in which the substance of the cord could not be distinguished. Above the third dorsal vertebra the cord was entire, but of a deep-red colour, and very soft.

The *suddenness* of the symptoms may lead you to suspect the true nature of these cases; but I cannot pretend to point out any other feature by which they may be distinguished from other morbid conditions of the spine, already spoken of. I show you one preparation; of which, however, I do not know the history.

I have nothing to add, to what I have already said, respecting that species of palsy which is called *hemiplegia*: and I have only a very few further observations to make in regard to *paraplegia*.

The cause of this kind of palsy is sometimes obvious; sometimes most obscure. If we find, in the spinal canal, blood effused, softening of the substance of the cord, traces of inflammation of its investing membranes, tumours occupying it or pressing upon it, pressure from disease or displacement of the bones, we have a sufficient explanation of the paralysis of those parts of the body, the nerves of which come from the spinal marrow at or below the place of the disease. There are three preparations on the table, of tumours that pressed upon the cord; scrofulous tumours I believe they are: each of the three persons from whose bodies they were respectively taken was more or less completely paraplegic.

But in very many cases we detect no alteration that seems adequate to explain the paraplegia. The palsy creeps on slowly and insidiously, without any particular pain, or violent symptoms: there is no tenderness or bending of the vertebræ. The weakness commences mostly in the legs, which appear to the patient heavier than usual, and of which the healthy sensations are often perverted. The toes tingle, or are numb: he experiences a feeling in them as if a number of ants were crawling on the skin. This is so common a circumstance as to have given a name to the symptom, *formication*. The patient straddles as he walks. His legs are lifted awkwardly, the toes being often the last part to quit the ground: they are then flung obliquely forwards and outwards, and the feet flap down heavily and uncertainly at every step. By degrees the weakness of the lower limbs increases: the palsy creeps upwards, affects the bladder and rectum, and the muscles of the abdomen, at length invades the arms, and ultimately the patient dies: yet very faint traces of disease, or no traces at all, may be visible, by the naked eye at least, upon inspecting the brain and spinal cord. The commonest morbid condition is *softening* of some portion of the cord; and this is also the condition which is the most liable to be overlooked. Dr. Gull describes a case of paraplegia, and it is probably a sample of many more, in which it was only by the exercise of great patience and after repeated examination, that even the microscope discovered traces of inflammatory exudation in the cord; but these, though slight in amount, were distinct and decisive.

It is in these cases of paraplegia that you may expect to witness the very remarkable phenomena which I mentioned before as evincing the separate existence of a "true spinal marrow," distinct from the brain and its prolongations into the spinal canal, endowed with special and peculiar properties, and performing functions that are independent of sensation, of consciousness, and of the will. If you pinch or tickle the surface of the paralytic members, or apply a hot spoon to the sole of one foot, the limbs will, in many cases, start up and move strongly, not only without any voluntary effort on the part of the patient, but in spite of him; or even (in those instances in which there is *anæsthesia* as well as palsy) without his knowing it. The legs often spring up *of their own accord* as it seems; but, no doubt, the apparently spontaneous movement is frequently an excited movement, and takes place in obedience to the laws that govern the automatic motions of the body. Some impression, made first upon the peripheral extremities of afferent nerves, runs through the nervous arc of communication, and exhibits its ultimate effect at the extremities of the corresponding efferent motor nerves. We can imagine many such accidental and unsuspected sources of excitement; a casual touch, the varying contact of the bed-clothes, the bite of a flea for aught I know to the contrary, may suffice. Even the passage of fæces or of flatus along the lower bowel, or of urine through the urinary passages, may be enough (as we are taught by unquestionable facts) to produce these movements. They are more readily excited, *cæteris paribus*, in proportion as the interfering influence of the will is more completely cut off.

I knew a gentleman, who had retired from the medical profession, and who, though not paraplegic, laboured, I believe, under some morbid condition of the spine. He had been, in early life, a hard drinker, and had suffered delirium tremens. Every night, sometimes more than once or twice, the trunk of his body, and all his limbs, became for a while fixed and stiff, from rigidity of the muscles. A few days before his death, he told me this curious fact. Whenever he scraped his shoes on the scraper at the door, his leg flew up, with a spasmodic suddenness, from the iron, notwithstanding his endeavour to prevent it. He died suddenly. I believe he was found dead in his bed.

In some cases of paraplegia involuntary retractions of the palsied limbs can be excited; in some cases they cannot. When the influence of the cerebrum is quite excluded by the operation of disease affecting the spinal cord itself, then is the susceptibility of excited movements the most lively. But the increased susceptibility, which has this inverse relation to the voluntary power, is limited to that portion of the body, the nervous arcs belonging to which lie beyond the seat of the disease; more distant, I mean, from the brain. Hence it follows that we may determine, approximately, the place of the disease, by the test of these reflex actions. The mischief may be situated, or may extend, so low down, that there are no uninterrupted nervous arcs below it. Supposing it to lie as low as, or to reach, the commencement of the cauda equina, we should have no involuntary movements. Conversely, when no involuntary movements can be excited, the spinal disease is, at least, as low as the upper lumbar vertebræ. Thus, I say, we have another mode, in addition to those pointed out in a former lecture, of determining, in a given case of spinal palsy, whereabouts, or to what extent, the cord is implicated in the disease.

We do not so often observe these reflex movements in cases of hemiplegia; apparently for this reason, that in hemiplegia the sensorial influence is not, usually, so completely shut out as it is apt to be in paraplegia. Yet I have seen some of these phenomena in several hemiplegic patients. One of them, for example, whose right hand and arm were quite passive under the strongest efforts of his will to stir them, took notice himself, as did his nurse, that whenever he yawned and stretched himself, the fingers of the palsied hand participated in the action, and were thoroughly extended: and I could, by tickling the sole of his foot, excite some starting of the leg long before any power of voluntary movement returned.

Emotion has sometimes the same singular effect upon limbs and muscles over which volition has no dominion whatever. An artist with whom I am acquainted, and whose arm was almost completely powerless after a recent attack of paralysis, so that no exertion of his will sufficed to raise it from his side — was one day startled, as he was hobbling across a road, by the unexpected approach of a carriage. He noticed, with wonder, that during his attempts to get out of the way, the palsied arm was suddenly jerked up above his head. But he could not again lift it there after the fright was over.

With the loss of power there is usually more or less of *anæsthesia*: the limbs are numb; or feel as if they were swathed in bandages. Sometimes they are totally devoid of sensibility, so that the patient, lying in bed, does not know, till he lifts up the bed-clothes, whereabouts or in what position his legs are lying.

Do not forget the important fact that, in many, nay in most cases of paraplegia, the urine at length becomes ropy, stinking, ammoniacal; and that the bladder, after death, presents appearances such as chronic inflammation might produce; roughness and redness of its inner surface, and thickening of its coats. What is the order of these changes, and in what relation do they stand to each other? Is the quality of the urine first altered, and does the bladder then suffer from the perpetual contact of this unnatural secretion? or does the bladder become diseased in consequence of the palsy, and pour forth unhealthy mucus, whereby the quality of the urine is affected? The truth I believe to be implied in the latter of these questions. In support of that view I have heard the following facts affirmed. The urine voided being alkaliescent, the bladder was washed out by the injection and withdrawal of warm water. Then the next portions of urine that descended from the kidneys were immediately removed, and tested, and found to be acid. So also, after death, the urine has proved to be alkaline and mucous in the bladder, acid in the pelvis of the kidney. Dr. Bence Jones, in the *Philosophical Transactions* for 1845, has some excellent obser-

variations on this subject. He finds that, in such cases as I have been speaking of, the alkalinescence of the urine is always due to the presence of carbonate of ammonia. It arises from the decomposition of urea by altered mucus. The urine makes reddened litmus paper blue, but the red colour returns as the paper dries. The blue would be permanent if produced by a fixed alkali in the urine. Moreover, pus globules are always to be detected by the microscope in the secretion, before it becomes ammoniacal and ropy. While the urine continues acid, any pus which may be mixed with it retains its natural appearance, and fluid condition. Its globules remain distinct, and do not adhere to each other. But whenever the urine becomes alkalinescent, the carbonate of ammonia, acting upon the pus globules as the liquor potassæ might do, causes them to stick together: so that a stringy viscid matter is formed which includes epithelium, ammoniaco-magnesian phosphate, and granules of phosphate of lime. "All these together constitute the ropy mucus seen in cases of diseased bladder." There seems to be some connexion between an inflamed condition of the mucous membrane of the bladder, and the state of the spinal cord. Dr. Bence Jones calls attention to the fact, that sloughing of the external integuments is common in the palsied parts; and suggests that the internal integument of the bladder suffers some analogous change, whereby the urine is at length rendered ammoniacal. It is said, however, that in some instances of paraplegia, the urine has been *secreted* alkalinescent. If these cases have been accurately noted, disease may perhaps have been propagated from the bladder, backwards. Or the disorganization of the bladder, and the alkaline quality of the urine, may both have been common results of the interruption of the nervous influence.

We have reason to believe that the defect in some of these cases of paraplegia is merely functional: independent, I mean, of any such change in the nervous matter as is cognizable by our senses. It may be brought on by various causes: by cold; by intemperance in drinking; by excessive sexual intercourse; or, still more surely, by self-abuse. I have had the last cause assigned to me voluntarily by patients themselves. In such cases we may presume that the loss of function is confined to the spinal marrow. But there is another way in which paraplegia may be accounted for, although its physical cause is very liable to be overlooked. It may result from serous effusion into the spinal canal; which effusion may have originated *there*, or what seems sometimes to be more probable, may have been poured out *within the cranium*, and descended by the force of gravity to the lower part of the cavity of the spine. Dr. Baillie read a paper on this subject before the College of Physicians: it is contained in the sixth volume of the *Medical Transactions*. He was not the first person to whom this mode of explaining certain obscure cases of paraplegia suggested itself; but he was the first I believe who published upon it. This effusion may very readily be overlooked. Commonly the brain is examined first; and no great attention is paid to the escape of fluid from the vertebral canal. It would be better to lay open the spinal cavity first, at its lowest part, and to puncture the theca, and then to observe what quantity of fluid runs out when the body is placed upright. There *should* be a *certain* quantity; but if *much* serum so escaped, we might conclude that it had existed in hurtful abundance during life, and had caused the paraplegia. In most of these obscure cases you may trace *some* head symptoms; giddiness, transient confusion of thought, loss of memory; and it really will be worth your while to make the examination in the way I have pointed out, whenever you have occasion to inspect the body of a patient who has died paraplegic.

Paraplegia has been ascribed to some primary morbid condition of the *nerves* which belong to the spinal cord. That the functions of the *efferent*, or motor, nerves may be impaired, and even arrested, by exposure to cold, and by other injurious influences, is both possible and probable. But a diseased or disordered state of the *afferent* nerves has been assigned as a cause of the palsy. This is less clearly conceivable. Coexisting disease of the kidneys, and coexisting enteritis, have been thought sufficient to produce and keep up a paralytic condition of the lower limbs. The extremities of certain incident nerves being affected, a morbid impression is transmitted to the cord, suspensive of its central function. The efforts even of volition, which come from the brain, are no longer successful. Such is the theory. I do not say it is an erroneous theory; but I am bound to tell you that I think it unproven. My own experience has furnished me with no facts which go to support it.

I have met with three or four instances of paraplegia, in which the palsy appeared to result from the immersion of the lower part of the body, for some time, in cold water. Thus, in one of them, the patient had been in the habit of wading for hours together, in a river, while fly-fishing. We may reasonably suppose that, under these circumstances, the motor nerves, rather than the spinal cord, would be likely to suffer. A remarkable example of the effect of cold so applied, in benumbing the *sensations* of the parts exposed to it, fell under my notice in the spring of 1846. A lady, between 20 and 30 years of age, suffering from slight leucorrhœa, was directed by her physician to use the cold hip bath. Mistaking, I believe, his instructions, she sat in the cold water for twenty-five minutes, on twelve successive mornings, in the month of February. On each occasion she came out of the bath benumbed. At first the numbness was transient; but at length it became permanent. When I saw her the sensibility was nearly extinct, from that level of her body which the cold water reached, downwards. The parts were not quite destitute of feeling, but seemed to her as if muffled. She scarcely knew when her legs touched each other — nor whereabouts they were when she was lying in bed. She walked in an awkward manner, and said that her legs felt large and heavy; and if one of her shoes slipped off, she was not conscious of it. The inclination to make water came suddenly, and with hurry, and the urine sometimes escaped from her unawares, and she had no sensation that it was passing. Her bowels were never relieved without the aid of purgatives, and then with similar haste. The pulse was plainly to be felt in the tibial artery. There were no head symptoms.

I have related this case chiefly for the sake of mentioning the remedy to which it ultimately yielded. When warm baths, friction, blisters, and stimulants of various kinds had been tried in vain, Mr. Christophers, who had called me to see the patient with him, had recourse to electro-magnetism. After the second application, improvement became manifest; and in about three weeks the sensibility was completely restored, and the lady well.

Cases of paraplegia, such as I have been describing, are by no means uncommon. They are usually slow and tedious; and you will be called upon to administer to their relief. I need not repeat the caution which I have several times given, in respect to the condition of the bladder; you must take care that it does not become over-distended with urine; and you must enjoin strict attention on the part of the nurse to keeping the patient clean and dry. Friction along the course of the spine; blisters to the loins or sacrum, frequently repeated; issues; and electricity: all these means you will generally have opportunity enough for trying, and for regretting their intility. In such cases it may sometimes be warrantable and proper to employ strychnia: a poison which mainly affects the spinal cord; causing, when given in a sufficient dose, tetanic spasms of the limbs, with very little or no affection of the sensorium. I have heard of some striking instances of recovery from paraplegia under the exhibition of this drug. I wish I could tell you that I had ever *seen* such. Let me caution you against its indiscriminate use; or rather its abuse. No good can reasonably be expected from it, but much harm, unless the cord be free from organic disease. Even then I would not advise you to begin with a stronger dose of strychnia, or of the sulphate or the acetate of strychnia, than the twelfth part of a grain every six hours: this may be gradually and cautiously increased, until it gives rise to twitching of the limbs, or to some other obvious effect. The twitching is usually confined to the palsied limbs. This shows that it results from the agency of the remedy upon the excitomotory system, or true spinal marrow; of which the reflex function is always more readily excited when the cerebrum has lost its customary controlling power. When this symptom occurs, you had better go on with the same dose; it would be unsafe to increase it: and the progress of the case will soon inform you whether any benefit is likely to accrue from a continuance of the medicine. A paraplegic out-patient now attending the hospital has taken the strychnia. It made his palsied limbs start and extend themselves; but no permanent power has been gained.

There is one other drug which I should recommend you to try in such cases; viz., the tincture of cantharides. It certainly has sometimes a very beneficial effect. Generally, when it does good, it acts as a diuretic; and Dr. Seymour has thrown out the suggestion that it is most likely to be useful in those cases of serous effusion into the spinal cavity, of *spinal dropsy*, which I just now described. He recommends

the tincture as a good diuretic in several forms of dropsy; and supposes that it benefits paraplegia by tending to produce absorption of the serum effused within the vertebral canal.

Moreover, there is another principle upon which this medicine may be sometimes advisable. Cantharides are well known to have a peculiar effect upon the bladder; which effect is doubtless produced through the corresponding part of the spinal cord. If, by means of the Spanish fly, we can excite, though but from time to time, the function of that part, we may obviate, in a great degree, the distressing consequences of incontinence of urine, arising from paralysis of the *sphincter vesicæ*. Dr. Marshall Hall relates a very interesting fact, bearing directly upon this point. A young lady had a tumour within the tenth and eleventh dorsal vertebræ. It gradually, but completely, severed the spinal marrow, and induced perfect paraplegia. The bladder lost its power of retention. But on giving a dose of the tincture of cantharides the power of retaining the urine was always restored *for the time*. That power would cease, and again be restored, on suspending and repeating the medicine.

Dr. Hall remarks that the cantharides obviously acted through the segment of the excito-motory system left below the division of the spinal marrow.

The tincture may be given in half-drachm doses.

The forms of paralysis that have hitherto been noticed are forms of *partial* paralysis. When the palsy is still more limited, although the epithet partial would be equally applicable, the term *local* palsy is more commonly used. There is one of these local palsies which is exceedingly interesting, and of much importance: I mean palsy as it affects exclusively one side of the visage; *facial* palsy. It is sometimes called, not very correctly, paralysis of the *portio dura* of the seventh nerve. The most common kind of facial palsy is indeed paralysis of the muscles supplied by that nerve. But the word paralysis is misused when it is intended to express any other loss of function than that of the faculty of motion in muscular parts. It is incorrect to speak, as some authors do, of palsy of the kidney; it is equally inexact to speak of palsy of a nerve.

I say that *facial palsy*, and *facial anæsthesia* (for the two should be considered together), are very *interesting* affections, because they elucidate, in the human subject, some of the most curious discoveries of modern physiology: and they are *important* affections for you to study and understand, inasmuch as, though always distressful and alarming to the patient and his friends, and sometimes indeed indicative of extreme danger, they often are merely inconvenient and disfiguring, and bespeak no peril at all.

Let us first consider that affection in which the majority of the muscles on one side of the face alone are palsied. I have already briefly touched upon this form of palsy when it constitutes a part of hemiplegia. But it is of more consequence to attend to it when it occurs without any similiar affection of the limbs. If the arm, or leg, or both, are paralysed at the same time with one side of the face, we know that the whole results from disease in the brain, or in the upper end of the spinal cord. But it is not necessarily so when the face alone is palsied; and it is often of great moment to the comfort and the safety of the patient, that we should be able to tell whether the palsy does imply disease within the skull, or not.

The appearance presented by patients affected with facial palsy is peculiar, and very striking. From one half of the countenance all power of expression is gone; the features are blank, still, and unmeaning; the eyelids apart, and motionless. The other half retains its natural cast, except that, in some cases, the angle of the mouth on that side seems drawn a little awry. This is apt to be mistaken for proof of a spasmodic condition of that part; but it is owing simply, as I stated before, to the want of the usual balance or counterpoise from the corresponding muscular fibres of the palsied side. The patient cannot laugh, or weep, or frown, or express any feeling or emotion with one side of his face, while the features of the other may be in full play. One half of the aspect, with its unwinking eye, its fixed and solemn stare, might be that of a dead person; the other half is alive and merry. The incongruity would be ludicrously droll, were it not so pitiable also, and distressing. To the vulgar, who do not comprehend the possible extent of the misfortune, the whimsical appearance of such a patient is always a matter of mirth and laughter. On the

other hand, his friends and relations imagine that he has had a stroke, and are in great alarm for his life. In the majority of these cases there is not, however, any real danger of that kind to be apprehended; a circumstance which, of itself, would render the exact diagnosis of the complaint peculiarly interesting: and the exact diagnosis you may at once determine by noticing the condition of the eye. Dr. Todd has well remarked that inability to close the eyelids—paralysis of the orbicularis palpebrarum—is the pathognomonic sign of facial palsy from suspended function of the portio dura: and that this nerve is seldom affected in cases of hemiplegia depending on disease of the brain.

In general there is no deficiency of sensation. And, *vice versâ*, we sometimes have loss of sensibility in the same parts, without any diminution of the power of motion. The best way, I believe, to place the phenomena of these curious affections plainly before you, will be by examples.

A house-maid, Jane Smith by name, twenty-eight years old, became one of my out-patients at the Middlesex Hospital, with the following symptoms. She had lost all power of moving the right side of her face. When she endeavoured to raise her eyebrows, the right side of the forehead remained smooth, and the left was wrinkled. When she attempted to close her eyes, the right eye was but partially covered, the eye-ball rolling upwards, and carrying the cornea within the curtain of the upper lid, which descended a little to meet it. When she tried to snuff in air through the nose, not being able to keep the right nostril stiff and open, its sides came together, and no air passed up on that side. When she smiled, the right side of the face remained perfectly still, like a mask; and it wore at all times a vacant and inanimate character. When she was told to perform the action of blowing, her right cheek was puffed out like a loose bag, and the breath issued, whether she would or no, at the right angle of her mouth. The same thing happened with her food and drink; she could not prevent their escaping at the right corner of her mouth; nor could she convey morsels of food from the right to the left jaw, without the aid of her hand applied externally in support of the paralysed cheek. The masseter and temporal muscles, however, acted as strongly on the one side as on the other; she could chew perfectly well on the palsied side, and the sensation of the palsied parts remained perfect; and there was no paralysis of any other part of the body.

All these phenomena are invariably met with in all complete cases of this kind. I will contrast them with the phenomena presented by another of my patients, who was in the hospital, and whose name was Ann Church. I give their names, that I may the more readily distinguish the one from the other. When this woman, Church, applied for admission, she complained of intense pain, with some swelling, in the right temple, and extending thence generally over the right side of the face and head. It was soon discovered, however, that although she complained of most severe pain in these parts, they had entirely lost their ordinary sensibility to external impressions. She felt nothing when her forehead, or cheek, or nose, or chin, was touched on that side. In short, there was complete *anæsthesia* of the right half of the face; just as in Smith's case there was complete *palsy*. The insensibility was very exactly limited to the right half, and terminated abruptly at the middle line. It was remarkably evident in a part in respect to which the bystanders could scarcely be deceived, even if there had been any reason (which there was not) for distrusting the patient's own statement. The surface of the eyeball is proverbially sensitive, even to slight impressions. But you might place your finger upon this woman's right eye, or you might brush it with a feather, without giving her the smallest pain, or producing any sensation at all: whereas, on the left side, the lightest touch caused involuntary shrinking, and closure of the eyelids, and a gush of tears. She declared also that she had no feeling in the right half of her mouth; she neither tasted sapid substances, nor was she at all conscious, from any sensation produced by them, that they were placed there. Her lips on the same side were equally destitute of sensibility; so that when she drank, having no perception of the contact of the cup with her lips beyond their middle point, she felt as if she were drinking from a vessel with a broken rim. This is a circumstance which all persons who are thus affected are much struck with: and it almost always forms a part of their voluntary account of themselves.

Besides this defect of sensibility, the power of contracting the masseter and temporal muscles on the right side was entirely abolished in this patient. You may deceive

yourselves on this point, if you do not investigate it carefully, and with certain precautions. At least I have known persons doubt, because, having directed the patient to open and shut his mouth, they have confounded the movement of the whole jaw with the action of the masseter muscle. But if you tell the patient first to close his mouth, and then to perform the action of grinding with his teeth, placing your fingers at the same time on the corresponding muscles on each side, the difference, when it exists, will be very striking. In the woman of whom I speak, no swelling of the masseter or temporal muscle on the affected side took place when she forcibly closed her jaws. There was no other paralysis.

Now we cannot separate the physiology from the pathology of such affections as these. Nor ought we. The morbid conditions of which the two cases just described furnish samples, illustrate in a very beautiful manner the modern doctrine respecting the special uses of particular nerves. In the first of the two cases the palsy resulted from suppression of the function of the hard portion of the seventh pair of cerebral nerves; and the anæsthesia, in the last of the cases, depended upon suspension of the function of the fifth pair. You know that experiments performed upon living animals have proved that the division, by the scalpel, of the portio dura, before it spreads out into that remarkable nervous network on the side of the face, paralyses all the muscles, the combined play of which gives variety and significant expression to the countenance; and that, on the other hand, the division of the fifth nerve deprives the same parts of their sensibility. In these two cases, and in such as these, for they are by no means infrequent, a similar set of experiments upon the same nerves, in the *human* living body, is performed before our eyes by the agency of disease, or accident: and the result justifies most completely those conclusions which had been deduced in the first instance, from contrived observations made upon the lower animals.

There is one point in the history of these cases upon which I must dwell a moment longer; for it is a most interesting point. That the condition of the temporal and masseter muscles should be reversed in two patients so oppositely situated, was no more than might have been expected. But in each these muscles were affected in a manner the very contrary of that which the general circumstances of the case would, *à priori*, have prepared us to anticipate. Where the superficial muscles were paralysed, and the principal movements of the face suspended, there the masseter and temporal muscles were in full power and action; and where the loss of sensation was the predominant phenomenon, and the ordinary motion and expression of the countenance remained, there these muscles were in a state of complete palsy.

A few years only ago, this difference and apparent inconsistency would have been quite inexplicable. The progress of modern science has removed the difficulty, by establishing a general agreement between the *functions* of different nerves, and certain observed peculiarities in their *anatomical relations and arrangements*.

Suffer me to *remind* you (for I know that these interesting points of physiology must have already been taught you) that the nerves which proceed from the spinal column on each side are connected with it by two fasciculi of nervous fibrils — two *roots*, as they are metaphorically called — of unequal size; that when the larger of these, which is situated posteriorly, and is furnished with a ganglion, is divided in a living animal, the parts to which the nerve is distributed lose the faculty of sensation, while the power of voluntary motion remains unimpaired; and that when the smaller and anterior, which has no ganglion, is alone cut, the same parts are instantly palsied, but retain their sensibility. In other words, the posterior fasciculi minister to the faculty of sensation, the anterior to that of motion.

Now the fifth pair of nerves was observed to have a similar origin; to be composed, that is to say, of two fasciculi or roots, one larger than the other, and invested with a ganglion; the other smaller, and having no ganglion. It was natural to infer that the functions of these roots would be analogous to those of the corresponding portions of the spinal nerves; that the ganglionic fasciculus would relate to sensation, and the other to motion. And such is found to be the case; and the arrangement here is really very curious. The smaller portion of the fifth nerve is exclusively expended upon a very few muscles; viz., the masseter, the temporal, the two pterygoid muscles, the circumflexus palati, and the tensor tympani. The action of the two first of these, of the masseter and temporal muscles, is obvious to common observation; and

therefore their condition is noticed in such cases as I have related. Again, these very same muscles have been shown, by careful dissection, to receive no nervous branches from the seventh nerve, which is a motor nerve, and which ramifies so abundantly upon the superficial muscles of the face.

It was to be expected, therefore, that any diseased state confined to the *portio dura* of the seventh nerve, would leave the temporal and masseter muscles fully effective: and that disease involving the fifth nerve, but leaving the seventh untouched, would destroy, not only the general sensibility of the face on that side, but also the power of contracting these particular muscles. And this was thoroughly exemplified in the two cases that I have detailed. The girl Smith had total palsy of the superficial muscles; but sensation, and the action of the deeper-seated muscles, continued perfect: while in the woman Church there was default of sensibility, and paralysis of the temporal and masseter muscles; but the movements of the superficial muscles were unimpeded.

Total interruption of the function of the *portio dura* will paralyse these superficial muscles of the face: and such interruption may be occasioned either by *sudden injury* done to the trunk of the nerve; or by *disease* affecting its proper structure; or by *pressure*, the consequence of disease in parts contiguous to it. And it is of great importance to observe that the morbid condition which causes the interruption may be situated at any point in the course of the trunk of the nerve: while it is yet within the cranium; or during its passage through the petrous portion of the temporal bone; or after it emerges upon the face, through the stylo-mastoid foramen, to be ultimately spread in meshes over the cheek and temple. The nerve may be compressed or hurt while still within the skull; but in *most* cases of this kind other portions also of the nervous matter are involved in the mischief, and other sets of voluntary muscles testify this by their immobility or their irregular action. This is sometimes the case when facial palsy occurs as a part of hemiplegia. In most instances, however, of hemiplegia, there is but slight distortion of the countenance, a mere hanging of the cheek, with no paralysis of the orbicularis muscle of the eye: motor branches of the fifth nerve only being affected. When the *facial muscles alone* are paralysed, it happens in a great majority of instances that the nervous function is interrupted in that part of the *portio dura* which lies incased in the bone, or in the more exposed part which issues in front of the ear: and hence it arises that this particular form of palsy is, in general, unattended with any danger to life.

The physical cause of this remarkable disfigurement, and the true explanation of its prevailing immunity from danger, were first pointed out by Sir Charles Bell: but both the existence of the malady as a distinct form of disease, and its comparative harmlessness of character, had been observed and described some years previously: although the reason neither of the one nor of the other was at that time understood. Dr. Powell had narrated, in the fifth volume of the *Transactions of the College of Physicians*, three marked instances of this form of local palsy; and had noticed at the same time its apparent independence of any apoplectic tendency, or cerebral disease.

The exciting causes of the complaint are various. Sometimes it is the consequence of mechanical violence, by which it is plain that the nerve has been lacerated, or otherwise injured. Sir Charles Bell, to whom we are indebted for much information on the subject, mentions several examples of this kind. In one a man was shot by a pistol-ball, which entered the ear and tore the *portio dura* across at its root. In another, the patient was gored by an ox; the horn of the animal entered beneath the angle of the jaw, and came out in front of the ear, tearing the nerve across. In a third, the nerve was divided by a surgeon's scalpel, in an operation for the removal of a tumour which lay above and around its course. I have myself known the same disaster to result from the unlucky incision of an abscess near the ear. In all these cases the injury was external and obvious. In a fourth the palsy followed a blow on the ear which caused hæmorrhage from that part: here probably the nerve was hurt in its passage through the bone. Some time ago, a man was brought into the Middlesex Hospital who had fallen from a height, upon his head. The muscles of the left side of the face were paralysed. He died in a few days; and examination of the head showed a fracture in the base of the skull, passing through the petrous portion of the temporal bone, and rending the seventh nerve at its entrance into the meatus

auditorius internus. In the year 1832 I had a patient (Richard Hills) in the hospital with the same kind of paralysis, which seemed, in him, to have been occasioned by a mere shock or jar. He was a coachman, and one day, when he was off his box, his horses started away, and he ran to their heads to stop them, but was thrown down in the attempt, striking his hip and elbow. He received no blow on the head at all. Three hours afterwards he found that he could not *spit* properly. The affection is not unfrequently discovered by that circumstance. He could not avoid spitting on his clothes on one side; and he could not whistle. Another circumstance worthy of notice took place in this man, which often, though not always, happens in these cases, and which I did not mention before. He remained for about two months in the hospital; and regained during that time in some degree the power of exercising the affected muscles; but he still was unable to bring the right eyelids together. The eye itself was unharmed. After he was made an out-patient he resumed his functions on the coach-box; and his eye, permanently half-open and unprotected, was more exposed to dust and to currents of wind than it had been when he was an in-patient. Moreover he got drunk; and he soon presented himself again with universal redness and inflammation of the conjunctiva. Sometimes the inflammation in such cases produces opacity of the cornea and a total loss of vision. This is one of the worst consequences of facial palsy. Fortunately it is only an *occasional* consequence: and it will occur or not, according to the quantity of motion which remains to the eyelids, and the degree of exposure to the ordinary causes of inflammation. I have noticed that—either from differences in the completeness of the palsy, or from peculiarities of the features—the speech is more embarrassed in some of these patients than in others. Labial sounds, and words that require the explosive pressure of the buccinator muscle, are uttered imperfectly; and the patient soon learns to assist his defective articulation, by supporting the palsied cheek, and so affording a fulcrum to the lips, with his hand.

Sometimes the palsy depends upon manifest *external disease*; sometimes upon disease which is hidden, and probably *internal*; in the bony canal. Sir C. Bell describes an instance in which it accompanied the disorder called the mumps. Dr. Malden, of Worcester, witnessed another in which a fixed, hard, indolent tumour, had formed between the ramus of the lower jaw and the mastoid process of the temporal bone. As this tumour gradually subsided, the palsy disappeared. In each of Dr. Powell's three cases the affection was apparently caused by exposure of the side of the head for some time to a stream of cold air. A medical acquaintance of mine residing in London, had a patient at Greenwich, whom he visited daily. It was cold weather; and on one occasion, as he was returning in the cabin of a steamboat, he was sensibly incommoded by a keen east wind, which blew through an open window directly upon his ear. The next day he presented himself to me with that side of his face fixed in the manner I have been describing. Exactly the same mishap befel a Scottish physician while travelling to London by a coach: and sent him in great alarm to Sir C. Bell. Some years ago a marked example of facial palsy occurred in one of my hospital patients; it appeared to be owing to his having been constantly in the streets for some days without shoes or stockings, during a cold thaw. It may be presumed that in these instances some swelling was produced in the soft parts around the nerve, compressing it where it lies within the unyielding bone. Exposure to cold in this way is the commonest of all the exciting causes of the complaint, and cases thus arising are more obedient to treatment than most others. Probably some of you saw a female patient who came under my care in the hospital in May last (1838), in whom facial palsy had existed on one side for eighteen years. When about three years old she had the measles; and a scrofulous tumour formed behind the ear, and broke: and after some time, a portion of carious bone came away. Then the wound healed (of which deep traces are still visible); and the peculiarity of her features was observed. There are still other cases in which we fail to discover any direct explanation of the paralysis, either in the history of the patient, or in his physical condition. In the girl Smith, whose symptoms I stated in detail as an example of the appearances uniformly present, the malady came on without any obvious cause, and it resisted all the means employed for its removal.

That the greater number of cases of this kind are free from serious peril, is a fact of great practical importance. It enables us to quiet the alarm of the patient and of

his family: and it regulates in many instances the *treatment*; rendering it less active than it might and perhaps ought to be, if the palsy were really the harbinger of apoplexy. At the same time you should not be ignorant that a similar limitation of paralysis to the particular muscles supplied by the portio dura is sometimes (though rarely) observed, when the disease has a more inward origin; when it affects and involves the brain itself. The following case caused me much anxiety, for the subject of it was a personal friend of mine. — I was summoned to his house in the autumn of 1829, and found him with complete palsy of the left side of the face. It had existed a day or two. I shall not describe the appearances and symptoms that resulted from the paralysis: for they were precisely the same as were presented by the girl Smith; and they are always, and necessarily, very much alike. But though the *palsy* was strictly limited to this set of muscles, there were other symptoms present which indicated that the interruption of the functions of the portio dura was connected with some morbid condition within the cranium: nausea and vomiting, twitching of the muscles of the *other* side of the face, great drowsiness, and a slow pulse, 48 only in the minute. He lurched also, and staggered as he walked; but he distinguished this from the reeling of vertigo, and denied the latter sensation altogether. He was deaf, too, on the affected side.

His previous history did not tend to diminish the fears which his actual state excited.

In the preceding February, he had been attacked, rather suddenly, with intense pain just above the right eyebrow, and became extremely drowsy. Being desirous, on account of these feelings, to excuse himself from a dinner engagement, he found that he was unable to write a proper note: he could not remember how he ought to express himself.

All these symptoms soon passed off; after the operation, I believe, of an emetic. But he had another attack of the same kind in the subsequent May: the same severe pain over the right brow, with great drowsiness and confusion of mind. He could not recollect the first line of the *Æneid*. He wished a friend to look at the *signatures* of some letters that had arrived: and though he knew the root, he could not tell how the word he wished to use was formed: whether it was signition, or signation, or signature. The digestive organs on this occasion were made the object of treatment, and he soon got well.

Another part of his history was instructive; and therefore I mention it. Before these attacks he was in the habit of eating and drinking freely: and his power of digestion was supposed to be enormous. After the attack in May he commenced a strict course of temperance. He drank no wine *till three or four days before* the occurrence of the facial palsy: he had then taken it again; about four glasses daily: and on one of the days he drank two glasses of champagne.

It was of some moment to this gentleman, not only that he should recover, but that he should recover quickly. He had been appointed by Government to a mission in Ceylon, and all his equipment was already on board a vessel which would sail in a fortnight.

Cupping behind the ears, blistering, purgatives, and small doses of calomel continued till the gums were slightly sore, removed the paralysis, and all the other symptoms, in about ten days. He went to Ceylon, and accomplished his mission so ably that after his return the Government appointed him to one of far greater importance in India. He has remained perfectly well; and possesses one of the clearest and strongest intellects that I am acquainted with.

I must trouble you with one more case, to complete the history of this disease: a case in which the cause of the facial palsy was situated within the cranium and proved fatal, and became visible after death.

Samuel Dovey, a tailor, fifty-seven years old, was admitted under my care into the hospital, in February, 1833, with complete palsy of the muscles supplied by the portio dura on the right side, and of no others. There were symptoms enough, however, to show that some serious mischief was going on within the skull. He suffered intense headache, more on the left than the right side; was dizzy and staggering; and could not get to the ward without being led.

The palsy had come on about ten days before, in the night. He found when he came down stairs the next morning that he could not spit as usual; and his friends

observed the unnatural state of his features. He had had no fit, nor loss of consciousness; but he thought his memory was failing. At the time when the paralysis was first noticed, he had some numbness and tingling of the right arm, extending to the last two fingers. He was quite deaf in the right ear. This is a point deserving attention in such cases. The deafness, when it occurs, marks an affection of *both the portions* of the seventh nerve: and therefore indicates the probability of an *internal* cause.

The whole progress of this case was very interesting; but I must confine myself to those circumstances which bear upon our present topic. He lived about a month after his admission, and during that interval he suffered great pain in the head, was delirious at times, and at other times in a state of coma: at one period he suddenly presented the ordinary symptoms of apoplexy, from which he partly recovered.

I found a cancerous tumour occupying the right hemisphere of the brain; and at its under part was an apoplectic clot, as big as a hazel-nut. I found also a very satisfactory explanation of the deafness and of the facial palsy which had been noticed during his life-time. The portio dura and the portio mollis, where they emerge as distinct cords from the medulla oblongata on the right side, were adherent to each other. The portio dura was both harder and larger than the corresponding nerve on the opposite side, while the portio mollis was wasted and diffuent. The same change was traced up to their entrance into the petrous portion of the temporal bone. Immediately over the medulla oblongata, and in a vertical line above the point of emergence of the seventh pair of nerves, a nipple-like portion of brain projected downwards, and had apparently communicated pressure to these nerves; and this projection from the lower surface of the brain seemed to have been produced by the general pressure resulting from the growth of the tumour.

The remarks which I have been applying to *palsy* of these parts hold true also in respect to their *loss of sensibility*. The anæsthesia may or may not portend danger to life, according as the interruption of nervous function on which it depends is situated more or less near to the origin of the fifth pair of nerves in the brain. The patient, Church, whose case I have several times referred to in this lecture, left the hospital with the sensibility of her face nearly as perfect as ever. The treatment consisted in local blood-letting and counter-irritation. She had erysipelas of the head while in the hospital, and was in some danger from that complaint, which was attended with a good deal of fever and delirium. With the exception of the delirium, which belonged no doubt to the erysipelas, there was no reason to suspect any affection of her brain.

I have incidentally adverted to the plan of treatment to be pursued in these cases of facial palsy. When the complaint is recent, and has an obvious cause, the appropriate remedies will readily suggest themselves. When, for example, it has come on after exposure to a current of cold air, or after a blow, or any circumstance likely to give rise to inflammation, you must treat the case as you would treat inflammation; bearing always in mind that a small amount of disorganization, a little thickening or induration of the parts around the nerve, may render the deformity and the inconvenience *permanent*. If there be inflammatory fever, bleed from the arm: if there be not, take blood from the neighbourhood of the affected nerve by cupping: apply fomentations; or, what is better in these cases, conduct the steam of hot water against and into the ear: and administer mercury so as just to touch the gums. I should always take this latter precaution, lest any effusion of lymph should cause abiding pressure on the nerve. If the palsy give way before the gums become tender, the mercury need not be pressed further. The iodide of potassium is often an eligible remedy in these cases.

Where there is any ground to suspect that the brain is implicated, the treatment just described must be pursued with greater diligence, and with such modifications as the nature of the case may require. If there be evidence of chronic disease in the petrous portion of the temporal bone, such as tenderness of the mastoid process, deafness, a protracted discharge from the ear, and an imperfect state of the membrana tympani, we can scarcely expect much good from very active treatment. We must then have recourse to counter-irritation, and such other measures as I spoke of when the subject of otitis was briefly considered.

The examples which are met with of local palsy, and local anæsthesia, are number-

less; but those which I have mentioned are the most common and the most important. They are always deserving of attention; but more so when any suspicion arises that they may be connected with cerebral disease. Sometimes they evidently have no such connexion. In the month of November, 1834, a coachman became my patient in the hospital with incomplete paralysis affecting some of the muscles of the right leg alone, with numbness of the foot. He could both stand and walk; but on advancing that leg, his foot flapped suddenly down, and he could not deliberately direct and plant it like the other. His general health was quite good: he had no headache, nor giddiness, nor palsy of any other part. But a month ago he had been sitting with the right leg thrown over the opposite knee; and he continued in that position until the foot felt numb, and tingling, and was (what is called) asleep; and it had remained in the same condition from that time. After some general treatment (cupping and purgatives) before he came to the hospital—treatment which was quite proper in the way of precaution, but which was probably, in truth, unnecessary—I had his leg electrified; and in about ten days the sensation and the power of the limb were almost restored. Mr. Swan mentions a somewhat similar case, in which anæsthesia of the hand was produced by strong pressure made upon the wrist.

There are some very curious facts connected with anæsthesia, showing that the voluntary exercise of the muscles is regulated in some measure by the sensations of the limb that is employed. The sense of resistance prompts to such contraction of the muscles as is required to balance that resistance; reminding the will (so to speak) of the necessity that exists for its perpetual and vigilant operation. Continued volition is essential to the continuance of the muscular tension. Thus Dr. Yelloly describes a woman who had no power of feeling in her hand and fingers, although the power of moving them, and of grasping substances, was perfect. This woman found that she could carry glasses or plates in that hand very well and safely, if she continued to look at and attend to them; but if her eyes were turned another way, as she did not feel what she held, she was very apt to drop it. Dr. Ley met with just such another case. A woman had defective sensibility on one side of the body: she could hold her child in the arm of that side so long as her attention was directed to it; but if surrounding objects diverted her from taking notice of the state of her arm, the flexor muscles soon began to slacken, and the child was in danger of falling. All this is exceedingly curious.

Andral has recorded a most singular example of local anæsthesia, which preceded an attack of apoplexy. The patient lost, from time to time, all sensation in certain isolated parts of the skin upon the thorax: there were five or six of these insensible spots, each about the size of a five-franc piece. You might pinch the skin in these places without producing the slightest feeling in the patient. In all other parts the sensibility was perfect and lively.

There are other cases also on record, more remarkable still; in which the patients have lost both the power of motion and the faculty of sensation in almost every part of the body, and yet have survived for a considerable time. Thus one person (whose case is related in the *Bulletin des Sciences Médicales* for January, 1828,) became first amaurotic, then deaf, and then by degrees lost all power of sensation and motion except in the tongue and in the muscles of deglutition and respiration. His speech and intellects were unimpaired. It was accidentally discovered that a small patch on the right cheek retained its sensibility; and by tracing letters on this sensible spot, his wife and children were enabled to interchange ideas with him. He died at length, and his body was not examined.

I shall finish what I have to say on this head, by relating a case of the same kind, which occurred under Dr. Abercrombie's notice; and which we are sure, therefore, would be observed with care, and recorded with fidelity.

A servant girl, about 20 years old, sprained her back in lifting some heavy article of furniture. She felt no great inconvenience at the time; but some little while after, weakness of the legs came on, and gradually increased to complete paraplegia. After an interval, the affection extended to her arms, and she then had not a vestige of motion of any of the parts below the head, except a very slight movement of one of the fingers; but the internal functions were all perfect, and her utterance was distinct, except that in speaking she was sometimes seized with spasmodic twitches of the lips and lower jaw. She lived in that state, without any change of the symp-

toms, and her general health continuing good, for about twenty years. In the morning she was taken out of bed, and placed in a chair, so contrived as to support her in a sitting posture. Her arms rested on a cross board which passed before her; and if by any accident one of them slipped from this support, she had no resource but to call for the assistance of another person to replace it. Having been on one occasion left alone for about two hours after one of her arms had thus slipped down, the hand had become extensively œdematous. In the same manner, if her head fell forward upon her thorax, it remained in that position until raised by an attendant. Her mind was entire. She died after four days' illness with symptoms of low typhus fever. You may suppose that Dr. Abercrombie looked with the greatest interest for the cause of these most remarkable symptoms. "I examined the body with the utmost care, (says he,) along with Dr. Pitcairn, who had been in the habit of seeing her for several years: and we could not discover any disease either in the brain or in the spinal cord."

It is much to be regretted that when this case was under observation, the excitatory functions were not understood, nor attended to.

I shall next proceed to consider those diseases (and there are several of them) which are marked by definite symptoms, which consist essentially in some disturbance or disorder of the nervous system, but which are not accounted for by any physical changes that we can appreciate in any part of that system. After some of these diseases we do, to be sure, sometimes meet with morbid appearances in the nervous centres; but none that are constant, or uniform.

LECTURE XXXII.

Tetanus. Its Symptoms and Varieties. Causes. Diagnosis. Pathology. Treatment: Opium; Blood-letting; the Warm Bath; the Cold Bath.

In those diseases of the nervous system which have hitherto engaged our attention, the function of voluntary motion, when it has been affected at all, has mostly suffered in the way of diminution, or suspension; the power of moving has been impaired, or lost; there has been complete or incomplete *palsy*. Sometimes, indeed, convulsion, or an irregular and involuntary action of the muscles, has also occurred. But, distinct from the paralytic affections, there is a class of *spasmodic* diseases, of which it is the main and leading feature, that the function of voluntary motion is (not morbidly heightened, as in the preternatural strength of a madman; nor lowered, as in palsy; but) *perverted*: performed in an irregular and unnatural manner.

There are two sorts of spasm. One of these is marked by a long-continued contraction of the affected muscles, not rapidly alternating with relaxation: the relaxation taking place slowly, after some time: and then, perhaps, the contraction, after another interval, coming on again. This is called *tonic* spasm; and, by Cullen, spastic rigidity. A very familiar example of it is the common cramp of the leg. In the other form of spasm, the contractions of the affected muscles take place repeatedly, forcibly, and in quick succession; and the relaxation is, of course, as sudden and frequent. This has been named *clonic* spasm. We find illustrations of it in convulsions.

Sometimes the two are mixed together in the same disease; certain muscles undergoing convulsions or clonic spasm, and certain other muscles being affected with rigidity or tonic spasm. But it is convenient to keep the distinction in mind.

We recognise these disorders by the unnatural conditions of the *muscles*: but you will please to remember that the fault lies in the *nervous* system.

With regard to spasmodic diseases generally, I may say that some of them constitute the most appalling and fatal maladies to which the human body is liable; and some

of them, though frightful to look upon, and productive of extreme distress to patients and their friends, are trivial in their consequences, and scarcely ever attended with any peril to life.

I propose first of all to consider one of the most formidable and worst of these spasmodic diseases, viz., *tetanus*: of which tonic spasm is essentially characteristic. Its name is derived from *τείνω*, to stretch.

In respect to all those diseases concerning the exact or full pathology of which we are ignorant, and which we identify by the group of symptoms they present, rather than by any organic changes of structure in any part of the body, the most convenient mode of proceeding will be, first to describe the distinctive symptoms.

Tetanus, then, is characterized by an involuntary, long-continued, violent, and painful contraction—in one word, by *cramp*—of the voluntary muscles of various parts, or of nearly the whole body.

There is no difficulty in recognising the disease when it is fully formed. But it is of much importance to be aware of the marks of its approach, and of its earliest symptoms; in respect of the treatment to be then adopted.

The muscles that seem, in general, to be the earliest affected are those of the neck, jaws, and throat. The patient feels a difficulty and uneasiness in bending or turning his head; and supposes that he has got what is called a stiff neck. He finds also that he is unable to open his mouth with the customary facility. At length the jaws close: sometimes gradually, but with great firmness; sometimes (it is said) suddenly, and with a snap. In four cases, perhaps, out of five, the disease begins in this way, with *trismus*, or *locked jaw*: so that this last is the vulgar name for the complaint. Along with this symptom, or very soon after it, the muscles concerned in swallowing become affected; and in a short time there comes on, what is often the most distressing part of the disorder, an acute pain at the lower part of the sternum, piercing through to the back. This depends, it can scarcely be doubted, upon cramp of the diaphragm. The pain is subject to aggravation in paroxysms; and each paroxysm of pain is attended with increased contraction of the other parts also that are implicated. The spasm extends to the muscles of the *trunk*: to the *large* muscles of the *extremities*: the muscles of the *face*: and last of all, in general, to the muscles of the tongue, and of the hands and fingers, which often remain moveable at the will of the patient, after all the other voluntary muscles of the body have become fixed; and frequently the muscles of the wrists and hands escape altogether.

With respect to *all* the muscles involved, from the time when they are first affected to the time when the disease is relieved, or the patient dies, they *continue* in a state of contraction, and are swelled and hard in their centres. The jaw, for instance, can never be completely opened, and the muscles of the abdomen are so rigid as to make it hard, like a board. But, besides this, they are all subject to aggravations or exacerbations of the spasm, which occur perhaps every ten minutes, or quarter of an hour, and last for two or three minutes at a time; and then the muscles fall back into the minor degree of contraction in which they were prior to the exacerbation. In a very few instances only has a perfect remission of the spasm been observed. The exacerbations usually begin by an increase of the pain felt at the sternum. Sometimes there is no obvious exciting cause of their occurrence; but frequently it is evident that they are brought on by exertions of the body; even by slight movements, such as belong to a change of posture, to the attempt at swallowing or speaking. As the disease advances, these paroxysms of aggravation become more frequent, and a rapid increase in the frequency of their recurrence is one of the most unequivocal signs that the case is severe and dangerous. The more speedily the intervals between the paroxysms shorten, the worse.

It is a curious thing, that the spasm is observed to give way, sometimes at least, and the muscles to be relaxed, during sleep. To be sure, in the severer cases, the patient is seldom able to sleep; and it may be that in the less violent instances, the spasm abates or ceases, and the exhausted sufferer sinks into repose in consequence of this abatement. However, a similar phenomenon occurs in at least another of these spasmodic diseases, as we shall see hereafter. Mr. Mayo had a boy afflicted with tetanus, in the Middlesex Hospital. On visiting him one day, he found him asleep, and remarked that he lay perfectly relaxed. The abdominal muscles were soft and yielding, and had not the least tension. The boy was awakened, and at the

instant the full tension of the muscles returned. Not being further disturbed, he fell asleep again in a few minutes, when the muscles again slackened; and again, upon his being a second time roused, resumed the state of spasm.

In most cases the strong muscles of the back are the *most* affected, and they overcome those on the anterior part of the body; so that sometimes the patient during the paroxysm rests only upon his head and his heels, while his body is raised into the shape of an arch. This form of the complaint is called *opisthotonos*, a bending backwards. The sterno-mastoid muscles of the neck have been so stretched and misplaced as to become powerful *extensors* of the head. In a few instances the body is bent forwards, so that the head and knees are in contact, and the patient is rolled together like a ball. This is called *emprosthotonos*. In the only example of *emprosthotonos* which I ever saw, these two conditions alternated with one another. The patient was a girl, in Edinburgh, under the care of a friend of mine, who took me to see her. It was a case of hysteria rather than of tetanus; but all at once she would be drawn into a position such, that the top of her head, and her feet, were alone supported on the bed, while her body was bent backwards, like a bow; then, after a time, with equal suddenness, the opposite posture was assumed, her forehead and knees being brought together. Still more rarely the body is bent to one side. This is *pleurosthotonos*, or *tetanus lateralis*; and this I never saw. Sometimes again, in the height of the spasm, the antagonist muscles counteract each other exactly; and the head and trunk are rigidly extended: and the term *tetanus* is by some writers confined to this form of the disease. It is called *trismus* when the jaw only is affected.

It is well to know that these varieties occur, and may be looked for; but in all of them — trismus, opisthotonos, emprosthotonos, or pleurosthotonos — it is the same disease; and the prognosis is not altered any more than the diagnosis, by the variety that happens.

During the fits of exacerbation, the aspect of the sufferer is often frightful. The forehead is corrugated and the brow knit, the orbicularis muscle of the eye rigid, the eyeball motionless and staring, the nostril spread, the corners of the mouth are drawn back, the set teeth exposed, and all the features fixed in a ghastly grin — the true *risus sardonius*. The tongue is apt to get between the teeth, and to be severely bitten.

All the contractions are attended with intense pain. You may form some notion of the severity of this pain, if you have ever been troubled by spasm of the gastrocnemius, or cramp of the leg, and if you can bring your mind to conceive that the same sensation which you then felt in the calf, involves nearly all the voluntary muscles of the body. The pain is worst during the exacerbations, and that which is experienced at the sternum is commonly the most complained of. Even to this, however, there are occasional, though very rare, exceptions. Sir Gilbert Blane has described a case of tetanus, which ran the usual course, and terminated fatally, yet the patient suffered no pain: the sensation excited by the violent muscular contractions was a sort of tingling, of rather a pleasurable kind.

So *violent* are the contractions sometimes, that the teeth have been broken by them. There is one case related in which the thigh bones were fractured by the forcible action of the femoral muscles; and another in which the psoas muscles were found, after death, to have been torn across. Dr. Latham tells me that he once saw one of the recti muscles, in front of the abdomen, thus rent asunder.

With all this disturbance of the muscular system, there is commonly very little derangement of the other functions of the body. The disorder is almost always attended with obstinate costiveness; partly, perhaps, from the spasmodic closure of the anus, partly, perhaps, in some cases, from the medicines that are given. When stools are obtained, they are usually very offensive and unnatural. There is no fever. The pulse and respiration are quickened, and a sweat frequently breaks out during the exacerbations, from the pain and anxiety then experienced: but this does not occur during the intervals between the paroxysms. In the last stages of the fatal cases, the pulse becomes quick and feeble, and the sweat is cold, as in other instances of approaching dissolution.

What is still more worthy of observation is that the mental functions are unaffected. There is seldom any delirium, or coma, or disturbance of the intellect. These symp-

toms only appear (if they appear at all) when other indications of the failure of the powers of life come on.

The mode of death in this disease seems to be of a mixed nature. Partly it appears to result from apnoea; the thorax being held as in a vice by the spasm of the muscles, and the breathing for a time suspended, or much embarrassed: partly, and chiefly, it occurs from asthenia; the power of the heart flags and is exhausted by the continuance of the suffering, by the fatigue and expenditure consequent upon the muscular action, and by the patient's inability, in many cases, to take sufficient nourishment. When death happens suddenly, as it sometimes does, in a paroxysm, it is owing, in all probability, to spasm of the respiratory muscles, and perhaps of those of the glottis among the rest.

Most cases of tetanus may be traced to one of two causes: which are, *exposure to cold*, especially to sudden alternations of temperature, and *bodily injuries*. In many instances both these causes co-operate in producing the disease. When it supervenes upon some bodily hurt, it is called *traumatic tetanus*: when it arises spontaneously, or after exposure to cold, it is held to be *idiopathic*. In this country, and I believe in every other, the traumatic variety of the disease is much more common than the spontaneous. But in what manner soever it may originate, tetanus is far more frequent in hot than in temperate climates and seasons. In this case, however, as in so many others, the heat appears to act as a *predisposing* cause only; the exciting cause, in addition to the wound in the traumatic species, being the application of cold (particularly, according to Hennen, of *cold air in motion*) after the heat, or during the prevalence of hot weather. Thus it is stated that after the battle of Muskau, in the midst of great heats, very few of the French troops were affected with tetanus; whereas those who were wounded in the battle of Dresden, when the weather was cold and wet, just after a very hot season, were decimated by that complaint; which did not spare even those who underwent immediate amputation.

Idiopathic tetanus is extremely rare in this country. Dr. Gregory, of Edinburgh, used to mention in his lectures the case, seen and treated by himself, of a man who, having fallen asleep in moist grass, awoke with a stiff neck, which afterwards went on into regular tetanus. A good example of well-marked tetanus, arising from exposure to cold, is narrated in the *Edinburgh Medical and Surgical Journal*, by Dr. Hall, of Berwick.

The history of that species of tetanus which occurs in connexion with wounds and injuries, presents nothing constant or uniform. The disease is liable to follow hurts of any parts of the body, and of every kind, degree, and extent; from a slight cut or scratch, to a compound fracture, or a severe surgical operation. It comes on also in various stages and conditions of the injury. Sir James M'Grigor tells us (in the sixth volume of the *Medico-Chirurgical Transactions*) that in the Peninsular war the complaint supervened "in every description, and in every stage of wounds, from the slightest to the most formidable; the healthy and the sloughing; the incised and the lacerated; the most simple and the most complicated." Sometimes, however, the discharge from the wound has been observed to be remarkably diminished, or suppressed, at the coming on of the tetanic symptoms; and sometimes the wound has healed completely before the commencement of the attack of tetanus. To show you how very trivial the injury may be, how various in kind and in place, I may mention a few instances that have been collected, in illustration of the manner in which this terrible disorder may originate. It has been known to arise from the sticking of a fish-bone in the fauces; from a slight wound of the ear by a musket-shot; from the mere stroke of a whip-lash under the eye, although the skin was not broken; from cutting a corn; from a bite on the finger by a tame sparrow; from the blow of a stick on the neck and on the hand; from the insertion of a seton; from the extraction of a tooth; from the injection of a hydrocele; from the operation of cupping.

Nevertheless there are some sorts of injury, and some parts of the body, more frequently than others concerned in the pathogeny of tetanus. The disorder more often supervenes upon injuries of the extremities, than of the trunk, head or neck; and upon wounds made by puncture than upon most other hurts. Penetrating wounds in the sole of the foot, such as are not seldom inflicted by treading upon a nail, or a splinter; and laceration, or other violence done to the muscles that constitute the ball of the thumb, are very apt to be followed by tetanic spasm. Some have supposed

that the disease has some special connexion with injuries of tendinous parts; but there can be no doubt that it is essentially a malady of the nervous tissue.

The tetanic symptoms occur at no fixed period after the reception of the injury. Professor Robinson, of Edinburgh, was once at table, when a negro servant lacerated his thumb by the fracture of a china dish. He was seized with convulsions almost instantly, and died with tetanic symptoms in a quarter of an hour. Such rapid progress as this, however, is quite out of the usual course of the disease: probably fright had something to do with it. Hennen, in his work on *Military Surgery*, states that terror is frequently the immediate antecedent of the attack. In general the tetanus supervenes between the fourth and the fourteenth day after the infliction of the injury: some time in the second week is the most common period of all. In the Peninsular war it did not commence later than the twenty-second day. In some rare instances, its accession is still longer deferred. "Of the nature of the changes that take place in the interval (justly remarks Dr. Alison) we have no information whatever." The longer, however, that the disease delays its assaults in these traumatic cases, after the reception of the local injury, the milder, in general, does it prove, and the more room is there for hoping that it will end favourably.

When the disorder arises from exposure to cold and damp, it comes on much earlier; often in a few hours. If, for example, the exposure take place during the night, the complaint may begin to declare itself the next morning.

Although tetanus may be excited by a wound, independently of any exposure to cold, or by cold without any bodily injury, there is good reason for thinking that, in many instances, one of these causes alone would fail to produce it, while both together call it forth.

After the disease has set in, its rate of progress is various. Almost all writers divide it into acute and chronic tetanus. But the difference is merely in the degree of severity. When the spasms come on suddenly, recur often from the beginning, and increase in frequency and violence, the chance of recovery is but small. The patient, in these cases, sometimes dies on the second, and generally before the fifth day. If he live to the ninth day of the disease, his prospect is somewhat better, and the spasmodic symptoms may gradually abate and disappear. Some, however, have died as late as the sixteenth, the twentieth, and even the thirty-fifth day: but this last is very rare.

The idiopathic tetanus, or that which is produced by cold, although it commences earlier, is more generally of a chronic character than the traumatic: that is to say, the spasmodic contractions take place more slowly, and the paroxysms do not increase in violence, and in rapidity of recurrence, as they are apt to do in the symptomatic variety: and accordingly this form of the malady is much oftener, I dare not say cured, but recovered from, than the other.

With respect to the diagnosis of tetanus, there is only one point in which it is at all ambiguous or important. There is no other disease that is likely to be confounded with it, except perhaps that extraordinary disease, hysteria, which sometimes mimics its phenomena. I have already alluded to one example of this kind that I myself saw. But there is a form of *poisoning* that may easily be mistaken for tetanus. The symptoms produced by a poisonous dose of strychnia, or its salts, or the vegetables from which it is procured, are the symptoms of tetanus. And as this drug is now readily obtained, and its noxious qualities are well known, it is not unlikely to be made an instrument of suicide, or of murder. It is necessary, therefore, that you should be acquainted with the effects of this poison, which constitutes the active principle of the *nux vomica*, the *fabæ S^{ci} Ignatii*, and the *upas tiuté*. Dr. Christison has excellently well described these effects as they are observable in brutes; and I have once, by accident, had an opportunity of witnessing them in the human body. I shall not be wandering from our present subject if I enumerate the symptoms to be expected from a large dose of strychnia; especially as I have lately been advising you to make trial of it as a remedy in certain forms of disease. Dr. Christison, who had made experiments with it upon animals, tells us that the creature "becomes agitated, and trembles, and is then seized with stiffness and starting of the limbs. These symptoms increase, until at length it is attacked with a fit of general spasm, in which the head is bent back, and the spine stiffened, the limbs extended and rigid, and the respiration checked by the fixing of the chest. The fit is then succeeded by an interval of

calm, during which the senses are quite entire, or unnaturally acute. But another paroxysm soon sets in, and then another and another, till at length a fit takes place more violent than any before it, and the animal perishes suffocated."

Some time ago I had occasion to prescribe the strychnia for two patients in the Middlesex Hospital, both of whom had paraplegia. I directed one grain to be intimately mixed with crumb of bread, so that it might be divided into twelve pills: and one of these pills, or one-twelfth of a grain of strychnia, was to be taken by each patient every six hours. Unluckily, through mistake or negligence of the person who was at that time the dispenser, a grain of the poison was administered at once, to each patient. It was given about seven in the evening. At half-past seven it began to produce its characteristic effect upon one of the patients. He was suddenly seized with tetanic spasms; his legs were separated widely from each other, and rigidly extended: and his head and trunk bent backwards. He was, in fact, in a state of opisthotonos. His abdomen was quite hard, and his limbs were stiff, even when the violence of the paroxysms abated. He cried out with the pain at the coming on of these spasms. Any attempt at movement, even the touching him by another person, brought them on. This is just what happens in the *disease*. The opening of a door, a sudden current of air, a shake of the floor, or bed, the smallest bodily effort, the act of swallowing, nay, even the imagination of these influences, may be sufficient to renew the spasmodic tightening of the affected muscles. My patient spoke of a particular sense of constriction all over the abdomen, as if it were drawn in. His intellect was quite unaffected. He had two extremely violent attacks of the kind I have been describing, in which he thought he should have died: and to say the truth I was myself horribly afraid of the same catastrophe. Afterwards, from half-past eight o'clock to between eleven and twelve, he had several slighter and shorter fits. He was left weak and exhausted by them: but he soon recovered. I may as well tell you that his paraplegia was not a whit benefited by this violent action of the remedy.

You may suppose that when I found one of my patients in this alarming state, I became very anxious to ascertain the condition of the other, who had taken the same quantity of the strychnia, and lay in another ward. He told me that he had been for a short period very dizzy, and had trembled all over; and at the time when I saw him, he had a weight or uneasy sensation at the nape of his neck, which drew his head backwards; and he experienced some difficulty in opening his mouth, and in articulating his words. But he thought these symptoms were diminishing rather than increasing. He was perspiring profusely. It is stated by Dr. Christison that if the spasms do not come on within two hours after the poison was swallowed, the patient is safe. It was more than two hours since this patient had taken the strychnine. I gave him a full dose of purgative medicine, which acted as an emetic: and, after he had vomited, the unpleasant sensations about his head and neck left him.

I scarcely knew what to do with the other patient, in whom the spasms had commenced. There is nothing satisfactorily made out, that I know of, concerning the mode of treating such cases. Of course, if one saw the patient early, and knew what he had swallowed, the first thing to be done would be to procure its evacuation from the stomach. But here it had had full time to get into the circulation: and no emetic could have withdrawn that part of it at least, which had already found its way into the blood-vessels. When I reached him, though the spasms were strong, they were less violent than they had been, and their violence seemed upon the wane: but they were brought on by any, almost the slightest, muscular effort, or change of position. I hoped therefore that the most dangerous period was passing off (and so it turned out), and I was fearful of doing harm by exciting those movements of the body which accompany the act of vomiting. I recollected, too, that another patient in the hospital, under the care of one of my colleagues, had once been attacked with opisthotonos after taking half a grain of strychnia; and that brandy and water had been given to him; and that he got well from that time, without having another paroxysm. So I gave my patient some brandy and water; and he seemed the better for it: but whether or no it contributed much to his recovery I cannot be sure.

Now how are we to tell, when we meet with such symptoms as these, whether they are the result of disease, or of poisoning? The symptoms are the symptoms of tetanus; I know of no test whereby to distinguish them from the symptoms of tetanus caused by exposure to cold, or supervening upon a wound, except the period of time

over which they are spread. Dr. Christison states that "the disease never proves so quickly fatal as the rapid cases of poisoning with *nux vomica*." "Besides, the fits of natural tetanus are almost always slow in being formed, while *nux vomica* brings on perfect fits in an hour or less." If indeed the case related by Professor Robinson, in which the negro was dead in fifteen minutes, could be relied on as having been a genuine instance of tetanus, this distinction, drawn from the rapidity with which the poison kills, would scarcely hold. But that case is unique, and of such doubtful value that it need not disturb our estimate. Again, persons who have taken an overdose of strychnia sometimes survive the tetanic symptoms, but die afterwards from the irritant effects of the poison upon the mucous membrane of the alimentary canal. This we do not observe in the disease. "It is right to remember, however (adds Dr. Christison), that *nux vomica* (or its poisonous element, strychnine), may be given in small doses, frequently repeated and gradually increased, so as to imitate exactly the phenomena of tetanus from natural causes." In suspicious or questionable cases, we must look into the history of the patient; inquire whether he were likely to destroy himself, or to be murdered by others; what he last swallowed, and when it was taken; whether he have lately been exposed to the injurious influence of cold, especially to a stream of cold air while he was perspiring; and whether he have recently received any bodily hurt. By a careful investigation of all the circumstances, we shall generally be enabled to decide the true nature of the case; but it is clearly necessary that our eyes should be open to the possibility of a case of poisoning, by some of the preparations of strychnia, being palmed upon us for a case of natural disease.¹

The *pathology* of tetanus is undoubtedly obscure: but not more so, I conceive, than that of those nervous diseases in general which produce violent symptoms, and even death itself, without leaving any traces of their operation inscribed upon the dead materials of the body. Nay, it is not so obscure as several others. I think we may fairly come to the conclusion that the symptoms result from some peculiar condition of the *spinal cord*, produced and kept up by irritation of its substance, or of its afferent nerves; and that the *brain* is not involved in the disease. The French (at least some of the most modern writers on tetanus) hold that it is always an inflammatory complaint; and that it consists essentially in inflammation of the spinal marrow: and some of them have sought to remedy it by enormous blood-lettings; from fourteen to fifteen pounds of blood being taken in the course of a few days by one practitioner; and another bleeding his patient eight times, and applying 792 leeches along the course of the spine, and to the epigastrium. But this doctrine of inflammation being at the bottom of every case of tetanus is contradicted by the plainest facts; and the practice founded upon it has been pushed to a most extravagant and absurd extent. Numberless instances occur of inflammation of the spinal cord and its membranes without any tetanus; and equally numerous examples of tetanus have been met with, when no unnatural appearance at all could be discovered within the vertebral canal. I say we must content ourselves with referring the phenomena of the disease to *irritation*, direct or indirect, of the spinal cord; or of its nervous appendages. It is conjectured by Dr. Todd and Mr. Bowman that the changes which take place in the nerves, and in the nervous centres, whereby sensations and muscular contractions are produced, are molecular changes, rapidly propagated from the point where the stimulus is applied; and analogous with "that remarkable change in the particles of a piece of soft iron, in virtue of which it acquires the properties of a magnet so long as it is maintained in a certain relation to a galvanic current; these properties being instantaneously communicated when the circuit is completed, and as instantaneously removed when it is broken. A *state of polarity* is induced in the particles of the nerve by the action of the stimulus, which is capable of exciting an analogous change in other particles, whether muscular or nervous; whence results the peculiar effect of the nerve's influence."

¹ This warning, suggested in the first delivery of these lectures, has been terribly justified by subsequent events. The names of Palmer and of Dove will ever retain an infamous celebrity in the annals of our criminal jurisprudence, for cold-blooded murderings by the poison of strychnia. Wainwright's case, which was less notorious, but of precisely the same character with Palmer's, had occurred previously. It forms the basis of Sir E. Bulwer Lytton's well-known novel *Lucretia*.

In accordance with this theory these authors hold, with great show of reason, that in tetanic spasm, the natural polar force of the spinal cord is greatly exalted, and kept so, by the constant irritation applied directly to the cord itself, or propagated to it by the nerves of the injured part.

If you *irritate*, mechanically, by means of a pair of forceps, the exposed spinal cord of a recently decapitated animal, a turtle for example, you produce spasmodic contraction of the limbs. What difficulty is there in supposing that some mechanical irritation existing within the spinal canal of a living man may have a similar effect? It may be, and probably is, sometimes, the mechanical irritation caused by the altered state of the blood-vessels under inflammation; for sometimes we do find traces of such inflammation in the spinal marrow after death by tetanus.

Again, if you irritate, by pinching, one of the spinal nerves of a turtle whose head has just been cut off—if you thus irritate one of these nerves in any part of its course, what happens? why the muscles of the limbs contract spasmodically; those on the side to which that nerve belongs become rigid, and those on the other side also. That property of the cord comes into play which I have so often mentioned: a property which it possesses independently of the brain; which it evinces when all communication with the brain is cut off; a property, therefore, which may be manifested without any exercise of volition, and even in spite of efforts made by the will to restrain its manifestation: I mean, of course, the property whereby it is capable of receiving impressions through the medium of its afferent nerves, from parts at a distance, and of originating motion in the muscles of the trunk and limbs through the medium of its efferent nerves. By the courtesy of Dr. Marshall Hall I have been afforded the opportunity of witnessing, in the headless turtle, the phenomena that I have been describing to you. Surely they throw a broad light upon the pathology of tetanus, and of sundry other affections. We infer from them, most legitimately as it seems to me, that the tonic spasm which characterizes the disease we are considering, may be caused by a morbid condition of the spinal marrow itself; or of the nerves that belong to it. In the latter case, irritation is set up at the free extremity, or somewhere in the course, of incident nerves; along these nerves an influence is conducted to the cranio-spinal axis, in which a process or change takes place, whereby an answering influence is reflected to the muscles along motor nerves: and the whole circle of action and reaction is run through with the suddenness and swiftness of lightning, or of thought. You cannot expect that visible marks of the irritating cause should, in all cases, be left upon the body; any more than you could discern the pinch made by the forceps after they were withdrawn.

When, in the experiments to which I have referred, Dr. Hall plucked at, or compressed, one of the denuded spinal nerves, spasmodic motions were excited in the muscles of *both sides*; and *above*, as well as *below*, the junction of that nerve with the cord. This shows that the change (whatever it be) that is wrought in the cord by impressions made upon one of its afferent nerves, is not necessarily confined to the corresponding *segment* of the cord; but may be instantly communicated, in both directions, throughout its entire course: the whole of this centre of the excito-motory system responding to the influence conveyed by a single nerve, as completely as a tight string vibrates from end to end, when struck at any one point. We frequently, indeed, find that the excited motions are more limited; but it is important to mark this ready consent of the whole cord, under sufficient excitement.

Dr. Hall has given certain distinguishing epithets to tetanus, according to the supposed source and locality of the irritation. When the irritating cause operates directly upon the spinal cord itself, he calls the disease *centric* tetanus; when it resides in some part of the body distant from the spinal cord, he calls the disease *eccentric* tetanus. These are good and intelligible names; and I shall take leave to adopt them.

Observe now how well this explanation meets the facts of the case. We sometimes find the spinal cord or its membranes inflamed, when there has been tetanic spasm. We then refer the spasm to the centric irritation. But in a far greater number of cases we can detect no marks whatever of disease in the spinal canal, while we know that an irritating cause has been applied to parts at a distance. Often we have evidence which is visible, that a nerve has been injured, torn across perhaps, or half torn, or compressed in some way or other; just as we might compress a nerve, with a pair of forceps, in a decapitated turtle. That experiment shows us that very slight

irritation may be enough to produce the spasmodic action; and we find that slight injuries, as well as severe, will bring on the disease, when, by the operation of certain injurious agencies, the frame has been predisposed, and rendered morbidly susceptible. There is no part of the trunk or limbs which is not supplied with nerves from the spinal cord; and we find that injuries of various parts, or of almost any part, in an individual predisposed to take on the disordered action, may produce it. The exciting cause may be a wound irritating a particular nerve: it may be exposure to cold, acting upon the extremities of various nerves that proceed from the surface: it may be a bundle of worms, irritating the nerves spread upon the mucous tissue of the alimentary canal; for I omitted to state before that some writers, especially MM. Laurent and Lombard, have maintained that tetanus is almost always, even when it supervenes after wounds, the result of the presence of worms in the digestive organs. They have founded this opinion upon the *fact*, that worms have been very frequently indeed discovered in the stomachs or intestines of persons dead of this disorder. I think this is a point well worth attending to. It is objected that worms infest the human body without causing tetanus: but the very same thing may be said of the operation of cold; and of external injuries. Any of these may probably excite the disorder, when the body is preternaturally susceptible of it. The real mystery lies in this predisposition. We have reason to suppose that a high atmospheric temperature, continued for some time, is *one* predisposing cause; but how it operates, or what is that state of system in which the increased susceptibility consists, these are points concerning which we are really in the dark.

The disease is common enough in brutes: and it is frequently *eccentric* in them; brought on by injuries, mostly of the extremities. Locked-jaw is a familiar term in the nosology of farriers. It is not uncommon in the horse after castration. I remember a mare belonging to my father dying of that disease a few days after foaling. It often results, in these animals, from a prick in the foot, by a nail, in shoeing. Dr. Parry noticed eccentric tetanus in lambs. "I have often seen lambs," says he, "whose ears, for the purpose of marking them, have been bored with a red-hot iron too near the root, so rigid all over with tetanus, alternating with convulsions, that their bodies would project in a right line with their hind legs, when one held them out horizontally by the hind feet." Dr. Mason Good tells us that parrots also are frequently affected with trismus: a calamity which, supposing the bird to be within ear-shot, it would be difficult to commiserate.

We are not advancing any wild theory, then, respecting the controverted pathology of this disease, when we lay down the following propositions; that it is essentially a disorder of the excito-motory apparatus; that it results from irritation of a peculiar kind, affecting that part of the nervous system; that the irritating cause may be centric,—within the spinal canal itself; that, again, it may be, and often is, eccentric,—situated at the extremity, or somewhere in the course, of one or more of the afferent spinal nerves; and that a certain predisposition is for the most part necessary, to render the body susceptible of the disease under the operation of the exciting irritation.

At one time it was supposed that the physical cause of the disorder had been detected, in the presence of more or fewer thin scales of bony or earthy matter, lying in or upon the arachnoid of the cord. I have myself seen these after death preceded by tetanic symptoms. But tetanus often happens and proves fatal without them: and they are often met with when there has been no tetanus. If, therefore, there be any connexion between these thin plates of ossification and the occurrence of tetanus (which may well be questioned), it must be of this kind; that the scales of earthy matter predispose the spinal cord, somehow, to be affected by the exciting causes of the disease.

The doctrines recently propounded by Dr. Marshall Hall, of which the importance becomes daily more apparent, and by which his name will be enduringly connected with the physiology of the nervous system, receive a strong confirmation from the phenomena of tetanus. They furnish a key to many problems which had previously perplexed the pathologist; and they do this simply by distinguishing the proper functions of the two distinct nervous centres; the brain and the spinal cord. But the practical application of these new views is yet in its infancy.

The *treatment* of tetanus is a mortifying subject. The disease is and has always

been a lamentably fatal one. Hippocrates says, *ἐπὶ τραύματι σπασμὸς ἐπιγενομένος, θανάσιμος*, tetanus supervening on a wound, is mortal: and the aphorism holds true, with very few exceptions, in the present day. Almost all the acute and severe traumatic cases are fatal. Hennen declares that he never saw a case of "acute symptomatic tetanus" recover. Dr. Dickson found all curative measures followed by "unqualified disappointment." Mr. Morgan uses these words: "I have never yet seen or heard of an instance of recovery from acute tetanus." Another of Hippocrates' aphorisms is, *ὅσοις ὑπὸ τετανὸν ἀλίσκονται ἐκ τεσσάρων ἡμέρων ἀπολλύνται*, they who are seized with tetanus die within four days; but he adds, *ἢ δὲ ταύτας διαφύγῃσι ὕμεις γίνονται*; if they get over this period they recover. And to this we can only add now, that those who survive the first few days, and ultimately get well, recover in a variety of different ways, and under various modes of treatment. But as to the mode of treatment which is to be preferred, or even as to the real efficacy of any mode, there is much room for doubt. Under every plan of management a vast majority die.

Let us briefly pass in review the principal remedies that have been tried, and inquire what degree of success has followed their employment.

One drug from which much benefit has been hoped for, is *opium*. In some spasmodic disorders it is of unquestionable service. Very large doses of it have been given and borne in tetanus; and some have recovered under its use, and more have died.

It is well known that pain fortifies the nervous system against the peculiar influence of narcotic substances. We need not, therefore, be surprised that opium, administered in enormous quantities, in this painful disease, has had but little effect. I was assured by a physician, with whom I formed an acquaintance in Edinburgh some years ago, and who is known, I find, to a student now present, that his own wife, while labouring under a tetanic affection, swallowed, in twenty successive days, upwards of 40,000 drops of laudanum, which is at the rate of more than four ounces a day; in all, more than two imperial quarts. The lady recovered. A case is recorded in the second volume of the *Medico-Chirurgical Transactions*, in which an ounce of *solid* opium was taken, in divided doses, every day, for twenty-two days. This appears a more astounding instance than the former; but I am not sure that it was so; for, in this complaint, solid opium does not always dissolve in the stomach. I have heard the late Mr. Abernethy say that he had found enough undissolved pills of opium in the stomach after death, to poison a dozen healthy persons. This fact should teach you, if you resolve on trying opium at all, to exhibit it in a liquid form; laudanum, or a solution of the acetate or of the muriate of morphia. And with the internal administration of opium, it would be well, perhaps, to combine its external use; to apply a broad strip of opiate plaster along the whole length of the spinal column.

It is sometimes a difficult matter to introduce medicine by the mouth, so strong is the spasmodic contraction of the muscles that close the jaws. You cannot get the mouth open. Some persons set to work to heave it open, by levers; and it has been proposed, and I believe practised, to break off or extract a tooth or two, to make a passage for the introduction of medicine and of nourishment; but I hope you will never be guilty of such clumsy barbarity as this. Food and physic may be carried into the fauces and into the stomach by means of a flexible tube: and this may be inserted through the nostril; or through the mouth, by passing it between the jaws, behind the back teeth, where there is always an aperture that will admit a tube sufficiently large.

After all, in respect to the cures that have been ascribed to the opiate treatment, they have all (so far as I know) occurred in cases of the milder or more chronic tetanus; and mostly in the idiopathic form of the disease; and this circumstance makes it a question whether they were *cures* at all; whether they were not simply recoveries.

Dr. William Budd (in the paper already referred to) challenges, on physiological principles, the propriety of giving any opium in this disease. He says, "It has been ascertained that the effect of that drug is to excite, and not to quiet, the motor function of the spinal cord: indeed, it is well known that the motor acts of the cord may be rendered much more active and powerful, by giving, before decapitation, opium to animals that are to be subjects of experiment." He considers "these objections,

furnished by theory, to be motives sufficient for the future exclusion of opium from the treatment of tetanus."

I had long been aware that the effect of opium upon frogs was to produce tetanic spasms. But in no case of poisoning by opium in the human subject (and I have seen a great many) have I ever witnessed any approach to tetanus: and I much question the safety of arguing, in such matters, from what we know to happen in the inferior animals, to what we suppose would happen in man.

The failure, however, of opium in the severer forms of the malady, and its equivocal utility in any, taken together with these theoretical objections, prevent my recommending opium as a remedy for tetanus.

What is the result of experience in regard to *blood-letting* in tetanus? I am afraid that, as a curative agent, it has very little power over the disease. Yet it may be, in certain cases, of use, as an auxiliary to other measures. When the disorder bears any aspect of inflammation—when, for instance, fever is lighted up, and pain is felt along the course of the spine, or when the approach of the spasm is marked by the super-vention or the increase of pain in the wound—then our chance of doing good by venesection is the greatest. Some of the cases that happened in the Peninsular war, were decidedly benefited by blood-letting practised under such circumstances. I need scarcely say that though the bleeding, when adopted, should be early, free, and full, so as to produce some sensible impression upon the system, yet we must always use this remedy with caution. The tendency of the disease is to exhaust the power of the heart; and if by one over-bleeding we bring that organ to a stand-still, it may refuse to begin again.

In a complaint which depends so much on irritation, and so often on manifest irritation of external parts, we look naturally to the *warm bath* for help. And it has been fairly tried: and some persons have found it useful; and others have found it useless, doing neither good nor harm; and some have condemned it as actually hurtful.

The *cold bath* has been extolled as a much more powerful agent than the warm; and so, doubtless, it is. But it is more potent for harm as well as for good. For example: a tetanic patient, in St. Thomas's Hospital, was plunged into a cold bath, at his own request. "All the symptoms disappeared (says Mr. Morgan) in a moment; and he was almost immediately taken out of the bath; but he was taken out lifeless." Sir James McGrigor says that, during the campaign in Spain, "the warm bath gave only momentary relief; and the cold bath was worse than useless."

However, the application of cold water to the surface has, in many recorded instances, been of at least temporary benefit and comfort: and in the West Indies, where the disease is common, the cold affusion still continues, I believe to be the most favourite expedient. After it, the patient is rubbed dry, put to bed, and has laudanum administered. I have again to observe, of this remedy also, that it is chiefly serviceable in the idiopathic form of tetanus. It has been tried upon animals. Dr. Parry says that it was quite unavailing in the case of certain lambs that had the disease. In a note, which I made at the time, of Mr. Abernethy's lecture on tetanus, I find the following statement. "The effect of cold in diminishing excessive muscular action was strikingly shown in the case of a horse belonging to Professor Coleman, which had tetanus. The animal was slung, and carried out of the stable, and laid on the snow, which was then on the ground: and he was covered over with snow also. A horse affected with tetanus is a curious sight. His legs straddle, and become stiff; his ears are pricked up; and his tail sticks out. In this case, on the application of the snow, his ears sank, his tail became pliant, and the rigidity of his muscles was removed. He was again taken into the stable, and the spasms returned." Mr. Abernethy said, that were he himself the subject of tetanus, he would desire to have the cold affusion tried. If you are willing to assay the same remedy, do not plunge your patient into a cold bath, but take him out of his bed on an extended sheet, pour cold water over his body, wipe him dry, and place him in another dry bed. This will often, for a time at least, diminish the spasmodic action; and the patient will sometimes sleep comfortably after it.

Dr. Todd has suggested to me the application of *ice* to the *spine*; a measure which he has found eminently beneficial in convulsions. This mode of employing cold as a remedy in tetanus seems well worthy of trial. It would have the advantage of not inflicting any shock which might excite or disturb the reflex function of the cord, through its incident nerves.

LECTURE XXXIII.

Treatment of Tetanus, continued. Wine; Mercury; Purgatives; Digitalis; Tobacco; Musk; Prussic Acid; Belladonna; Carbonate of Iron; Oil of Turpentine; Strychnia; Surgical Expedients; General Rules. Hydrophobia.

IN the last lecture we considered the symptoms, the nature, the causes, and to a certain extent the treatment, of that terrible malady, *tetanus*. There is good reason for believing that it is essentially a disorder of the excito-motory apparatus: that it is caused by irritation of a peculiar kind, affecting that part of the nervous system, and producing tonic spasm of the voluntary muscles: that the irritating cause may be centric, situated within the spinal canal, and applied directly to the cord; or eccentric, situated out of the spinal canal, applied to some part of one or more of its afferent nerves directly, and thus influencing indirectly the cord itself, and through it the reflex motor nerves: that a certain ill-understood state of the system is necessary, a certain aptitude to take on the disease, before the exciting cause can be efficient; and that one circumstance which has been ascertained to tend to the production of such an aptitude, is a long-continued high temperature of the atmosphere.

I mentioned several remedies and plans of treatment which have been fairly tried, and mostly tried in vain, for the removal of this disease. The severe cases, and especially the severe traumatic cases, almost all prove fatal; the less severe cases, those in which the paroxysms are less violent and less frequent, and which run on for several days, sometimes terminate in health: whether in consequence of the measures employed, or whether in spite of them, it is not easy to say. The idiopathic cases, as they are called, those which appear to be produced by exposure to cold and wet, are usually the less severe, and the more hopeful. The remedies that have been tried, and which were mentioned in the last lecture, are opium; blood-letting; the warm-bath; the cold-bath. I showed you that, under each of these remedies, a great number of patients died, and some recovered; and that the recoveries had been almost exclusively amongst those patients in whom the disease appeared originally in its milder form. So that whether the complaint was actually cured in any of these cases, whether, *i. e.*, any single patient recovered, or recovered sooner, from using any of these remedies, who would have died, or in whom the disease would have been protracted, if he had *not* used them, is a matter of uncertainty.

The celebrated American physician, Dr. Rush, regarding the disease as essentially a disease of debility, and looking, probably, at its common tendency to death by asthenia, wrote a paper to recommend the employment of bark, and wine, and spirits, in full doses. It is curious enough, but quite in agreement with what has been already stated of opium, that how much wine soever may be swallowed by the patient, nothing like intoxication is produced by it. The system resists the ordinary influence of the alcohol. In one instance related by Dr. Currie, the disease lasted six weeks, and in that space of time the patient drank 110 bottles of port wine. The same author mentions a remarkable case, in which a horse, which was attacked by tetanus, and happened to be a great favourite with its master, was treated with wine, and got well, after swallowing more port wine than he was worth. Whenever *this* plan has appeared to do good, it has been in the more chronic variety of the complaint.

Mercury, you may be sure, has not been left untried. It is said that the system is slow in submitting to its influence, in this malady. The specific effect of mercury upon the gums is not, however, so strongly resisted as that of wine or opium upon the nerves. Nor can we be surprised at this, when we consider that in tetanus the functions of organic life are, comparatively, but little involved. It is clear that there is not time for any effectual exhibition of mercury in those severe cases that are early fatal. In its more chronic form the disorder has been known to yield upon the mouth becoming affected. This happened, if I mistake not, in Mr. Mayo's patient, mentioned in the last lecture. Tetanus has sometimes, however, commenced while the patient was in a state of salivation. Dr. Wells has recorded three instances of

that kind. The experience of the military surgeons who were in Spain, is, upon the whole, against the reputed efficacy of mercury. We must take care not to conclude too hastily, that because a patient uses a certain remedy and recovers, he recovers through the operation of that remedy: any more than we should conclude, if he recovered during a general election, that the election had cured him. Yet this absurd and unsafe mode of reasoning is for ever employed in respect to disease, by the public; and too often, I fear, by ourselves.

Purgatives have been much given in tetanus; and often with manifest advantage: I mean in the less severe cases. But very large doses are commonly required to procure evacuations from the bowels. Whether the torpor of the intestines be always the effect of the disease, or whether it may not sometimes be, in part at least, a consequence of the opium that is given, I am not sure. When they do act, very unnatural motions are frequently produced. Mr. Abernethy tells us of a hospital patient of his who recovered under the use of purgatives; they were long before they had any effect, and when they did at last operate, such foetid stuff came from him, that no one who could crawl out of the ward would remain in it. He says also that the nurses, in other cases, have reported the stools to be more like sloughs than fæces. Enormous quantities of drastic purgatives have been given. You may read an instance of this in the second volume of the *Medico-Chirurgical Transactions*. It is related by Mr. Harkness. There is a still more extraordinary case detailed by Dr. Briggs, in the fifth volume of the *Edinburgh Medical and Surgical Journal*. In little more than 48 hours, the patient in that case took 210 grains of scammony, 89 of gamboge, 80 of calomel, an ounce and four scruples of jalap, and $2\frac{1}{3}$ pints of what we call *black dose*, the infusion and tincture of senna: and all this without either sickness or griping; but on the contrary, with the most decided benefit. In the first week of his disease, the patient swallowed — of calomel, 280 grains; scammony, 260; gamboge, 110; jalap, 3 ounces and 10 grains; infusion of senna, $5\frac{3}{4}$ pints. And altogether in the first 25 days — of calomel, 320 grains; scammony, 340; gamboge, 126; jalap, 5 ounces and $7\frac{1}{2}$ drachms; infusion of senna, $10\frac{3}{4}$ pints; besides an ounce and a half and 35 grains of the colocynth pill. I mention all this to show what the system will bear, under the bondage of the disease; not as an encouragement to you to prescribe such doses.

It is certainly proper and necessary to clear out the bowels, and to endeavour to correct unhealthy secretions; yet numerous evacuations, the act of going to stool, often repeated, should be avoided. Under such obstinacy of the bowels, the croton oil would perhaps be the most eligible purgative.

Foxglove and tobacco are two medicines, or rather poisons, which have been used; both, probably, upon the same principle. Their effects, when full doses have been given, are much alike: sickness, faintness, feebleness and fluttering of the pulse, coldness of the surface, with that slack and passive state of the muscles which belongs to syncope. But if we consider that the influence of these substances upon the involuntary muscles, especially upon the heart, is more certain and decided than upon the muscles of voluntary motion, which are the muscles involved in the tetanic spasm, and if we take also into account the strong disposition observable in tetanus towards death by asthenia, we shall scarcely be prepared to expect any good, but the contrary, from digitalis, or tobacco; especially in the latter periods, when, so far from obviating the tendency to death, they would seem to co-operate with the disease in extinguishing life. However, if the result of experience were clearly in their favour, we should not be warranted, by mere theoretical views, in withholding these drugs. The army surgeons, some of them, have fancied digitalis useful. Sir James M'Grigor mentions a case in which it caused a relaxation of the spasms; but the man died afterwards, apparently from the effects of the remedy. And this is just what I find with digitalis. When given in large doses (and small ones here must be useless) it becomes unmanageable. Certainly we have no such accounts of its sanative power as would induce me to give it with much expectation of success, or to give it at all.

The tobacco is not given by the mouth, but thrown up into the rectum: either the smoke of its burning leaves, or (what is probably as efficacious, and I should think more uniform and less unsafe) an infusion of them in water. Mr. Curling, after analysing a large number of cases of tetanus, thinks tobacco the best remedy we at present possess. Mr. Travers is of the same opinion. However, I should re-

commend great caution in the use of this ticklish remedy. You ought to know that, when injected in other emergencies, in strangulated hernia, for example, mortal syncope has followed such enemata.

Musk, in large doses, has been strongly recommended by a Frenchman, Fournier-Pescay, who has written on this disease. He gave ten or twenty grains at intervals; so that a drachm, or even two drachms, were taken in the course of the day; and he declares that he found it more efficacious than anything else that he had tried.

Prussic acid and *belladonna* are said by Dr. Elliotson to have been freely prescribed, and to have failed; whether in his own hands, or in those of others, I do not know.

There is another remedy which the same physician has employed; and employed not without success: *the carbonate of iron*. Reflecting, he tells us, upon the effects of this medicine in another complaint which has some points of analogy with tetanus, viz. chorea, of which I shall soon speak, and considering how miserably narcotics had failed, he determined to give the carbonate of iron a fair trial upon the first opportunity. He has published some account of its effects, in tetanus, in the *Medico-Chirurgical Transactions*. In the first case in which he used it, the tetanus supervened upon a compound dislocation of the great toe. The method in which the remedy was administered was this. The carbonate was made into an electuary by mixing it with twice its weight of treacle. The electuary thus made was well stirred in beef-tea just as the patient was about to drink it. He took this every two hours, as much as he could swallow: and he got well. The next case is described as being a very severe one; it resulted from a contusion of the thumb. Dr. Elliotson says that he never saw a case, *which did well*, that was more severe. This patient also took the carbonate of iron, as much as could be got down; and that was about two pounds a day. He had injections twice daily, to keep the bowels unloaded: and the iron is described as having come away in large red lumps, in shape like horse-dung. This man recovered. In a third case, where a chilblain above the heel was the exciting cause, the boy died within twenty-four hours of the time when the remedy was first prescribed. To produce its influence upon the system (Dr. Elliotson observes, very truly) iron must be given *for a few days*: nay, he holds that months sometimes elapse before it has any effect. So that if it really be useful in tetanus, we cannot expect much good from it in the more acute cases: and these are the cases for which we want a remedy.

Oil of turpentine is one of the many substances that have been praised as useful in tetanus. Now, bearing in mind its power (which I shall hereafter describe, but which you must at present take for granted) as a worm-killer, and also the frequency with which worms are met with in the stomach and bowels after death by tetanus, this is one of the drugs which I should employ as a *purgative*, taking my chance of whatever good might possibly arise from its specific or anthelmintic qualities. It may be given in such cases either by the mouth, or in an enema, or at both ends together: but it must be given in large doses, not less than an ounce at a time; and it may be mixed with an equal quantity of castor oil. The one oil dissolves, or becomes incorporated, in the other.

Strychnia has been suggested as a remedy for severe tetanus; not in infinitesimal doses, as Hahnemann would, I suppose, prescribe it, but in sufficient quantity to produce a sensible effect. The principle upon which this has been recommended is the same with that on which the nitrate of silver ointment is applied to the inflamed conjunctiva in purulent ophthalmia. We know that strychnia acts upon the spinal cord, affecting apparently those parts and those functions of the cord which are affected in tetanus; and in so fatal a malady, it would be justifiable, I conceive, to give the strychnia, in the hope that it might occasion a morbid action which would supersede the morbid action of the disease, and yet be less perilous and more manageable than it. But it would be right to try such a remedy as this, in the first instance, *in corpore vili*; upon one of the lower animals. This, were it successful, would be a cure, according to the Hahnemannian doctrine—*similia similibus curantur*—a doctrine much older, however, than Hahnemann. But the opposite maxim, *contraria contrariis*, has been suggested. Mr. Morgan proposes to give such poisons as are known to cause paralysis, with the view of countervailing the undue action of the muscles in tetanus. He produces artificial tetanus by inserting a poison brought

from Java, called "chatic," into a wound, and then relieves the tetanic symptoms by a North American poison, the ticunas. Professor Sewell, of the Veterinary College, has tried this principle in one case at least, where the tetanus was the result, not of any poison, but of disease. Not having had an opportunity of getting the particulars of this case from Mr. Sewell himself, I give you Mr. Mayo's account of it. "A horse, suffering from a severe attack of tetanus and locked-jaw, the mouth being too firmly closed to admit the introduction of either food or medicine, was inoculated on the fleshy part of the shoulder with an arrow point coated with the wourali poison. In ten minutes apparent death was produced. Artificial respiration was immediately commenced, and kept up about four hours, when reanimation took place. The animal rose up, apparently perfectly recovered, and eagerly partook of corn and hay. He was unluckily too abundantly supplied with food during the night. The consequence was over-distension of the stomach, of which the animal died the following day, without, however, having the slightest recurrence of tetanic symptoms." I had fancied that the death had resulted from some injurious effect upon the lungs, produced by the artificial breathing. But I have little doubt that Mr. Mayo derived his statement from Mr. Sewell himself. The experiment deserves to be carefully repeated.

The virtue of the vapour of æther, or of the newer substance, chloroform, will doubtless be put to the proof in these diseases of painful spasm. Nay, I read in the newspapers and medical journals that this remedy has already been tried in tetanus; and not without marked relief of suffering. That it will prove equal to the cure of the severer cases, which would end fatally without it, is, in my mind, a matter of hope rather than of expectation.

I have but little to say concerning what may be called the surgical treatment of traumatic tetanus. It was a natural thing, the source of the irritation being supposed to be the wound, to expect relief from amputation of the limb. But that will not arrest the morbid action after it has once been fairly established. Dr. Elliotson says he has searched scores of books, and found only one case in which the limb and the disease were lopped away together. However, Mr. Blizard Curling, in his *Essay on Tetanus*, refers to seven instances of recovery, after the injured part had been amputated. Yet he states that "it is almost impossible to ascertain with certainty how far the amputation, in these cases, was of service." I believe I cannot offer you better advice on this subject than may be gathered from the concluding remarks of a very distinguished and philosophical surgeon, in his lectures on this disease. I allude to the late Mr. Abernethy, whose pupil I had the good fortune to be. He said, "The state of the part injured is not the sole cause of tetanus. In cases of tetanus I have often amputated injured fingers; and though I did not thereby save my patients, yet I think that the symptoms were mitigated after such amputations. In such cases, then, I would not amputate any considerable member; nor even a small one, unless I thought that, from the injury sustained, it would prove useless to its possessor, even though the case should terminate favourably."

The tourniquet has been applied to the hurt limb; but not, so far as I know, with any good effect. The most promising expedient which surgery offers is the division of the principal nerve proceeding to, or rather from, the seat of the injury. This, supposing the nerve to be known and accessible, is less formidable, less severe, less hazardous, less maiming, and, if we may judge from past experience, more effectual too, than amputation of the part. Dr. Murray has recorded (in the eleventh volume of the *Medical Gazette*,) a very interesting case in which the operation was followed by most decided and instant relief. The patient was a young midshipman, who, having trodden on a rusty nail, which pierced the sole of his left foot, had kept watch the same night upon deck, the weather being very cold. The disease began the next day, and the symptoms ran high. It was a case, therefore, of severe or acute tetanus. Without loss of time the posterior tibial nerve was divided. The limb was previously cold, and as the patient said, dead, and he had little power of moving it. He could not articulate distinctly, on account of the closed state of his jaws. The nerve was cut through by one stroke of the scalpel; and "immediately (says Dr. Murray) he opened his mouth with an exclamation; and on looking at his countenance I was astonished at the striking improvement in it. I asked him how he felt, and he said he was already much better, and that his leg had come to life again." Some stiffness

of the jaws and neck remained for a day or two; but he soon recovered. Dr. Murray refers to another case mentioned by Baron Larrey, in which division of the nerve had a similar result.

Probably, to be successful, the operation must be *early*; before the morbid condition peculiar to the disorder has had time to root itself in the nervous system.

Although, in the present state of our knowledge, there is no one remedy or plan on which we can rely for the cure of this fearful malady, we may with much confidence lay down certain general rules, the observance of which will secure to the patient the best chance of a favourable result.

Since any, the smallest, movement or impression made upon the surface, or upon the senses, will bring on the severer degrees of spasm, it is of primary importance to protect the patient against these sources of trouble, so sure to aggravate his sufferings, and so likely to augment his danger. Hence if blood-letting should be thought advisable, it should be done early, sufficiently, and once for all. There should be no repetition of venæsection, or of cupping, or of leeches, unless the circumstances and progress of the case plainly demand them. The same remark applies to the frequent use of purgatives. The bowels should be well cleared in the outset, and then let alone. The patient should lie in a darkened room; from which noise also should, as much as possible, be excluded. He should not be surrounded by a multitude of friends or attendants. He should be enjoined to speak, to move, to swallow, as seldom as he can. In the severe traumatic cases, the nerve, in my judgment, should be promptly divided. And in all cases, there being no special indications to the contrary, I should be more inclined to administer wine in large doses, and nutriment, than any particular drug. If the tendency to mortal asthenia can be staved off, the disturbance of the excito-motory apparatus may, perchance, subside or pass away.

There is a form of this complaint called *trismus nascentium*. As the name implies, it occurs in newly-born children. It is very frequent and very fatal in the West Indies; coming on usually in the second week after birth. Hence it has been called "the ninth-day disease." Another of its names in the British settlements there, is "the jaw-fall;" from the circumstance that shortly before death the lower jaw, which had previously been firmly pressed against the upper, drops on the breast. It has been said that a fourth of the infant negroes in Jamaica used to die of this disorder. Some persons refer it to the irritation produced by the retention of the meconium in the intestines; others to irritation from the wound made by dividing the navel string. A dose of purgative medicine appears to be the most hopeful remedy. The complaint is common, I am told, in ill-ventilated lying-in hospitals. Pure air must therefore be desirable as an adjuvant.

Tetanic symptoms sometimes occur (but I should think very rarely) in ague. Or paroxysms of tetanus return at regular intervals, and terminate by profuse perspiration: the patient being well during the intermissions. When such phenomena arise, the treatment proper in severe forms of ague must be adopted: what that treatment is, I shall in no long time be able, I hope, to lay before you.

Again, tetanus is occasionally a symptom in hysteria; and then the treatment applicable to hysteria must be had recourse to; especially enemata containing oil of turpentine, or the same medicine given by the mouth; and the cold affusion.

If the disease of which I have been speaking be dangerous, and very often fatal, in spite of all remedial measures, that which I propose to bring next under your attention is still more appalling; for I believe that hitherto it has been uniformly mortal. I know not that any one has ever been rescued by art, or saved by the efforts of nature, from *Hydrophobia*, after that frightful disease has once declared itself by its characteristic symptoms. The nature of those symptoms, and the absence of all definite or constant traces of organic change in the dead body, sufficiently mark the disease as belonging essentially to the nervous system, and as being essentially a *spasmodic* disease also.

What are the symptoms, stated in broad outline? These. Excessive nervous irritability and apprehension; spasmodic contractions of the muscles of the fauces, excited by various external influences, and especially by the sight or sound of liquids, and by attempts to swallow them; and extreme difficulty, amounting sometimes to impossibility, of drinking.

This is one of the diseases which are produced by animal poisons; and its course will be most conveniently traced if we include in our description of it the very first step towards its production, — the application of the specific poison to the body. A man is bitten by a dog. After a time the symptoms proper to hydrophobia come on. After another, but much shorter, interval, the man is dead. Before we advert to the many very interesting points of inquiry which arise out of the contemplation of this malady, let us follow the tragedy from its commencement to its closing scene.

A person is bitten, then, by a mad dog. Does the existence of rabies in the animal modify in any way the injury thus inflicted? No; the wound that is made behaves just the same to all appearance, as it would have behaved if the dog had not been rabid; and it gradually heals. After an uncertain interval — which lies, *for the most part*, between six weeks and eighteen months, and which has been called the period of *incubation* — the following symptoms begin to be noticeable. The patient experiences pain, or some uneasy or unnatural sensation, in the situation of the bite. If it have healed up, the cicatrix tingles, or aches, or feels cold, or stiff, or numb; sometimes it becomes visibly red, swelled, or livid; on one occasion a papular eruption took place around it; sometimes it opens afresh, and discharges a peculiar ichor. The pain or uneasiness extends from the sore or scar towards the central parts of the body: *i. e.* if the bite have been inflicted on a limb, the morbid sensations extend towards the trunk. All this gives fearful notice of what is about to happen. This period is called the period of *recrudescence*. I believe it seldom fails to occur, although it sometimes is not noticed; the attention of the patient, and of his medical advisers, being absorbed by the horrible sequel. Very soon after this renewal of local irritation — within a few hours perhaps, but certainly within a very few days, during which the patient feels uncomfortable and ill — the specific constitutional symptoms begin: he is hurried and irritable; speaks of pain and stiffness, perhaps, about his neck and throat; unexpectedly he finds himself unable to swallow fluids, and every attempt to do so brings on a paroxysm of choking and sobbing, of a very distressful kind to behold; and this continues for two or three days, till the patient dies exhausted; in the way of *asthenia*.

I have seen three, perhaps four examples of this terrific malady. As they constitute the whole of my personal experience in the matter, I shall relate these cases.

The first occurred in the year 1826, in the person of a coachman, the back of whose right hand had been struck, ten weeks previously, by the teeth of a terrier dog; but, as both the patient and his fellow-servants declared, there was no wound made, no blood drawn, no breach or lifting of the skin; but merely an indentation, showing where the animal's teeth had pressed. He was brought to St. Bartholomew's Hospital on a Tuesday. On the preceding Thursday his hand had become painful, and swelled a little. On Friday the pain extended into the arm, and became more severe. His wife stated that he had been in the habit of sponging his head and body every morning with cold water, but that, on this morning, he refrained from doing so, on account of some feeling of spasm about the throat. His own remark on this was, that "he could not think how he could be so silly."

On Saturday the extent and the severity of the pain had still further increased. On this and the preceding night he got no sleep. He felt ill and drowsy on the Sunday, but drove a carriage to Kensington Gardens: he was, however, obliged to hold both whip and reins in his left hand. The pain extended to the shoulder. He was then bled. A slop-basin full of blood was taken, with much relief to the pain; and purgative medicine was given, which operated well.

The next day he complained of "feeling very ill all over," and he told his medical attendant that he could not take his draughts, because of the spasm in his throat. That gentleman (Mr. Macdonald), concealing his own suspicions as to the true nature of the disease, said, "Oh, you don't like the taste of your physic! drink some water." But he declared he had the same difficulty with water. The next day he came to the hospital. When there, water was brought and placed before him in a basin, for the alleged purpose of allowing him to wash his hands. It did not seem to disturb him, nor to excite any particular attention. Water was then offered him to drink, which he took, and carried to his mouth, but drew his head from it with a convulsive shudder. After this, on the same morning, he was much questioned by several persons about the supposed cause of his illness; and water was again brought him,

which agitated him, and he became exceedingly distressed and unquiet, complaining of the air which blew upon him.

I first saw him myself soon after this. He was then, to all outward appearance, well; lying on his back, without spasm, without anxiety; his face somewhat flushed. He said he had a little headache, but no pain in the arm. His pulse was 132, full, and strong; his tongue moist, and slightly furred. He appeared to be a very quiet, good-tempered man; and smiled generally when he was spoken to.

I was naturally much interested by this case, and at nine in the evening I visited the patient again. He was composed and tranquil. Gruel was mentioned, and then he sighed two or three times deeply; then sat up, and, after a moment's look of serious terror took half a spoonful of the gruel in a hurried gasping manner; and said he would not take more at a time, lest *the sensation* should come on. He was desired to drink the last portion of the gruel from the basin. He accordingly seized it with hurry, carried it to his mouth with an air of determination, and then a violent choking spasm of the muscles about the throat ensued, the sterno-mastöidei starting strongly forwards. Most of the gruel was spilled over his chin; and he observed that he had been too much in a hurry, or he should have managed it.

The treatment consisted in full doses of opium, repeated at frequent intervals. On this visit to him I noticed, that while attempting to take some of the gruel with a spoon, he seemed inclined to doze as he sat. Otherwise there were no signs of his being overwhelmed, or even sensibly affected by the opium, unless indeed his general quietness was the consequence of it. He was quite rational and calm, except when attempting to take fluids.

On the Wednesday, at noon, he was nearly in the same state, but said he was better. In the course of the night some morsels of ice had been given him: with considerable effort he swallowed two or three of these; the third or fourth caused so much spasm, however, that he was obliged to throw it out of his mouth: but so great was his resolution that he seized it again, and, by a strong exertion, succeeded in swallowing it. He complained now that his mouth was and had been clammy; and he champed much, and spat out a good deal of tough mucus. At his own request, and (as he said) that he might injure no one, a strait-waistcoat was brought, which he assisted in putting on. But he was perfectly tranquil then.

I now had an opportunity of seeing him take some arrow-root. He sat up in bed to eat it; and before attempting to do so, he made hurried inspirations, and sobbings precisely resembling those which occur when one wades gradually into cold water. He swallowed small quantities of arrow-root eight or nine times, with hurry and difficulty, and with sighs that succeeded each other rapidly. He said that he felt the upper part of his throat narrower than it should be. He continued to take laudanum mixed with sugar and bread into a kind of pulp.

By the evening of that day the disease had not made much further progress. He again sat up and tried to eat some thinnish gruel. While taking the basin into his hand, he drew back his head to a distance from it, apparently involuntarily. He took one half-spoonful with effort and distress, then sighed deeply and rapidly, or rather his breathing consisted of a succession of sighs at short intervals: he gave up the basin, and sank back on his pillow still sighing. In the course of that night he ceased to take the laudanum; he could no longer attempt it. The next day he was still composed, though more easily irritated; and it was found that he had lost the power of moving the left arm. His pulse was 140, and much weaker than before, and his mental powers were failing. He gradually sank, and died in the evening, having repeated the Lord's prayer an hour previously. During the last hours of life he had been moaning, and tossing from side to side: his bowels were purged; fluid stools ran from him, and distressed him greatly. His lower extremities first became cold, and the coldness extended by degrees up to his chest. He hawked up in the course of the day a considerable quantity of ropy mucus, and much frothy saliva came from his mouth towards the close. As his wife was wiping this away, his teeth, whether by convulsive accident or otherwise, came in contact with her finger, and drew blood. The part was cut out; and no bad consequence followed that I know of.

The examination of his body threw no satisfactory light upon the essential nature of the disease. Blood and serous fluid escaped on the removal of the calvarium. The vessels of the membranes were full, and the brain itself was mottled somewhat

by its vascularity. There were a few spots of ecchymosis on the heart. The back part of the tongue was very vascular. The stomach presented the most notable appearance. There was a quantity of brownish-coloured mucus on its inner surface, and the mucous membrane had disappeared from a space about four inches in diameter at its left and larger end. That space alone was diaphanous; its edges sloped inwards; and a segment of this thin place looked exactly like a piece of china. On a white ground, there were inosculating vessels, some of them blue, and some of them of a coffee-coloured brown. I conclude that this appearance was produced by the action of the gastric juice after death.

This was in some respects a remarkable case. It was remarkable for its duration. Dr. Bardsley, in the article on Hydrophobia in the *Cyclopædia of Practical Medicine*, states that the patients "invariably go on from bad to worse, and finally die before the sixth day." Now if we reckon that stage of the complaint here referred to by Dr. Bardsley to have begun on the morning of Friday, when he was obliged to omit his sponging because of the spasm about his throat, this patient did not die till the middle of the seventh day. In fact it was a very protracted case; and the symptoms were less violent than usual. Whether this was owing to the opium he took or not, it would be difficult to determine.

In the second of the cases which it has been my lot to witness, the characteristic symptoms of hydrophobia were more faintly pronounced than is usual.

On my arrival at the Middlesex Hospital, on Thursday, the 5th of October, 1837, I was told that a patient had been admitted (under one of my colleagues) labouring probably under hydrophobia. He had applied at the hospital in the middle of the night; but was then sent away, after receiving some aperient pills, with assurances that he was only feverish and nervous. On his reapplication in the morning he had been admitted.

I found him in the ward: a man twenty-five years old, of dark complexion and hair. He expressed his conviction that he was afflicted with hydrophobia; and said he was prepared for his fate. I observed that every now and then he suddenly sighed in a very peculiar manner; just as I had seen the former patient sigh. This would happen sometimes in the middle of a sentence, while he was speaking. He told us he had been bitten by a dog in the latter end of July; the dog was swimming, and like to drown, in a canal, and upon his reaching over to lift him out of the water, the animal seized upon his hand. After dragging the dog out, he beat him for his ingratitude; and then the dog ran off, and was pursued by a mob of boys, who had previously been pelting him as a mad dog. There was a scar on the middle finger of the right hand; the nail of that finger had (he said) been torn through, and each of the two adjacent fingers had been more slightly bitten. His pulse was 84; but varied in frequency at short intervals.

He acknowledged that after receiving the bite he was uneasy as to its possible effects, and read books about hydrophobia at the time: but he affirmed that he had afterwards ceased entirely to think about it. He had persuaded himself that the dog could not be mad, from its being in the water. On Tuesday, if not earlier, he had been uncomfortable and restless; and on Wednesday he found he could not swallow liquids. On one of these days he experienced a slight pricking sensation, without any redness or tenderness, in the site of the scar; his right arm and leg seemed to himself hotter than the opposite limbs; and the arm, though not tender, felt raw, and he could not bear the light contact of his clothes upon it. He became feverish also. From time to time a slight expression of terror passed across his features, and then he made a sudden, deep, sighing inspiration: at other times his breathing and appearance were perfectly natural. It was said that when some water was brought him he drew himself back from it with horror. He talked a good deal.

I saw him eat rice, made pulpy with milk. He took it without looking at the spoon, from which he averted his eyes, and ate several mouthfuls, in a gulping manner, and with evident effort. His bowels had been purged by the pills, and he declared that the noise of the water in the water-closet had distressed him. The sound of some water poured from one vessel into another by the patient in the next bed, had also agitated him. So did the contact of my cold hand on his arm; and currents of air, even the breath of any one speaking to him; so that he insisted on

conversing with the apothecary in such a position that the chin of each was upon the other's shoulder. But there was no actual or apparent spasm.

At this time he affirmed that the presence of company cheered him, and did him good; and begged that he might not be removed into a separate room. And he wished for some amusing book that he might read.

In the evening I again went to see him. He did not seem worse, though he said "his symptoms were increasing." He had taken a dose of musk and some morphia.

The next day I found the hospital in some confusion. Between eleven and twelve o'clock in the preceding night some of the officers of the hospital had gone to his bed, while he was apparently asleep, and certainly very quiet. They asked him if he would like some water. This seems to have greatly excited him; and immediately after their departure he rushed out of bed, (terrified, he said,) became furious and unmanageable, and was never again tranquil till he died, about the same time the next night. He was now put into a room by himself; and, taking advantage of the momentary absence of the nurse, he bolted himself in alone; and declared he would admit no person but her. The door was at length forced, and a strait-waistcoat was put upon him. He then became quieter in his manner; begged that no unnecessary violence might be used; asked to be poisoned; spat at some of the bystanders, and reproached them, talking rapidly and wildly like an insane person; yet loudly and angrily imposing silence on every one who addressed him. He said he could not bear to hear any one speak; that he did not like my bass voice. Then he would sneer at the students, and say they showed bravery enough now he was confined: "was it right for young gentlemen of education to stand there gazing with curiosity on a dying man?" asked for bread soaked in water, and when it was held towards him, snatched it with his mouth in a savage manner; spoke of his "poisoned tooth," and talked perpetually. He took a fancy to one of the students, and begged that he might remain with him.

About this time he vomited some yellow fluid, and thought he felt the better for it, and asked for an emetic; and some tartarized antimony was given him. He was now pale, and his lips were livid; but none of the distinctive spasmodic attacks occurred: indeed, water was not at this time suffered to be brought near him. This circumstance it was, this absence of the peculiar spasmodic paroxysms which characterize hydrophobia, that induced several medical men, of much sagacity and experience, to doubt, and even with some positiveness to deny, that the patient was suffering under that disease at all. They supposed him to be hysterical, half-crazy, or on the brink of delirium tremens. But though slightly expressed, the symptoms were unlike anything I had ever witnessed, except in the previous instance. And the closing scene was quite distinctive.

It appeared, and he spoke of it as a thing which distressed him, that when he was most excited, his urine passed involuntarily.

In the evening I found his father with him. He had recognised him, and kissed his mother-in-law; but soon began to rave, and to be apparently occupied with absent persons. He was pale and weak, and lay with his head over the side of the bed, spitting continually upon the floor, which was thus made quite wet. He wished to have his hands at liberty that he might "clear his mouth." He was soliloquizing when I went into the room, in this way: "Monsters—monsters—see that monster Susan—take her away." (It appeared that he was now speaking of a young woman who had had a child by him.) "I thought they would do much for science, but never supposed they would inflict such agony as this;" and so on.

A little later Mr. Arnott visited him. He had then no pulse at the wrist. The waistcoat was removed. He sat up, and used some water, brought to wash his hands, without apparent distress. Soon after, he sank back exhausted; and expired.

His father corroborated what the patient had said of the dog; and told us his son was clever, and better educated than many of his rank (he was a tailor), but always exceedingly nervous.

The body was examined the next day. Its posterior and undermost surface was very livid. The blood everywhere quite fluid. The veins of the spinal cord, on its posterior part, were turgid; not at all so on the anterior. The substance of the cord was quite natural. There was some fluid in the theca. The brain appeared to me, in every part, quite sound and healthy.

The head and face, which had been hanging over the table while the spinal canal was opened from behind, were deeply purple, as though universally bruised. This colour diminished rapidly after the corpse was placed supine, and the head raised somewhat above the level of the body. The papillæ at the back part of the tongue were greatly exaggerated, and looked like large vesicles. The cartilage of the epiglottis, at its lower part, was red. At about the middle portion of the œsophagus there was an appearance as if the cuticle had been abraded. The mucous membrane of the stomach was soft, and red here and there, with a dotted injection resembling ecchymosis, especially on its rugæ. The air-passages were apparently healthy.

On the first day of the year, 1855, a lady, 32 years old, residing at a short distance from London, was bitten on the ulnar side of the middle finger of her right hand, in the furrow between the skin and the nail, by a white cat, belonging to the stables of the house. The young lady's brother had seen the cat quarrelling with a terrier the day before; and afterwards struggling and fighting with another cat. Supposing that the animal might be ill, or hurt, the lady desired to have it brought to her, and placed it on her lap, when it bit her.

The cat was killed the same day, but not before it had scratched the gardener's child, flown furiously at another man, and seized and bitten a whip with which the coachman had attacked it.

On Wednesday, the 14th of March, Miss L. began to feel generally unwell. On the 16th, pain ran from the bitten finger along the ulnar aspect of the right arm into the axilla, and across the chest about the level of the fifth rib. No redness nor swelling was visible about the scar, but she spoke of a sensation there as if the skin were *hin*. This pain did not last long, nor did it recur.

On Saturday, the 17th, she found a difficulty in swallowing medicine. Dr. Todd visited her in the evening, with Dr. Garrett, and prescribed enemata, containing each ten drops of laudanum. Some puffiness in the right axilla was noticed, but it soon disappeared.

On Sunday morning I met those physicians in consultation. I found Miss L. in bed, with a wildish expression about her eyes. Her tongue was dry and furred; her pulse eighty, soft, with occasional accelerations for a few beats only. There was a slight systolic murmur of the heart, which was beating noisily. Her bowels had acted during the night. She had twice passed urine into the bed, and she said that she had done so inevitably, and that "it showed how weak she was." The debility was indeed very great, as we perceived upon raising and sustaining her in a sitting position.

A morsel of ice was given to her. She put it hastily into her mouth, then drew back her head, and stretched out her arm with a repelling gesture, and sighed many times; but she failed to swallow the ice. Afterwards she succeeded better with some tea, which she took in spoonfuls, yet with a strange hurry, and with sighing gasps, and a rolling upwards of the eyes.

It was remarkable that this patient was not agitated, nor apparently distressed, by the sound of liquids poured from one vessel into another, nor by the access of light upon the sudden withdrawing of the window-blinds, nor by currents of air, for she even bore to be fanned.

I conclude that she knew, or at least suspected, what was the matter with her, for she said that to drink some tea would be a *test*.

She then, without much difficulty, ate a boiled egg; and under encouragement, and our expression of hopes that she was better, and exhortations to be careful and not to hurry, she swallowed, with seeming ease, a glass of wine in successive tea-spoonfuls—until the last spoonful, from which she recoiled with a look of terror, exclaiming, despondently, "It is no better."

We were to visit her again together the next day, though I had my misgivings as to her surviving the night. I learned from Dr. Garrett that she swallowed more wine in the afternoon, and fancied it did her harm. She had then paroxysms of sighing respiration, with intervals of comparative calm. At seven in the evening Dr. Garrett noticed that she began to eject, in a gulping manner, saliva and viscid mucus from her mouth. This increased in quantity, and was attended with a sort of chewing, or champing. At ten o'clock she desired that the servants should, one by one, be

brought to her bedside, and exhorted them severally to the observance of their religious duties. She expired at a quarter past seven o'clock on the Monday morning, her mind having continued clear to the end.

The fourth case to which I have alluded was, in my judgment, a genuine instance of hydrophobia, although some, who witnessed it, doubted. I am indebted for the opportunity of seeing it to the kindness of Dr. Sibson, under whose care the patient, a female child five years of age, was lying in St. Mary's Hospital.

She had been bitten by a spaniel on the 3d of May, in the right leg, just above the calf, where the scars of the injury were visible. The dog was killed the same afternoon. The child began to be unwell on the 4th of June. The symptoms reported were shudderings whenever a stream of wind passed over her, and spasms, and gasping dysphagia upon her attempting to drink. I saw her on the 9th of that month. Her countenance was tranquil, but now and then assumed a sort of idiotic smile. When a current of air was directed upon her by blowing, she made a sighing start, cried for a few seconds, and was again quiet. She took from a spoon some wine and beef-tea readily, and seemed to like them, for she opened her mouth always as she saw the spoon approach her lips. When the cup was offered her, she clutched it with her hands, and drank a little, with obvious distress. A mirror waved before her face did not appear to affect her. She had dozed a little in the night. Occasionally she closed her eyes, which could then be seen to quiver or vibrate beneath the shut lids. I thought she squinted slightly. She did not utter any articulate words, but she had talked a good deal the day before. She was very weak; unable to stand. Though I observed no mucus about her mouth, she was said to have spat out viscid frothy saliva, and frequently to have grinded her teeth. She particularly disliked to have her abdomen touched, or uncovered.

On repeating my visit to the hospital later in the day I found her weaker. She had voided urine into the bed, and had vomited several times. She made frequent little backward jerks of the head—which was held back rather rigidly by the muscles of the neck, while her shoulders were firmly hunched up. Swallowing could still be effected, but with increased difficulty. She was less sensible to currents of air. Her pulse was very rapid. Afterwards sickness and hiccup came on, and she died early the next morning.

A careful examination of the dead body disclosed nothing remarkable, or illustrative of the disorder. The symptoms had been attributed to worms; but the intestines were slit up through their whole length, and no worms were found.

Generally, the disease, when it has once set in, and shown the peculiar hydrophobic symptoms, runs a short and fierce course. The nervous irritability becomes extreme. The peculiar paroxysms of choking spasm, and sobbing, are excited, not only by attempts to swallow liquids, but by the very sight or sound of them. Dr. Elliotson mentions a boy who was thrown into a state of violent agitation by hearing a dresser who sat up with him make water. The passage of a gust of wind across his face, the waving of a polished surface, as of a mirror, before his eyes, the crawling of an insect over his skin, is often sufficient to excite great irritation, and the peculiar strangling sensation about the fauces, in a hydrophobic patient. These circumstances were but little observable in the patients whose cases I have related. The first of them indeed was remarkably calm and tranquil under the disease. In general the patient is dreadfully irritable, and apprehensive, and suspicious; and in most cases there is a degree of mania or delirium mixed up with the irritability; the sufferer is very garrulous and excited. In this respect there is a marked difference between hydrophobia and tetanus. In the latter disorder the mental faculties are clear, and the patients serene, and what is called heart-whole, to the last. The two diseases differ in another striking particular: the spasm in the one case is tonic, in the other clonic. In tetanus, again, there is no thirst, and seldom any accumulation of tough and stringy mucus in the fauces and about the angles of the mouth: in hydrophobia both these symptoms are always, I believe, present. So probably is vomiting; but vomiting in tetanus is rare. The nervous irritability in hydrophobia is doubtless a part of the disease, and is very seldom absent even now-a-days. Some time ago it might perhaps have been plausibly attributed to the treatment adopted. I allude to that period in which it was believed that these miserable persons had both the power,

and the inclination, to impart the disease to others by biting them; and when, under pretence of shortening *his* sufferings, but really, I am afraid, with the cowardly view of protecting *themselves*, his friends were accustomed to smother the unhappy patient between two feather-beds, or to open a vein, and to leave him to bleed to death. Any person suspecting what was the matter, and foreseeing such a termination to his disease, might well be nervous and irritable. But now that this barbarous practice has been exploded, and the dread of being smothered does not occur to the mind of the patient, he is still found to be exquisitely irritable and timorous. The foam and sticky mucus that gather in the throat and mouth, these patients make great efforts, by spitting and blowing, to get rid of; and the sounds they thus produce have been exaggerated by ignorance and credulity into the barking and foaming of a dog. In the same way the paraplegia which sometimes takes place, rendering the patient unable to stand upright, has been misconstrued into a desire on his part to go on all fours like a dog. The pulse, though it may be strong and hard at the outset, becomes, in a short time, frequent and feeble, and the general strength declines with great rapidity. Death occasionally takes place within twenty-four hours after the commencement of the specific symptoms. Most commonly of all it happens on the second or third day; now and then it is postponed to the fifth day; and in still rarer instances, of which my first case was one, death does not occur till the seventh, or eighth, or ninth day. In most cases the paroxysms, becoming more violent and frequent, exhaust the patient; but occasionally the symptoms undergo a marked alteration before death. The paroxysms cease, the nervous irritability disappears, the patient is able to eat and drink and converse with ease; those sights and sounds which so annoyed and distressed him before, no longer cause him any disquiet. In this state he often sinks into a sleep, and suddenly wakes from it to die: sometimes his existence is put an end to by a sudden and violent convulsion.

It is needless for me to go into a minute account of the morbid appearances that have been met with in persons dead of hydrophobia. They are various, uncertain, unsatisfactory. Decomposition of the tissues, and of the blood, is said to take place rapidly. In some bodies the most careful examination has discovered nothing amiss. In others, vascularity of the brain, or of the spinal cord, has been noticed. And in not a few instances the mucous membrane of the fauces, œsophagus, and stomach—or of the larynx and trachea—or of both these tracts—has been found red, and covered with adhesive mucus. But we must take care not to attribute undue importance to these last appearances—not to conclude that they have been the cause of the symptoms, when, in truth, they may have been the effect of the disease. That we should find the parts in the throat red and congested is what we might naturally expect, when we consider the violent straining spasmodic action of these parts for some time before death. The morbid anatomy of this disease throws but little light upon its nature, or upon its proper treatment.

Many interesting questions present themselves relative to hydrophobia. I will state the principal of these as shortly as I can.

1. You will be surprised when I tell you that some persons have made it a question whether there is any such disease at all. I have known such. The late Sir Isaac Pennington, who was Regius Professor of Physic at Cambridge, had never seen a case of hydrophobia, and nothing could persuade him that any one else had seen anything more than a nervous complaint produced by the alarmed imagination of the patient, who, having been bitten by a dog reputed to be mad, and having the fear of feather-beds before his eyes, was frightened into a belief that he had hydrophobia, and ultimately scared out of his very existence. Now if you meet with such incredulous persons, and think it worth your while to argue the point with them, you may object to their unbelief, the improbability that so many persons who have been bitten by mad dogs should have suffered so precisely the same train of symptoms, and at last have died, from the mere force of a morbid imagination. You may urge them with the fact that many of these persons have been under no apprehension at all until the disease has seized upon them; that many also have been men of naturally strong and firm minds, not at all likely to be frightened into believing that they were seriously ill unless they really were so, and still less likely to be terrified into their graves. And if this have no weight with such reasoners, you may bring forward the conclusive facts that the disease has befallen infants, and idiots, who had never heard

or understood a word about mad dogs or hydrophobia, and in whom the imagination could have had no power in calling forth the complaint. And if they are proof against this, you must give them up: I can suggest nothing more.

2. Allowing that the disease exists as a real, and not merely imaginary disease, and also that it is caused by the bite of a rabid animal: this important question arises—has it any *other* cause?

Sitting aside that quibbling application of the term hydrophobia, which some writers have chosen to make, to diseases in which, from some painful affection of the throat, the patients have been unwilling to attempt to swallow fluids, there are cases recorded, exactly resembling hydrophobia in their symptoms, and occurring in persons who were never known to have been bitten by, or even to have been in the presence of, a rabid animal. The celebrated and accurate Pinel has given the history of such a case. There is another by Savitrotte, in the *Journal des Savans* (August 1757). Now it is just possible that this disease may sometimes develop itself in the human body without any contagion having been applied: and it is also possible, and much more probable, in my judgment, that the poison may have been applied without the person's being aware of it. We shall see, by and by, some very possible ways in which that might happen. All that we need concern ourselves with practically, is this—that in 999 cases out of 1000 the disease in the human body is derived from a rabid animal. If it ever be spontaneous, we cannot reckon upon meeting with such a case: indeed, many medical men pass through life without witnessing the disorder at all.

[Many well authenticated cases are on record, in which disease having all the pathognomonic symptoms of hydrophobia, occurred without the slightest evidence of its being the result of the bite or scratch of a rabid animal, or its having been produced by a virus accidentally introduced into the system from any possible source. We met ourselves, a few years ago, with precisely such a case, in which the most cautious investigation convinced us, that the disease had been produced independently of any specific contagion from without. — C.]

3. Granting, then, that the disease in man is the result of an animal poison, the next question is, from what animals may he receive the infection?

We are sure that the disease, by the inoculation of which hydrophobia may be produced in man, is common in the *dog*, and that it has been communicated to the human animal by the fox also, the wolf, the jackal, the raccoon, and the cat. Mr. Youatt says that the saliva of the badger, the horse, the human being, have undoubtedly produced rabies, and some affirm that it has been propagated even by the hen and the duck. The same author mentions a case in which a groom became affected with hydrophobia through a scratch which he received from the tooth of a horse that was labouring under the disease. All animals, even fowls, are susceptible of the disorder when bitten by the rabid dog. Of course it is an important question to have resolved, whether the saliva of all these is capable of conveying the malady. The case just now mentioned on Mr. Youatt's authority would seem to settle the question as respects the horse; but as horses, cows, turkeys, &c., do not generally bite, we have not many opportunities of supplying a positive answer to the general question: there can be no doubt about the *cat*, the *fox*, the *wolf*, and the *jackal*.

The late Duke of Richmond died in Canada of hydrophobia, communicated, it was thought, by a tame fox. In the 13th volume of the *Medico-Chirurgical Transactions*, an account is given by Mr. Hewitt, of several cases of fatal hydrophobia from the bite of a wild and rabid jackal. Many examples are on record of the production of the disease by the bites of mad cats and wolves.

The first case which I have spoken of, as having been seen by myself, would seem to prove, if all the facts were correctly stated at the time, that the saliva of the dog may be sufficient to produce the disease, when it is merely applied to the unbroken skin. It was affirmed by various persons that the teeth of the terrier did not break the cuticle. But we must take care not to draw a hasty general inference from a single case. The late Mr. Youatt, who had seen more of the disease probably both in man and in other animals, than any other person in this country, did not think that the saliva of a rabid animal could communicate the disorder through the un-

broken cuticle: he believed that there must be some abrasion or breach of surface. He held, however, that it might be communicated by mere contact with the mucous membranes.

Of its harmlessness on the sound integument, he offered this presumption — that his own hands had many times, with perfect impunity, been covered with the saliva of the mad dog. He records some singular instances in which the disease was transmitted by contact of the saliva with the mucous membranes. "A man endeavoured to untie with his teeth a knot that had been firmly drawn in a cord. Eight weeks afterwards he expired, undeniably rabid. It was then recollected that with this cord a mad dog had been confined. A woman was attacked by a rabid dog, and escaped with the laceration of her gown. In the act of mending it she thoughtlessly pressed down the seam with her teeth. She died." If these cases be authentic, they are conclusive of this question; unless, indeed, the lips of those who perished happened to have been chapped or abraded. But Mr. Youatt's own opinion was that the virus could not be received on a mucous surface without imminent danger.

The disease is said to have been caused by the *scratch* of a cat. But as we know that cats as well as dogs frequently apply their paws to their mouths, especially when the latter part is uneasy, (as it clearly is in mad dogs,) this fact of the production of the disease by a scratch, if thoroughly made out, would not *prove* that the disease can be introduced into the system in any other way than by means of the saliva.

LECTURE XXXIV.

Hydrophobia, concluded. Various Questions considered respecting the Disease as it appears in the Human Subject, and respecting Rabies in the Dog. Pathology of the Disorder. Treatment. Preventive Measures.

AFTER giving you some account of the phenomena of *hydrophobia*, or *rabies canina*, I began to notice, in the last lecture, the chief of the interesting questions which naturally present themselves to the minds of most men, and especially of medical men, in respect to that shocking disorder.

In the first place, there is such a disorder. It appears, too, secondly, from statements made upon credible authority, that the same group and succession of symptoms as characterize the disease when it is produced by the bite of a rabid animal, have been observed to occur in persons who were never known to have been bitten. In my own opinion it is more probable that these persons had been exposed to the virus without being aware of it, than that the disease was spontaneously engendered in their bodies. I would make the same remark with regard to an instance which is said to have happened of hydrophobia in a lad who had been bitten five weeks before by a *healthy* dog: the dog remaining well at the time of his seizure and death. Mr. Youatt holds, indeed, that however the disease originated, it never occurs now, not even in the dog, except as a consequence of the application of the specific contagion. It is certain, in the third place, that (besides the dog) the wolf, the fox, the jackal, and the cat, have communicated the disorder to the human animal. The death from hydrophobia of a boy after being bitten by a raccoon, is recorded by Dr. Russel, of Lincoln, Massachusetts, in a report contained in the Transactions of the American Medical Association for 1856. Mr. Youatt affirms, in his pamphlet on this disorder, that the saliva of the badger, of the horse, and of the human being, has caused rabies; and I mentioned, on his authority, a case in which a groom contracted the disease through a scratch which he received while administering a ball to a rabid horse. But I feel much less certain about these latter animals. Respecting the dog, the fox, the wolf, the jackal, the cat, there can be no question. The result of certain experiments made at the Veterinary School, at Alfort, is opposed to Mr. Youatt's statement. Professor Dupuy made wounds in cows and sheep, and rubbed upon these wounds sponges

which had been chewed by rabid animals of the same species : yet he never succeeded in communicating the disorder in this way ; but when he used a sponge that had been mumbled by a mad dog, then the disease occurred in the sheep and cows.

It is still more interesting to inquire, whether the saliva of a human being, labouring under hydrophobia, be capable of inoculating another human being with the same complaint? Mr. Youatt says, yes : that the disease has undoubtedly been so produced. If this be so, the fact will teach us — not to desert or neglect these unhappy patients, still less to murder them by smothering—but to minister to their wants with certain precautions : so as not to suffer their saliva to come in contact with any sore or abraded surface ; nor, if it can be avoided, with any mucous surface. On the other hand, all carefulness of that kind will be unnecessary, if the disease cannot be propagated by the human saliva. Certainly, many experimenters have tried in vain to inoculate dogs with the spittle of a hydrophobic man ; but there is one authentic experiment on record, which makes it too probable that the disease, though it may not be communicated often, or easily, is yet communicable. The experiment is said to have been made by MM. Magendie and Breschet, at the Hôtel-Dieu, and to have been witnessed by a great number of medical men and students. Two healthy dogs were inoculated, on the 19th of June, 1813, with the saliva of a patient, named Surlu, who died of hydrophobia the same day in that hospital. One of these dogs became mad on the 27th of the following month. They caused this dog to bite others, which, in their turn, became rabid also : and in this way they propagated the malady, among dogs, during the whole summer. Now this is a very striking fact, yet it ought not to be considered conclusive : for it is possible that the dog might have gone mad at that time, whether he had been so inoculated, or not. It may have been a mere coincidence. We want repetitions of such experiments to *settle* the point : nevertheless, we have enough in this one experiment to make us use all necessary caution when engaged in attending upon a hydrophobic patient.

I just touched upon the question, whether the saliva of a rabid dog could produce the disease if it fell upon the *sound skin*? The first of the cases which I related as having been witnessed by myself, would appear to give an affirmative answer to this question. Mr. Youatt thinks the disease would *not* follow such an application of the virus ; but that it cannot be received upon even the unbroken surface of a mucous membrane without the greatest danger. Horses are said to have died mad, after eating straw upon which rabid pigs had died. Portal was assured that two dogs, which had licked the mouth of another dog that was rabid, were attacked with rabies seven or eight days afterwards. Mr. Gilman, of Highgate, in a little pamphlet on Hydrophobia, quotes an instance from Dr. Percival, in which a mad dog licked the face of a sleeping man, near his mouth, and the man died of hydrophobia, although the strictest search failed to discover the smallest scratch or abrasion on any part of his skin.

At the very close of the lecture I observed, that even should it be clearly proved that hydrophobia has ever resulted from the scratch of a rabid animal's *claws*—the claws of a cat, for example—we are not to set it down as a sure thing that the disease can be introduced into the system independently of the saliva of the diseased animal. As we know that dogs and cats are in the habit of putting their paws to their mouths when they feel uneasy there, we may readily understand how the poisonous saliva may be introduced by a mere scratch with the creature's nails. Mr. Youatt believes that the saliva *only* is capable of conveying the disease.

4. Supposing the virus to have been inserted into the part bitten, what becomes of it? Is it immediately taken into the system, and does it, like the poison of small-pox, in some mysterious way, multiply and diffuse or mature itself in the body, until the disease explodes? Or does it remain imprisoned in the wound, or in the cicatrix, for a time? This is an important practical question. For if the poison lurk for some weeks in the place where it was originally deposited, we might successfully remove it at any time between the infliction of the bite and the period of recrudescence. Now the facts, that at this period of recrudescence the wound or scar is re-inflamed often, and almost always becomes the seat of some fresh morbid phenomena, pain, swelling, numbness, and the like, spreading towards the trunk—and that, *soon after this*, the peculiar paroxysmal symptoms begin—these facts are strong in favour of the belief that the poison does lie inert in the place of the original hurt, for some time. Dr.

Bardsley states that the recrudescent pains seem always to follow the course of the nerves, and do certainly never inflame or irritate the lymphatic glands in the vicinity, though passing in a parallel course towards the trunk. He affirms the entire absence of any fact contrary to this observation in the works of the numerous authors who have written on the subject. I mention this statement, because it certainly is not correct. Mr. Mayo says, "in one case which I witnessed and examined after death, the inner part of the cicatrix was bloodshot; and a gland in the axilla had swelled at the coming on of the hydrophobic symptoms." And I find among my notes of Mr. Abernethy's lectures, another striking case, still more to the point. "A very intelligent boy had been bitten by a dog in the finger: he was brought into St. Bartholomew's Hospital. Caustic had been liberally used, affecting the sinewy parts, and producing a terrible sore: yet the boy was recovering himself, and the sore was healing. One day, as Mr. Abernethy was going round the hospital, he saw and spoke to the boy, who said he thought himself getting well, but that he had that day an odd sensation in his fingers, stretching upwards into his hand and arm. Going up the arm, Mr. Abernethy saw two red lines, like inflamed absorbents: they doubtless were so. He affected to make light of the matter, ordered a poultice, and recommended the boy to take some medicine. Early the next morning Mr. Abernethy visited the ward, pretending he had some other patient there, whom he wished particularly to see: and, when going out again, he asked the boy, in a careless tone, how he was. He said that he had lost the pain, but that he was very unwell, and had not slept all night. Mr. Abernethy felt his pulse, told him he was a little feverish, as might be expected, and asked him if he were not thirsty, and would like some toast and water. The boy said he *was* thirsty, and that he *should* like some drink: when, however, the cup was brought, he pushed it from him; he could not drink. In forty-eight hours he was dead."

Facts such as these would lead to the conclusion that, in cases in which excision had not been performed in the first instance, the scar, or the sore, might be cut out with propriety at any time before the period of recrudescence: and if the case happened to be my own, I would have this done even *at* that period, the moment any new sensation manifested itself in the seat of the injury. Mr. Mayo on the same grounds, advocates the removal of the cicatrix, even although the hydrophobic symptoms may have appeared. I do not mean to say that the facts, now referred to, show with any certainty that the poison remains in the place where it was first deposited until the phenomena of recrudescence take place: but they afford some presumption in favour of that notion: and in such a disease as hydrophobia, we are bound to act upon the very lowest presumption that affords a chance for our patient's life. The poison may be absorbed into the general system at the period of recrudescence, although no affection of the absorbing vessels or glands should be manifest: through the veins, namely.

Poisons that find entrance into the blood do not remain inoperative there for an indefinite time, and then begin to manifest their poisonous influence. They lodge in this organ or in that, and presently disturb its functions: or they are, more or less rapidly, eliminated from the body through one or more of its natural emunctories; or they produce specific and constant results after periods of incubation, which are also definite and constant—as we see in cases of small-pox and measles. But there is no instance known that I am aware of (unless indeed it be so in this disease of hydrophobia) in which a poison circulates in the blood for an indefinite and long period, to give rise at last to symptoms that are strictly specific. Looking at the matter theoretically or practically, I should recommend, under the circumstances already stated, the excision of the cicatrix. The poison of hydrophobia may be detained in temporary and precarious union with some one of the animal tissues, just as I shall hereafter show you the poison of lead may be.¹

5. Another important question is this. Is a man who has been bitten by a mad

¹ The following curious statement, bearing upon this obscure but interesting question, I find in the *Monthly Journal of Medical Science* for November, 1853, quoted by Mr. Grove from the *Medico-Chirurgical Review*:—"A girl, aged 14 years, was seized with influenza. She complained of pain in each arm at the spots where, when an infant, she had been vaccinated; and, in fact, in these localities vaccine vesicles now became perfectly developed. An elder sister was re-vaccinated with the lymph hence obtained; beautiful vesicles formed, and ran a normal course."

dog, and in whose case no precautions have been taken, a doomed man? will he be sure to have the disease, and therefore to die of it? By no means. But few, upon the whole, of those who are so bitten, become affected with hydrophobia.

It is curious that different species of animals appear to be susceptible of hydrophobia in different degrees. Thus, according to Mr. Youatt, two dogs out of three, bitten by one that is rabid, become rabid. The majority of horses inoculated with the virus, perish. Cattle have a better chance: perhaps because in them the skin is looser and less easily penetrated. A full half (he thinks) of those that were seized by a mad dog, would escape. With sheep the bite is still less dangerous. He reckons that not more than one in three would be affected. The tooth, perhaps, has been wiped clean in its passage through the wool. The human being is least of all in danger. John Hunter states that he knew an instance in which, of twenty-one persons bitten, one alone fell a victim to hydrophobia. Dr. Hamilton estimates the proportion to be one in twenty-five. But I fear these computations are much too low. In 1780, a mad dog, in the neighbourhood of Senlis, took his course within a small circle, and bit fifteen persons before he was killed; three of these died of hydrophobia. The saliva of a rabid wolf would seem to be highly virulent and effective. These beasts fly always, I believe, at a naked part. Hence, probably, the fatality of their bites. The following statement applies exclusively to the wolf. In December, 1774, twenty persons were bitten in the neighbourhood of Troyes; nine of them died. Of seventeen persons similarly bitten in 1784, near Brive, ten died rabid. In May, 1817, twenty-three persons were bitten, and fourteen perished. Four died out of eleven that were bitten near Dijon: and eighteen of twenty-four bitten near Rochelle. At Bar-sur-Ornain, nineteen were bitten, of whom twelve died of hydrophobia within two months. Here we have one hundred and fourteen persons bitten by rabid wolves, and among them no less than sixty-seven victims; considerably more than one-half. There is no doubt, however, that the majority of persons who are bitten by a mad dog escape the disease. This may partly be owing to an inherent inaptitude for accepting it. We see some persons who, though often in the way of it, do not contract syphilis: there are others upon whom the contagion of small-pox has no influence. This difference exists, apparently, even among dogs. There was one dog, at Charenton, that did not become rabid after being bitten by a rabid dog; and it was so managed that, at different times, he was bitten by thirty different mad dogs; but he outlived it all. Much will depend also upon the circumstances and manner in which the bite is inflicted. If it be made through clothes, and especially through thick woollen garments, or through leather, the saliva may be wiped clean away from the tooth before it reaches the flesh. In the fifth volume of the *Edinburgh Medical and Surgical Journal*, there is a case described by Mr. Oldknow, of Nottingham, in which a man was bitten in three different places by the same dog, viz.—in the scrotum, the thigh, and the left hand; the bite on the hand was the last. Now it seems not improbable that, but for this last bite, on a naked part, he might have escaped. At least it was a remarkable circumstance that the phenomena of recrudescence occurred only in the hand and arm. The dog is supposed to have closed his mouth after inflicting the first two bites, and thus to have charged his teeth afresh with the poisonous saliva. In the American report, to which I just now referred, it is stated, that of seventy-five cases, the injury was inflicted on the hand in forty instances, on the face in fifteen, on the leg in eleven, on the arm in nine.

It is this frequent immunity from the disease in persons who have been bitten, that has tended to confer reputation upon so many vaunted methods of prevention. Ignorant persons, and knavish persons, have not failed to take advantage of this. They announce that they are in possession of some secret remedy which will prevent the virus from operating: they persuade the friends of those who die that the remedy was not rightly employed, or not resorted to sufficiently early: and they persuade those who escape that they escape by virtue of the preventive remedy. If the plunder they reap from the foolish and the frightened were all, this would be of less consequence; but unfortunately the hope of security without undergoing a painful operation leads many to neglect the only sure mode of obtaining safety.

Mr. Youatt is of opinion that the power of the virus ceases with the life of the animal. He states, that in many dissections of the dog, the saliva, in spite of all care, must have come in abundant contact with his hands, and they were not always sound.

I should strongly recommend you not to act upon this opinion : but to use the same precautions, in dissecting a rabid animal, as you would use if you were persuaded that the disease might be communicated with equal certainty before and after the death of the animal.

6. A still more anxious inquiry next arises. Whoever has been bitten by a rabid, or a suspected animal, must be considered, and will generally consider himself, as being in more or less danger of hydrophobia. This dread is not entirely removed, even by the adoption of the best means of prevention. Now, how long does this state of hazard continue? When is the peril fairly over? After what period may the person who has received the injury lay aside all apprehension of the disease? To this inquiry no satisfactory reply can be given. In a vast majority of instances, indeed, the disorder has broken out *within two months* from the infliction of the bite. But the exceptions to this rule are too numerous to permit us to put firm trust in the immunity afforded by that interval. Cases are recorded in which five, six, eleven, nineteen months, have intervened, between the insertion of the poison and the eruption of the consequent malady. Nay, in one instance, three years are said to have elapsed, and in another the enormous period of twelve years. In these cases one cannot help supposing that some unsuspected re-inoculation, some fresh application of the peculiar virus, may have taken place. If not, then we must conclude that the poison really lies imprisoned in the part; and only becomes destructive when, under certain obscure conditions, and at indefinite periods, it is set afloat in the circulating blood.¹

It is interesting to know that the same uncertainty of access has been noticed among infected *dogs*. On the night of the 8th of June, 1791, the man in charge of Lord Fitzwilliam's kennel was much disturbed by fightings among the hounds; and got up several times to quiet them. On each occasion he found the same dog quarrelling; at last, therefore, he shut that dog up by himself, and then there was no further disturbance. On the third day afterwards, the quarrelsome hound became unequivocally rabid; and on the fifth day he died. The whole pack were thereupon separately confined, and watched. Six of the dogs became subsequently mad; and at the following widely different intervals from the 8th of June, viz. — 23 days, 56, 67, 88, 155, and 183 days.

There are some considerations respecting this disease, which relate to both the biter and the bitten; the canine and the human being. And there are some which relate exclusively to the dog, yet concerning which we, as medical philosophers, ought not to be ignorant. I shall advert to a few of these.

One question I have already glanced at; viz., whether the disease may be produced by a healthy, though angry dog or cat. I referred to one instance in which this was supposed to have been the case; and I repeat that I should be more inclined to think, unless we had other examples of the same kind, that the person had been inoculated in some way that he was not aware of. But I have heard Mr. Youatt describe cases in which there had been no symptoms of rabies observed in the dog at the time the injury was inflicted, though soon afterwards the animal became decidedly rabid. It is much to be regretted that the dog is so often destroyed. When a person has been bitten by a dog or cat suspected to be rabid, the beast ought by no means to be killed, but to be secured, and kept under surveillance, and suffered, if it shall so happen, to die of the disease. If he do not die, in other words, if he be really not rabid, that will soon appear; and the mind of the patient will then be relieved from a very painful state of suspense and uncertainty, which might otherwise have haunted him for months or years. Should the dog die mad, the injured person will be no worse off than if the animal had been killed in the first instance: nay, in one respect he will be better off, inasmuch as certainty of evil is preferable to perpetual and uneasy doubt. "Give a dog a bad name (says the proverb), and hang him!" and it is literally so with the imputation of madness. A wretched dog is perhaps ill, or weary, or cross, or he may have been worried already by mischievous boys: the cry of mad dog is raised; and then he can expect no mercy. There are gross errors prevalent with regard to the signs of madness in the dog. If a dog be seen in a fit in the street, some person charitably offers a conjecture that perhaps he may be mad; the

¹ Romberg states that an analysis of 60 authentic observations has shown that the shortest period is 15 days, the longest from 7 to 9 months; and that the average period is from 4 to 7 weeks.—*Diseases of the Nervous System*. Sydenham Society's Translation. Vol. ii. p. 144.

next person has no doubt of it; and then, woe to that dog! But Mr. Youatt assures us that the rabid dog never has fits: that the existence of epilepsy is a clear proof that there is no rabies. Again, it is a very common belief that a rabid dog, like a hydrophobic man, will shun water; and if he take to a river, that is thought to be conclusive evidence against his being mad. But the truth is, that the disease, in the quadruped, cannot be called *hydrophobia*: there is no dread of water, but an unquenchable thirst; no spasm attending the effort to swallow, but sometimes in dogs an inability to swallow, from paralysis of the muscles about the jaws and throat. They will stand lap, lapping, without getting any of the liquid down. They fly eagerly to the water; and Mr. Youatt states that all other quadrupeds, with perhaps an occasional exception in the horse, drink with ease, and with increased avidity. This erroneous impression is not confined to the vulgar. In the case which I have more than once alluded to, and which is mentioned in Hufeland's Journal, of a lad who died of hydrophobia after having been bitten by a dog that had not been and was not then mad, one circumstance stated in evidence of the animal's freedom from rabies is, that he drank without difficulty a large quantity of water.

There is another superstitious opinion not at all uncommon, viz.—that healthy dogs recognise one that is mad, and fear him, and run away from his presence, in obedience to some mysterious and wonderful instinct, warning them of danger. This is quite unfounded. Equally mistaken are the notions that the mad dog exhales a peculiar and offensive smell, and that he may be known by his running with his tail between his legs; except, as Mr. Youatt says, when, weary and exhausted, he is seeking his home.

It will not be out of place to state what *are* the symptoms of rabies as observed in the dog, and as described by Mr. Youatt.

The earliest symptoms of madness in the dog (he says), are sullenness, fidgettiness, continual shifting of posture, a steadfast gaze expressive of suspicion, an earnest licking of some part, on which a scar may generally be found. If the ear be the affected part, the dog is incessantly and violently scratching it. If it be the foot, he gnaws it till the integuments are destroyed.

Occasional vomiting and a depraved appetite are very early noticeable. The dog will pick up and swallow bits of thread or silk from the carpet, hair, straw, even dung: and frequently he will lap his own urine, and devour his own excrement. Then the animal becomes irascible; flies fiercely at strangers; is impatient of correction; seizes the whip or stick; quarrels with his own companions; eagerly hunts and worries cats; demolishes his bed; and if chained up, makes violent efforts to escape, tearing his kennel to pieces with his teeth. If he be at large he usually attacks only those dogs that come in his way; but if he be naturally ferocious he will diligently and perseveringly seek his enemy. According to Mr. Youatt, the disease is principally propagated by the fighting dog in towns; and by the cur or lurcher in the country: by those dogs, therefore, which minister to the vices of the lower classes in town and country respectively. He maintains that if a well-enforced quarantine could be established, and every dog in the kingdom confined separately for seven months, the disease might be extirpated. This opinion is founded of course upon the belief that rabies never originates at present, any more than small-pox does, *spontaneously*; but is always propagated by the specific virus. And it is corroborated by the fact that rabies and hydrophobia are unknown in some countries: in the Isle of Cyprus, for example, and in Egypt. I fancy that South America is, or was, a stranger to it. It appears to have been imported into Jamaica, after that island had enjoyed an immunity from the disease for at least fifty years previously; and Dr. Heineken states that curs of the most wretched description abound in the island of Madeira; that they are afflicted with almost every disease, tormented by flies, and heat, and thirst, and famine, yet no rabid dog was ever seen there. On the contrary, 1666 deaths from hydrophobia, in the human subject, are stated to have occurred in Prussia in the space of ten years.

Very early in the disease, as it appears in the dog, the expression of countenance is remarkably changed; the eyes glisten, and there is slight strabismus. Twitchings of the face come on. About the second day a considerable discharge of saliva commences; but this does not continue more than ten or twelve hours, and is succeeded by insatiable thirst: the dog is incessantly drinking, or attempting to drink: he

plunges his muzzle into the water. When the flow of saliva has ceased he appears to be annoyed by some viscid matter in his fauces; and in the most eager and extraordinary manner he works with his paws at the corners of his mouth to get rid of it: and while thus employed he frequently loses his balance and rolls over.

A loss of power over the voluntary muscles is next observed. It begins with the lower jaw, which hangs down, and the mouth is partially open; but by a sudden effort the dog can sometimes close it, though occasionally the paralysis is complete. The tongue is affected in a less degree. The dog is able to use it in the act of lapping: but the mouth is not sufficiently closed to retain the water. Therefore, while he hangs over the fluid, eagerly lapping for several minutes, it is very little or not at all diminished. The paralysis often attacks the loins and extremities also. The animal staggers about, and frequently falls. Previously to this he is in almost incessant action. Mr. Youatt fancies that the dog is subject to what we call spectral illusions. He says he starts up and gazes eagerly at some real or imaginary object. He appears to be tracing the path of something floating around him, or he fixes his eye intently upon some spot in the wall, and suddenly plunges at it; then his eyes close, and his head droops.

Frequently, with his head erect, the dog utters a short and very peculiar howl: or if he bark, it is in a hoarse inward sound, altogether dissimilar from his usual tone, and generally terminating with this characteristic howl. Respiration is always affected; often the breathing is very laborious; and the *inspiration* is attended with a very singular grating, choking noise. On the fourth, fifth, or sixth day of the disease, he dies: occasionally in slight convulsions; but oftener without a struggle.

Mr. Youatt gives a detailed account of the appearances met with after death in the carcasses of these rabid dogs. They are not very constant or distinctive. The most curious and uniform consist in the presence of unnatural ingesta in the stomach: straw, hay, hair, horse-dung, earth. Sometimes the stomach is perfectly distended with these substances; and when it contains none of them, there is a fluid of the deepest chocolate colour mixed with olive; or still darker, like coffee: and when neither the unnatural ingesta nor the dark fluid appear, it will be found, Mr. Youatt says, upon careful inquiry, that the dog has vomited much hair, hay, straw, or the like.

In 1837, a few days after the case of hydrophobia occurred in the Middlesex Hospital, I saw the carcass of a dog, that had died rabid, examined by Mr. Ainslie at his and Mr. Youatt's Infirmary. The most remarkable morbid appearances were in the stomach, which contained some bits of straw and stick, and a considerable quantity of a dark fluid like thin treacle. In various parts of the stomach there were spots, almost black, of considerable size; apparently produced by dark blood partly extravasated beneath, and partly incorporated with, the mucous membrane.

I believe that Mr. Youatt's opinion, already mentioned, of the cause of rabies in dogs, and in all creatures — viz., that it always results from the introduction of a specific virus into the system — I believe that this opinion is not commonly entertained. Most people think that the disease is generated, *de novo*, in the dog at least; and causes have been assigned for it which certainly are not the true or the sole causes. Thus hydrophobia in the dog has been ascribed to extreme heat of the weather. It is thought by many to be particularly likely to occur in the dog-days; and to be, as Mr. Mayo observes, "a sort of dog-lunacy, having the same relation to Sirius that insanity has to the moon: which, indeed, in another sense, is probably true." Many cautions are annually put forth, about that period, for muzzling dogs, and so on: very good and proper advice, but, if those who have noted the statistics of the disease may be depended upon, it would be as appropriate at one period of the year as at another. Rabies occurs nearly as often in the spring, in the autumn, and even in winter, as it does in summer. M. Trollet, who has written an interesting essay on rabies, states that January, which is the coldest, and August, which is the hottest month in the year, are the very months which furnish the fewest examples of the disease. The disorder has often been ascribed to want of water in hot weather, and sometimes to want of food. But MM. Dupuytren, Breschet, and Magendie, have caused both dogs and cats to perish with hunger and thirst, without producing the smallest approach to a state of rabies. At the Veterinary School at Alfort, three dogs were subjected to some very cruel but decisive experiments. It

was during the heat of summer, and they were all chained in the full blaze of the sun. To one salted meat was given; to the second water only; and to the third neither food nor drink. They all died; but none of them became rabid. Nor does the supposition that the disorder has some connexion with the period of sexual heat in these animals appear to have any better foundation.

If you are desirous of knowing what my own opinion on this matter is, I must say that I think Mr. Youatt's doctrine by far the most probable one; that rabies never occurs except from inoculation of the specific virus. It has not been proved, and indeed it would scarcely be susceptible of proof, that the disease ever breaks out spontaneously; large tracts of country are totally free from it; and in nineteen cases out of twenty, perhaps, we trace the bite or the fray in which the inoculation has been effected.

If I were asked to define the seat of this terrible disease, I should place it, without hesitation, in that division of the nervous system which comprises the excitatory apparatus; the true spinal marrow, with its appendages of afferent and efferent nerves. Nay, I should go further, and say that it is the upper part of this apparatus, of which the functions are primarily and chiefly deranged: that the poison acts mainly upon the nervous arcs which pertain to the throat, and with which the eighth pair of nerves in particular is connected. There is nothing singular in this localization of the influence of a specific poison. The ergot of rye affects principally those arcs which belong to the uterus; cantharides those which govern the muscular fibres of the bladder. It is true that the mental functions are remarkably modified, and that paralysis of the lower extremities occurs, in most instances of the disease. But neither of these phenomena is constant; and they simply illustrate, when they do happen, the facility with which any morbid state of the spinal cord may propagate its influence in either direction. Whether in hydrophobia the essential change be centric or eccentric, cannot be determined with anything like certainty: but it seems to me to be most probable that the sensibility of the afferent nerves of the fauces, of the skin, and of the air-passages, is altered or morbidly exalted; whence, upon the application of the exciting stimulus, the peculiar sighing dyspnoea, and the strangling dysphagia, are produced by a reflected influence through the central axis upon the muscles concerned in these actions. But, as I said before, the pathology of the excitatory apparatus is as yet in its new birth.

What can I say of the *treatment* in hydrophobia; or in rabies? There is no well-authenticated case on record, that I am aware of, in which a hydrophobic person has recovered. As it has been, so it is still, *Ιατρος ιατραι θανατος*. The physician that cures is death. There can be no ground therefore for the recommendation of any especial drug, or form of medicine, nor even for any general plan of treatment, after the peculiar symptoms of the disease have once set in.

Of course those powerful remedial agencies that are in common use among medical men, have been fairly tried: copious blood-letting, mercury, opium, arsenic, sugar of lead, oil of turpentine, the cold affusion even: and not only those, but the strong poisons that are sometimes, but not so generally, employed for other diseases: belladonna, stramonium, prussic acid, white hellebore, strychnia, cantharides, the nitrous oxide gas: and no end of less gigantic remedies; such as alkalies, and especially ammonia, carbonate of iron, electricity and galvanism, tobacco-juice, and the guaco (which was introduced into this country a few years ago with high encomiums for its power over the disease), the mineral acids, violent exercise: and if we take into account the substances administered likewise to the brute, we may increase this list by the alisma plantago, scutellaria, box, and rue, all of which, at one time or another, have been vaunted as successful remedies; veratrum sabadilla also, and ticunas poison.

The difficulty of swallowing fluids, and in some cases of swallowing at all, is a serious obstacle to the fair trial of almost every form of internal remedy. It has been proposed to introduce powerful medicines into the rectum, in clysters; but to this also the patients have been found to make great resistance. The injection of medicines into the veins has been tried. Magendie hoped that he had discovered a cure, in first largely bleeding the patient, and then injecting his veins with a corresponding quantity of warm water: but it has always happened with this, and with other promising experiments, that just as the patient seemed to be about to recover, he has

died. The nervous irritability has in one instance or two been much calmed by the injection of a solution of a salt of morphia into the veins. In a case treated by Professor Todd, the symptoms appeared to be greatly mitigated, for a time, by applying ice to the cervical portion of the spine, and to the fauces.

When I last addressed you on this subject, I ventured to predict that the vapour of chloroform would be tried, and tried in vain, in this untractable malady. I can now inform you that it has been tried, and found as useless, except for the purposes of quieting fierce excitement, and of promoting the euthanasia, as every other remedial measure.

Mr. Mayo suggested bronchotomy: upon this ground (to use his own words), "that the principal character of the disease, and the rapid exhaustion which attends it, appear to depend in a great part upon the fits of spasm and closure of the glottis, brought on, not merely by the attempt or the idea of drinking, but by any sudden impression upon the senses. Now it is clear (he adds) that as far as the distressing feelings in the throat consist in a sense of suffocation, they would be put an end to or relieved by the establishment of a free opening in the windpipe." Dr. Marshall Hall would use, in combination with tracheotomy, the hydrocyanic acid. Now I should be sorry to say anything to damp your reasonable hope of benefit from any experiment; but I am bound to confess to you that I should not expect the smallest advantage from tracheotomy in this disease. The mode of death offers no encouragement to its use. There may be spasm of the glottis, but I doubt it. At any rate, the patients do not die of suffocation. The death is not death by apnoea, but by asthenia. We see persons labouring grievously for their breath for hours together, who yet survive, and are presently themselves again; persons, for instance, who are affected with severe spasmodic asthma. I have seen a man sitting up in bed a whole night long, inspiring with such difficulty that, if I had not been aware of his having, scores of times, been as bad before, I should have thought he could not exist five minutes longer. Now we have nothing of this dyspnoea in hydrophobia: and, as I said already, I am sorry, and diffident too, when I differ from great authorities on practical points, but I see no hope of cure, nor even of sufficient benefit to counterbalance the inconvenience and hazard of the operation, from the performance of bronchotomy. The principle is that of suffering the parts gradually to recover themselves, and of allowing the patient in the meanwhile to breathe through another channel. The principle is excellent (as I shall show you by and by), where there is a permanent obstacle to the admission of air to the lungs through the larynx; but in hydrophobia there is no such permanent obstacle to surmount. Though your patient, in laryngitis, should be at the point of death, yet open his windpipe, and he breathes again and is safe; but it is not at all uncommon for a hydrophobic patient to lose his spasms, to swallow well, and to breathe easily, yet he does not recover. This amendment is the prelude of death, the last flicker of the expiring lamp. Since I lectured upon this subject before, Dr. Latham has told me the following circumstance respecting a patient whom he treated for hydrophobia, in the Middlesex Hospital. He went one day to the ward, fully expecting to hear that the patient was dead. But he found him sitting up in his bed, quite calm, and free from spasm; and he had just drunk a large jug of porter. "Lawk, sir (said a nurse who stood by), what a wonderful cure!" The man himself seemed surprised at the change. But *he had no pulse*; his surface was cold as marble. In half an hour he sank back, and expired. Furthermore the experiment in question has been tried, and it has been tried by its proposer, Mr. Mayo, upon the dog, without affording, as Mr. Youatt assures us, the slightest relief. In the matter of cure, surgery, I fear, is as impotent as physic.

Not so, however, in the matter of prevention: this is the most important part of the practice. The early and complete excision of the bitten part is the only measure in which we can put any confidence: and even here we are met with a source of fallacy. In the majority of cases, no hydrophobia would ensue, though nothing at all were done to the wound. How can we know, then, that the disease is ever prevented by its excision? No doubt many persons go through the pain of the operation needlessly. But in no given case can we be sure of this. They get at any rate relief from the most harassing suspense, with which they would probably have been tortured for months. And if a large number of bitten persons, who had suffered the wound to heal as it would, could be compared with an equal number who had had the

bitten part cut out, hydrophobia would be found a frequent consequence of the bite in the first class—a very rare consequence of it in the second. Mr. Youatt, who trusted to caustic, and who had himself been bitten seven times, tells us that he had operated, with the caustic, on more than four hundred persons, all bitten by dogs respecting the nature of whose disease there could be no question; and that he had not lost a case. One man died of fright, but not one of hydrophobia. Moreover, a surgeon of St. George's Hospital told him that ten times that number had undergone the operation of excision there, after being bitten by dogs (all of which might not, however, have been rabid), and that it was not known that any one had been lost. Mr. Youatt, I say, trusted to caustic; and the caustic he used was the nitrate of silver. But I advise you to trust to nothing but the knife, if the situation of the bite will allow you to employ it effectually. If the injury be so deep or extensive, or so situated, that you cannot remove the whole surface of the wound, cut away what you can; then wash the wound thoroughly, and for some hours together, by means of a stream of warm water, which may be poured from a tea-kettle; place an exhausted cupping-glass from time to time over the exposed wound; and finally apply to every point of it a pencil of lunar caustic. If you cannot bring the solid caustic into contact with every part, you had better make use of some liquid escharotic; the nitric acid, for example. In my own case—and what I should choose for myself I should advise for another—if I had received a bite from a decidedly rabid animal upon my arm or leg, and the bite was of such a kind that the whole wound could not be excised, my reason would teach me to desire, and I hope I should have fortitude enough to endure, amputation of the limb above the place of the injury.

But if the wound be of such a size, and in such a part, that it can be excised, what is the proper way of cutting it out? Were I to give you any opinion, as from myself, upon that point, you might think, perhaps, that I was stepping beyond my proper province. I shall, therefore, again retail to you the advice of my old master, Mr. Abernethy. "The cell (he says) into which a penetrating tooth has gone, must be cut out. Let a skewer be shaped, as nearly as may be, into the form of the tooth, and then be placed in the cavity formed by the tooth; and next let the skewer, and the whole cell containing it, be removed together by an elliptical incision. We may examine the removed cell, to see if every portion with which the tooth might have come in contact has been taken away: the cell may even be filled with quicksilver, to see if a globule will escape. The efficient performance of the excision does not depend upon the extent, but upon the accuracy, of the operation." Mr. Abernethy was of opinion that when once the poison had been imbibed into the system, nothing ever had done good, and nothing, probably, ever would. I should be sorry to be so absolutely despairing in respect to a disorder from which dissection after death discloses no reason why the patient *might* not recover. He used to add, that as bleeding had been much extolled, had he hydrophobia he would allow a surgeon to bleed him even to death. Like Seneca, he would be willing to have his veins opened, though his disease might not permit him to indulge at the same time, like Seneca, in the luxury of a warm-bath.

I say *early* excision is the only sure preventive; but let me repeat that it will, in all suspicious cases, be advisable (if, for any reason, the operation have been omitted in the first instance,) to cut out the wound, or the cicatrix, within the first two months, or at any time before the symptoms of recrudescence have appeared. One would do it, though with less hope, as soon as possible *after* they had appeared; but I do not expect to hear of excision being successful then in stopping the disease. Dr. Bright has recorded a case in which the arm was amputated upon the supervention of tingling, and other symptoms, in the hand, in which the patient had been bitten some time before; but the amputation did not save him.

It has been proposed to fill the wound with ink, and then to wash it until every trace of the ink is gone; in this way, it is conceived, the complete ablation of the poison also will be ensured. With a timid or an obstinate patient, who would not submit to the knife or the caustic, some such expedient ought to be diligently tried; but it would be better to try it *after* excision, or after the application of the escharotic substance. It is impossible to take superfluous pains to obviate so fearful a disease as hydrophobia.

After the wound has been excised or cauterized, it has been recommended that it

should be prevented from healing, and made to discharge for a long time, by means of irritative dressings. This may be advisable when thorough excision, or complete cauterization, cannot be effected; but I should think it quite useless as auxiliary to those expedients, and only likely to keep up, or to produce, a hurtful irritability of the system.

The new power which we have happily obtained of suspending sensation by the inspiration of certain vapours will contribute to the prevention at least of hydrophobia, by divesting the process of excision of its pain, and therefore of its terrors.

I should perhaps have mentioned before, a theory, and a plan of preventive treatment, which made a great figure in all the journals, foreign and domestic, a few years ago. It was pretended by a Russian physician, Dr. Marochetti, that sometime between the third and the ninth day after a person has been inoculated with the hydrophobic poison, by the bite of a rabid dog, little pustules appear on or about the frænum of the tongue, containing a small quantity of sanious fluid, of a yellow or greenish colour. Pustules of the same kind were declared to exist also under the tongues of the mad dogs themselves. Now Dr. Marochetti pretended further, that if, from the very time of the bite, you gave the patient large doses of the decoction of broom tops, and looked out for the eruption of these pustules, which seldom lasted more than twenty-four hours, you might infallibly prevent the disease by opening and emptying the pustules, and then cauterizing them with a red hot iron; and afterwards causing the patient to gargle his mouth with that same decoction of broom. He held that the poison was deposited there for a short time, and then re-absorbed into the system; and he proposed to prevent such re-absorption. This was a very pretty theory; and took mightily in the medical world. But it has turned out a sort of hoax. I do not mean a wilful hoax on the part of Dr. Marochetti; for I have no doubt that he contrived to hoax himself. These pustules have been looked for again and again; but they have never been discovered in Englishmen affected with hydrophobia; nor in English mad dogs. The truth seems to be that the mucous follicles of the mouth, generally, and those at the base of the tongue, and those beneath the tongue, in particular, are commonly enlarged and exaggerated in the dog, and in the human animal, labouring under the disease; and these enlarged and altered follicles were regarded by the Russian physician as a specific eruption, which furnished the virus and pabulum of the complaint.

As almost every drug that has ever been included in any Pharmacopœia has been administered with the hope of *checking the disease*, so a great number of medicines and measures have been praised as preventives. Some people have great faith in sea-bathing; and they go to the coast to be ducked and half-drowned every day for six weeks; and if they escape hydrophobia they conclude that the immersion in salt water has saved them. Some of the specifics, as you may suppose, are great secrets; and they who possess them — whether they believe in them or not is another matter — sell them at no cheap rate to those who having been bitten by the dog, are weak enough to be bitten again by the quack. The composition of several of them has transpired; and they are found to consist either of ingredients the most insignificant and worthless, or of poisons of which the inefficacy had already been ascertained. The celebrated *pulvis antilyssus*, which was introduced by no less a person than Dr. Mead, into the London Pharmacopœia, was a mixture of ash-coloured liverwort and black pepper. The *Ormskirk medicine*, long famous, and scarcely obsolete yet in the north of England, was made up of bole armeniac, alum, chalk, elecampane, and oil of aniseed. The *Tonquin medicine* was composed of cinnabar and musk: and the *Tanjore pills* were a combination of mercury and arsenic. Even now scarce a year elapses but some correspondent of the newspapers, whose philanthropy is more conspicuous than his judgment or his knowledge, recommends a new and infallible preventive. I confess to you that I have not the slightest faith in any one of them; but as I had a great respect for Mr. Youatt's judgment, and as he was not *quite* so sceptical as I am on this point, and as patients or their friends will insist upon the adoption of protective measures sometimes, when the local means of prevention have been omitted or imperfect, I will tell you the result of his inquiries respecting these prophylactic drugs.

In the first place he never succeeded in curing the disease in the dog with anything that he had ever tried.

In the way of prophylaxis, he experimented with a great number of substances.

He thought that the box-wood, which is the basis of some celebrated preventive drinks in Hertfordshire and Kent, had some effect. He tried the alisma plantago, the boasted efficacy of which had been strictly inquired into by the magistracy of Toula, and the receipt purchased by the Russian Government at an immense price. But he had no success with it. He then put the belladonna to the test, beginning with two grains, and increasing the dose to a scruple twice every day, and continuing this for six weeks; and he says he is confident that he saved several dogs; but he lost almost as many. They all became debilitated and most rapidly emaciated.

Then, in the year 1820, his attention was directed to the scutellaria lateriflora, which Dr. Spalding, an American physician, had found highly successful as a preventive of rabies: and upon trial of it, he soon was brought to regard it as really valuable: and (not to tire you with a detail of his proceedings in the interim) he at length combined it with belladonna: "and the result" (I here quote his own language) "has been a medicine which I cannot, dare not, call a specific; for it has failed: but the use of which, in the cases of doubt and fear to which I have alluded, I would most earnestly recommend." He relates two experiments, which seem to have made a great impression upon his mind. They are as follows:—

"Three pieces of tape were thoroughly moistened with the saliva of a rabid dog, and inserted as rowels in the polls of three other dogs. To two the scutellaria and belladonna were given: the third, a fox-hound bitch, was abandoned to her fate. On the 29th day after the inoculation she became rabid." The others, at the time this was written, *i. e.*, some months subsequently, were living and well.

He afterwards took the same two dogs, and a third. He moistened two pieces of tape with the saliva of a rabid dog, and inserted them in the polls of one of the old dogs, and of the third dog. Another piece of tape, dragged repeatedly through the mouth of the same rabid dog *twenty-four hours after its death*, was inserted in the poll of the second of the old dogs. This dog and the new one were suffered to take their chance. To the other old dog the medicine was given. In the fourth week the new dog died undeniably rabid. The other two survived.

I repeat that I have no faith in these preventives. But sometimes some of them must be tried; and I would prefer those which are thus sanctioned by Mr. Youatt's good opinion to any others.

And with respect to the established disease, I think that if I were the unhappy subject of it, I should wish to be put into a hot air bath, and thoroughly sweated, and to take opiates; not so much in the hope of recovering as with a view to the euthanasia. But with all respect to those gentlemen who advocate that practice, no one, if I could help it, should make a hole in my windpipe.

LECTURE XXXV.

Epilepsy. Its symptoms and varieties; duration and recurrence of the paroxysms; periods of life at which they commence; warnings. Effects of the paroxysms, immediate and ultimate. Pathology. Anatomical characters. Causes.

THE great functions of which the brain is the material instrument are sensation, thought, and voluntary motion. The influence of the will is a cerebral influence: it reaches and acts upon the muscles through the interposition of the spinal cord. Motions that are involuntary belong more exclusively to the system of the true spinal marrow. Yet cerebral changes, morbid states of the brain, may excite them.

I have shown you that all these functions are liable, under disease, to be separately affected, and each in various ways and degrees. The number of combinations capable of arising out of disordered conditions of two, or three, or all of these functions, is very great. Yet the symptoms proper to the nervous system do arrange themselves

into groups sufficiently definite and constant to allow of our giving them distinctive names, and making them separate objects of inquiry.

At the same time, as might indeed be expected, these several groups have strong resemblances to each other. They are obviously of the same family: "*facies non omnibus una, nec diversa tamen; qualis debet esse sororum.*" Occasionally the features are so nearly alike, that we find it somewhat puzzling to determine with which of the sisters we are conversing; but usually there is some mark or other by which the individual may be identified.

Of these essentially nervous diseases, there are several in which the most prominent and obvious of the phenomena relate to the muscular system; irregular, violent, and involuntary contractions occurring of muscles which, in the healthy state of the body, are subject to the control of the will. I have spoken of two very frightful disorders belonging to this head:—of *tetanus*, namely, in which the muscles of voluntary motion present the most striking changes, being affected with tonic spasm; while the sensibility undergoes no other alteration than what is a consequence of that spasm, pain I mean in the muscles themselves; and the intellectual functions continue undisturbed:—and of *hydrophobia*, in which the natural sensibility suffers much, and the mental functions some derangement; yet still the characteristic features of the malady depend upon the irregular and uncontrollable action of muscles usually obedient to volition.

The disease which I am next to consider is scarcely less terrible to look upon, when it occurs in its severer forms, than tetanus or hydrophobia; but it is not attended with the same urgent and immediate peril to life. Yet it is, upon the whole, productive of even more distress and misery; and is liable to terminate in worse than death. You will understand that I am alluding to *Epilepsy*: a disease not painful probably in itself; seldom immediately fatal; often recovered from altogether: yet apt, in many cases, to end in insanity or fatuity; and carrying perpetual anxiety and dismay into those families which it has once visited.

The leading symptoms of epilepsy are, a temporary suspension of consciousness, with clonic spasm; recurring at intervals.

It is impossible to frame a perfect *definition* of epilepsy; nay, so various are its shapes, so numerous its modifications, that no general *description* even of it can be given. It will be necessary for me, therefore, here (as it has been before) to describe first the most ordinary form of the disease, as a standard; and then to note the several variations from that standard which are known to occur in practice.

A man, then, in the apparent enjoyment of perfect health, shall suddenly utter a loud cry, and fall instantly to the ground, senseless and convulsed. He strains and struggles violently. His breathing is embarrassed or suspended; his face becomes turgid and livid; he foams at the mouth; a choking sound is heard in his windpipe; he appears to be at the point of death by apnoea. But presently, and by degrees, these alarming phenomena diminish, and at length cease; the patient is left exhausted, heavy, stupid, comatose: but his life is no longer threatened. And in a short time he is once more, to all appearance, perfectly well. The same train of morbid phenomena recur however, again and again, at different, and mostly at irregular intervals. This is a brief description of the most ordinary form of epilepsy.

The suddenness of the attack is remarkable: in an instant, when it is least expected by himself or by those around him, in the middle of a sentence or of a gesture, the change may take place; and the miserable sufferer is stretched foaming, struggling, and insensible upon the earth. This fearful suddenness is expressed in the name of the disease, *ἐπιπλῆξια*, a seizure, an abrupt invasion. The ancients, among whom the complaint was well known, superstitiously ascribed it to the malice of demons, or to the anger of their offended deities. If a person were seized with epilepsy in the forum, it was considered an ill omen, and the meeting was at once dissolved, and all public business suspended for that day. Hence the disease was called *morbus comitialis*. *Morbus qui sputatur* was another of its names, because those present were accustomed to spit upon the epileptic man, or into their own bosoms; either to express their abomination, or to avert the evil omen from themselves. In this country its common designation is the *falling sickness*: or, more vaguely, *fits*. The cry which is frequently, though by no means always, uttered, is generally a piercing and terrifying scream. Women have often been thrown into hysterics upon hearing it. It is said

to have caused pregnant females to miscarry. Even the lower animals appear to be sometimes startled and alarmed by a note so harsh and unnatural. Dr. Cheyne informs us that, upon one occasion, "a parrot, himself no mean performer in discords, dropt from his perch, seemingly frightened to death by the appalling sound." The muscular convulsions are strong, irregular, and often universal. In most of the fits of which I have happened to see the commencement, the first effect of the spasm has been a twisting of the neck, the chin being raised, and brought round by a succession of jerks, towards one shoulder: and one side of the body is, usually, more strongly agitated than the other. The features are always greatly distorted. The brows are knit; the eyes sometimes quiver and roll about, sometimes are fixed and staring, sometimes are turned up beneath the lids, so that the cornea cannot be seen, and the white sclerotica alone is visible; the mouth is twisted awry; the tongue, thrust between the teeth, and caught by the violent closure of the jaws, is bitten, often severely; and the foam which issues from the mouth is reddened by blood. The hands are firmly clenched, and the thumbs bent inwards upon the palms: the arms are thrown about, striking the chest of the patient with great force, or bruising themselves against surrounding objects, or inflicting hard knocks upon the friends and neighbours who have hastened to the patient's assistance. It frequently happens that the urine and excrement are expelled during the violence of the spasm: and seminal emission sometimes takes place. The spasmodic contraction of the muscles is occasionally so powerful as to dislocate the bones to which they are attached: the joints of the jaw, and of the shoulder, have been thus put out; and the teeth are sometimes fractured.

When the convulsive paroxysm is over, the patient falls into a deep sleep. You might imagine that he slept from exhaustion, like a man worn out by great fatigue; but there is something more than this; the patient passes into a state of incomplete coma, or rather the insensibility continues after the convulsions have ceased. When he wakes he is often confused and incoherent for a time; by degrees, however, he resumes his ordinary appearance and condition; but he remembers nothing of what passed during the fit.

You may suppose that so much irregular contraction of the muscles of voluntary motion is not likely to occur without some derangement or modification of the functions of the circulation. The breathing is irregular, gasping, or arrested. The heart palpitates violently against the ribs during the paroxysm; the pulse becomes frequent and feeble; and sometimes it ceases to be tangible at the wrist during the height of the fit, and begins to be felt again as the spasms subside. The turgescence of the face indicates obstruction of the venous circulation; the cheeks and lips become purplish and livid, and the veins of the neck and forehead are visibly distended.

This, then, is one form, the most severe and the most common, as well as the best marked form, in which an epileptic attack occurs.

But there is a large class of cases, in which the symptoms are much more mild. There is very slight and transient, or even no convulsion at all; no turgescence of the face; no foaming at the mouth; no cry; but a sudden suspension of consciousness, a short period of insensibility, a fixed gaze, a totter perhaps, a look of confusion; but the patient does not fall. This is momentary; consciousness presently returns; the patient resumes the action in which he had been previously engaged, and is not always aware that it has been interrupted. Sometimes, with this temporary abeyance of the mental functions, there is some slight evidence of convulsion or involuntary action; the fingers of one hand, or less commonly of both, are moved irregularly, and without any object; or the eyes roll or are turned upwards; or the muscles of the face are twitched. Sometimes the patient is himself aware of what has been his condition, but shows some cunning in endeavouring to conceal it.

This slighter attack is called by the French, *petit mal*; while the severer form is named, *grand mal*. The former is spoken of also as *epileptic vertigo*, and distinguished by that appellation from the *epileptic fit*.

Of affections so different in degree, and in some respects so dissimilar in kind, you may be disposed to ask whether they really constitute the same disease. That they are essentially of the same stamp, we have this evidence; that both forms of attack occur in the same individuals. Sometimes a patient will suffer many recurrences of the epileptic vertigo, and at length will become affected with violent epileptic fits. Or the two forms will intermingle, sometimes the milder happening, sometimes the

severer. In such cases we cannot doubt that the attacks are in their nature the same, though different in their form and degree. And when (as sometimes happens) we meet with the slighter disease alone, we cannot refuse to assign to it the character and the name of *epilepsy*.

Between the two extremes, there are many links of gradation. Sometimes the sufferer sinks or slides down quietly, and without noise, is pale, is not convulsed at all, but insensible; much like one in a state of syncope. After recovering, he remains sick, languid, and confused, during the remainder of the day.

You will perceive, from what I have now said, the difficulty of giving any single description of epilepsy, which will include all its varieties. It is of course still more difficult to offer a strict definition of the disease. Cullen defines it to be "*musculorum convulsio, cum sopore*." Dr. Copland furnishes a larger and more comprehensive definition: "Sudden loss of sensation and consciousness, with spasmodic contraction of the voluntary muscles, quickly passing into violent convulsive distortions, attended and followed by sopor, recurring in paroxysms often more or less regular."

But almost every one of these circumstances may, in its turn, be wanting. There may be no convulsion; there may even be very slight and transient interruption of consciousness; there may be no subsequent coma or sopor; there may be no recurrence of the attack.

Yet I trust that you now have obtained some general notion of what is meant by an epileptic seizure. And I go on to inquire into several most important points connected with the paroxysms.

In the first place, they vary considerably in *duration*. Sometimes, as I have already stated, the seizure is slight, and does not occupy more than a moment or two of time. But even the severer attacks are often over in a few minutes. They seldom continue longer than half an hour, and probably the average duration may safely be laid at between five and ten minutes. Attacks that are spread over three or four, or more hours, generally consist of a succession of paroxysms, with indistinct intervals of comatose exhaustion. In the long-continued fits, or in the protracted succession of fits, the patients often die.

The periods at which the paroxysms *return* are also extremely variable. Occasionally the patient expires in the first paroxysm; occasionally, though he recovers from it, he never has another. Both of these occurrences are rare. Rather more frequently the fits recur at very long intervals; at the distance, I mean, of many years. Most commonly of all, they revisit the sufferer at irregular periods of a few days: sometimes every day, or every night: and not very unfrequently they take place many times in the twenty-four hours. This extreme frequency of repetition belongs principally to the slighter imperfect seizure, the *petit mal*. Sometimes the fits observe a strictly *regular* period of return; but, for the most part, they are quite uncertain and *irregular*.

The *time of life* at which the fits *commence*, and the circumstances attending their commencement, are deserving of notice. They not uncommonly begin in infancy. Those fits of convulsion to which young children are subject during the first dentition, and which sometimes appear to depend upon the irritation of teething, and sometimes upon manifest disorder of stomach and bowels — these fits are not distinguishable in their phenomena from genuine epilepsy, and we must reckon them as instances of epilepsy. It has been remarked by some one, that if you can trace the early history of an adult epileptic, you will almost always find that he or she suffered infantile convulsions.

[This is true. Epileptics, it will very generally be found, were affected with repeated attacks of convulsions during the period of infancy. In many subjects, however, we have known the disease to commence immediately subsequent to puberty, or even late in life. In the majority of these latter cases the disease appeared to be the result of a life of intemperance. — C.]

To what extent this is true I do not know, but I recommend it to you as a point worth attending to in your future opportunities of observation.

The epileptic attack may come on, for the first time, at *any* age. According to Dr. Bright (whose account of the disease, though short, is particularly perspicuous),

the most common periods are about the age of seven or eight years, probably about the time of the second dentition; and from fourteen to sixteen, shortly before the age of puberty. And the disease (he says) is very apt to occur for a few years subsequently to this. But sometimes the first fit has taken place between the ages of thirty and forty; in not a few cases, after sixty; and occasionally quite in the decline of life.

Dr. Bright offers a little piece of theory in respect to the periods at which epilepsy is apt to begin. It is a reasonable piece of theory, and serves to tie the alleged facts to one's memory, even if it be not yet proved to be true. Doubtless in many cases the circumstances that determine the first attack are quite accidental. But setting aside these casualties, he says, "there are leading periods in the evolution of the frame, and peculiar circumstances connected with certain periods, which may well be considered as influential in the production of the disease. In infancy, the nervous system is delicate, and easily acted upon by various causes of irritation. Then follows the trying period of teething. In a few years the second dentition occurs. In a few years later, all the great changes connected with the age of puberty. To this follow the excesses and exposures of manhood; and after the lapse of years, the vigour of the system fails, and many causes act to derange the nice balance of the constitution; the bowels often become sluggish; changes more or less serious take place in the structure of the arterial and venous systems; and many causes, organic or functional, which had before been unable to exert an influence on the vigorous frame, acquire power from its relative weakness."

The first accession of the disease takes place more commonly before than after puberty. Of sixty-six epileptic women, in whom the outset of that disease and the first period of menstruation were carefully noted, thirty-eight had epileptic fits before, and twenty-eight not till after that period.

The attacks are very apt to come on during the night; in the commencement of the disease they frequently are *confined* to the night. They are said chiefly to occur at the moment when the patient is sinking into sleep, or awaking from sleep. How far this is true I cannot tell. When the disease is *yielding*, the fits often happen in the night only; so that after they have, for a certain period, taken place in the day-time, or during the day and the night indiscriminately, it is reckoned a good prognostic sign if they begin to restrict themselves to the night. Some patients, under these circumstances, suppose that the physician has particular remedies that will make the fits happen in the night rather than in the day; and they ask for these remedies.

Sometimes each paroxysm arrives unannounced and unexpected; sometimes distinct *warnings* of its approach are given. The latter is less frequent than the former. Georget affirms that premonitory symptoms do not occur more than four or five times in a hundred cases. I am sure that this is much understated. When warning symptoms do happen, they are sometimes spread over a considerable period; several hours, or a whole day: sometimes they last just long enough to enable the patient to remove from a situation in which a fall would be attended with unusual danger: to dismount from horseback, to lie down in a boat, to get away from the fire-place, from the edge of a precipice, from the vicinity of water, to assume the horizontal position of his own free will and in his own manner, or to give notice to those about him of what is going to befall him. In some cases the warning is too short and sudden even for this. The *kind of notice* that he receives is very variable indeed. Often it consists in some unnatural state of the mind, the feelings, the temper; the patient is fidgety, irritable, low-spirited, timid, sullen; or, on the other hand, he feels unusually strong, and hearty, and cheerful. Sometimes there is a notable change in some one or more of the natural functions, or of the bodily sensations; the patient loses his appetite, or his appetite becomes voracious; a great flow of urine takes place; he smells an ill smell, is aware of a strange taste, hears extraordinary noises, or sees spectral illusions; not mere specks floating before him—*muscæ volitantes*—but distinct forms of persons and things not present. This is not very common, but it certainly happens. The late Dr. Gregory, of Edinburgh, was assured by a patient of undoubted veracity, that always, when he had a fit of epilepsy approaching, he fancied that he saw a little old woman in a red cloak, who came up to him, and struck him a blow on the head, and then he immediately lost all recollection, and fell down.

Headache, giddiness, dim or dazzled vision, are all of them common symptoms

among those which have been observed to be precursory of epilepsy. Sometimes there are circumstances which are obvious to a by-stander: a flushing of the face, or lividity, or, perhaps, pallor: delirium; difficult articulation; vomiting. Of twenty-one epileptics treated in the hospital at Wilna, by Joseph Frank, vomiting announced the paroxysm in seven. Some of the uneasy feelings are apt to come on and to continue even for several days previously to the attack; restlessness in particular, disturbed sleep, distressful dreams, a peculiar and sudden coldness of the extremities. An internal *working* is a phrase often used by such persons to express a sensation which is probably indescribable.

But the most curious precursory symptom of all, if we except the spectral illusions, is what is called the epileptic *aura*. This is a sensation which is likened by different patients to different things; to a stream of warm or cold air, to the trickling of water, to the creeping of a spider. The sensation proceeds commonly from some distant part of the body, — from one of the extremities, from a thumb, or finger, or toe, or from some spot on the trunk — and runs along the skin towards the head: occasionally it gets no further than the pit of the stomach: as soon as it reaches the head, or stops at the epigastrium, or elsewhere, the patient's consciousness forsakes him, and the paroxysm declares itself.

There seems to be some analogy between this epileptic aura and the well-known sensation, to be spoken of hereafter, of a ball rising from the stomach to the throat, and constituting the *globus hystericus*; except that in cases of epilepsy the sensation commonly begins in an extremity, and not in the stomach: and the fit comes on when it reaches the head, and not the throat. Sometimes, I think, these two sensations are blended.

In some instances, spasms of the muscles of the part whence the aura proceeds are observed to take place prior to the more general state of spasm.

This aura is certainly a very curious phenomenon. It has been thought to depend upon some change propagated along the nerve upwards to the brain, and to be sometimes connected with some injury done to, or some morbid impression made upon, an afferent nerve. I think that this explanation may apply to some cases.

Dr. John Thomson, of Edinburgh, used to state in his lectures, that he had known epilepsy to begin with an aura proceeding from an old cicatrix in the side. In a patient of my own, who was subject to epilepsy, the warning sensation commenced in one of his thumbs, which presently after began to be twisted inwards; but by tying his handkerchief tightly round the thumb, he could prevent the fit. Dr. Seymour mentions the case of an epileptic boy, who had learned to protect himself against a threatened paroxysm by biting his tongue.

In other cases the aura probably originates in some change within the head, and is analogous to the numbness or tingling that is often felt in some part of the body or extremities immediately before an attack of palsy or apoplexy. There is no real inconsistency in this twofold explanation: the source of the aura may be centric or eccentric; so also may the exciting cause of the paroxysm; as, in due time, I shall explain to you.

A knowledge of these warning circumstances is clearly of importance, always so far as respects the comparative security of the patient during the attack; sometimes as affording us the opportunity of staving off the fit altogether. And it is necessary to remark, that they sometimes give, as it were, a *false alarm*; they occur, and yet, although no measures of prevention are taken, no paroxysm follows.

The phenomena that *succeed* the paroxysm are also of great interest and moment.

I have already apprized you that the convulsions generally terminate before the insensibility is over: the patient draws, perhaps, two or three gasping sighs, and ceases to struggle. Some few persons are quite themselves again in a moment or two; some appear to recover consciousness, and then fall into a deep and prolonged slumber; but many do not regain their consciousness at all upon the cessation of the convulsions, remaining in a state of profound stupor, from which, however, they can generally be roused for a time. This state of coma (for it is nothing else) has been known to last a week. After the patient emerges from it, he is sometimes merely languid and inert; sometimes he is like a person stunned, or in a state approaching to idiocy, which gradually clears up; sometimes he is furiously delirious for awhile; not unfrequently there is a degree of partial paralysis, which also usually soon goes

off, though occasionally it is permanent; his eyes are fixed, or he squints, or his pupils are dilated, or he drags a leg, or he falters in speech. Sometimes he is completely hemiplegic. Most commonly he speaks of headache, or discomfort of some kind.

It is very seldom that the patients have any recollection whatever of what has passed during the fit. Many of them are not aware that they have had a fit: and those who do know it, discover the fact by finding themselves wet or dirty; by the injuries they have received during the convulsions; by the soreness of the bitten tongue; by their blood-shot eyes; by the bruises of their limbs; or by the confused or painful sensations which they subsequently experience, and which they have learned to associate with the conviction that a fit has happened, from having been informed on previous occasions, when they felt the same sensations, that they had suffered a paroxysm of insensibility and convulsions.

Upon the whole, it is seldom that any permanent ill effect can be noticed as having been left behind it by any one single fit; but, unhappily, this cannot often be said of their repetition.

Doubtless a single paroxysm does often leave the patient in a worse condition than that in which it found him; but this does not become perceptible to an ordinary observer, until after the alteration has been magnified and made apparent by repeated fits, and repeated small additions to the permanent injury. The friends of the patient remark that his memory is enfeebled in proportion to the number of the attacks; that his mental power and intelligence decline. His features even assume, by degrees, a peculiar character; he becomes subject to insane delusions, and too often he sinks at last into utter and hopeless fatuity. It is this tendency which renders epilepsy so sad and fearful a disease.

Foville affirms, that the intellectual degeneration is more constant, and comes on more early, in persons who are principally afflicted with the epileptic vertigo, the *petit mal*, the imperfect seizure, than in persons in whom the *grand mal*, the violent and decided paroxysm, takes place. Dr. Copland, on the other hand, is of opinion, that "the more severe the fits, the more is that result to be dreaded." This is a point which can only be settled by statistical facts. And as we all have the opportunity of collecting some such facts, and of adding them to the general stock, I mention this, and some other points that are still uncertain or disputed, as worth bearing in mind. More, probably, depends upon the *repetition* of the fits, than upon their precise *nature* or *severity*. Cases do occur in which epileptic persons preserve their faculties to a good old age; but those who are early epileptic do not often attain old age; and *whenever* the disease comes on, if it repeat itself frequently, it is followed much more often than not by impairment of the mind, or by some apoplectic or paralytic affection, which implies and accompanies the mental change. You will sometimes hear the cases of Julius Cæsar, of Mahomet, and of Bonaparte quoted, as examples of high intellectual power, existing and remaining in spite of epilepsy:—and it is allowable, perhaps, to make use of such cases for comforting the friends of epileptic persons: or for giving the advantage of sustained hope to the patient himself. But, in truth, these cases are not worth much. Napoleon is said, I know not upon what authority, however, to have suffered something like epilepsy during sexual intercourse. This is not very uncommon in persons subject to that disease. And, with respect to Julius Cæsar, we learn from Suetonius, that it was only in the latter part of his life that he laboured under epilepsy; and that he had two attacks while engaged in business.

Having now described the phenomena of epilepsy; the periods of life at which it is most apt to commence; its varieties; and its tendency and most common termination: let us next inquire what is known respecting the real seat and nature of this strange and melancholy complaint.

The functions that are affected are clearly functions of the *brain*. Sensation, thought, and motion regulated by the will, are the natural functions of that organ. The temporary abeyance of sensibility, thought, and volition; and violent and irregular action of the muscles which are thus withdrawn from the government of the will; constitute a paroxysm of epilepsy. We have, in this malady, another illustration of the fact, that when the controlling influence of the cerebrum is suspended,

the peculiar functions of the spinal marrow are exercised, not only in a disorderly, but also in an unusually energetic manner. That the brain and the spinal marrow, though physiologically distinct, are yet intimately connected with, and dependent upon, each other, a thousand familiar facts assure us; and there are good reasons for believing that the change, whatever it is, which is the immediate precursor and cause of the epileptic fit, may sometimes originate in the spinal cord, and thence extend to the brain; and sometimes originate in the brain, and communicate itself to the spinal cord. Dr. Marshall Hall's doctrine, that all convulsive diseases are diseases of the spinal marrow, cannot be properly applied to this convulsive disease of epilepsy. It is true that the spinal cord is concerned whenever there is convulsion; but it is concerned in every *voluntary* movement also, through the instrumentality of the brain itself; and it may be, and often is, irregularly influenced by a disordered and unnatural state of the brain. Tetanus may fairly be regarded as a disease of the cord and its proper appendages. The spasms arise and reach their height, while the powers of thought and sensation are undisturbed, and while volition remains, although the morbid condition of the cord renders it ineffectual. In epilepsy these cerebral functions are always implicated. There is *always* a loss of consciousness: and in the epileptic vertigo, the *petit mal*, there is frequently a suspension of consciousness only, *without any convulsion at all*. The brain, therefore, we must consider to be essentially concerned in this disorder.

What the precise state of the nervous matter may be, which determines the loss of consciousness and the spasms, we can only conjecture. A derangement in the relation between the arterial and venous circulation within the head; a temporary pressure somehow arising; a determination of blood towards the head; a diminution of the natural quantity of blood sent thither from the heart; all these have been assigned as possible causes of the paroxysms. Plausible reasons might be given in favour of the operation of each of them; but the speculation is more curious than useful. We have not yet penetrated the mystery of these remarkable phenomena, and it will be more profitable to turn to another question, which admits of a somewhat more definite answer, viz.: — what is the *morbid anatomy* of epilepsy?

Suppose that a person who has had epileptic fits, but in whom they have not been followed by any durable affection of the intellectual or locomotive functions, dies of some other malady; and that you have the opportunity of minutely examining the condition of his nervous system. Often you will find nothing at all which can throw any light upon the occurrence of the epileptic paroxysms; no appreciable alteration whatever in any part, either of the brain or of the spinal cord. In other cases you may discover some organic disease within the head: a scrofulous tubercle, a spiculum of bone projecting from the skull. Have you then detected the cause of the disease? All that can be said is, that the piece of bone or the tubercle was probably a *predisposing* cause of that derangement of the nervous substance which determined the paroxysms; the derangement itself, if, indeed, it were of such a nature as to be cognizable by our senses, has gone, with the symptoms; the tubercle or bone having in the mean time remained, without any sign which could betray its presence.

M. Foville, whose testimony in this matter is entitled to much weight, affirms that, in persons who have been subject to epilepsy, uncomplicated as yet with any permanent disorder of the intellect, or of the faculty of voluntary motion, and who have died *in the fit*, constant alterations are observable within the head; viz., a strong injection of the vessels of the encephalon. The membranes, the brain, and the cerebellum, are gorged, he asserts, with livid blood. But he goes on to say that this is to be ascribed to the mode of death; that we see the same appearances in persons who have died by hanging, or by any form of apnoea; that they are not peculiar to epilepsy, and do not explain the attack, but only point out the way in which it has been fatal.

It is, I fancy, a very common notion, both that such congestion does take place, and that it is the cause of the paroxysm: and it may be worth while shortly to state the reasons which are opposed to the conclusion, that the congestion (granting for the moment that it always happens) is a sufficient explanation of the attack.

In the first place it is not easy to conceive that the congestion could so suddenly arise and subside again, as it must sometimes do, if it be the immediate determining cause of the fit: within the space of a single minute, for example.

And in the second place, the signs of external congestion and plethora, by which signs we measure the amount of the *internal*, are most marked just when the symptoms of the paroxysm begin to subside and disappear. The congestion, which is the result of obstructed venous circulation, which again is a consequence of the muscular spasms, cannot be regarded as the *cause* of the convulsive symptoms.

Let us go a step further, and inquire into the state of the encephalon in those persons who, having suffered epilepsy, had, before death arrived, been affected with some permanent impairment of the mental functions, or (what often goes along with such impairment) with some degree or other of muscular paralysis or debility.

The most common alterations met with in the brain in such cases are the following.

Induration of the white matter of the brain, which presents a dull appearance; sometimes, besides the hardening, a general injection of the white matter; and in the majority of cases a marked dilatation of the blood-vessels. In some instances the consistence of the white matter is diminished, it is soft and flabby; but there is the same dilatation of the blood-vessels. These changes pervade the whole of the white matter in every part of the brain. At the same time the grey matter is found irregular on its surface, marbled or of a rosy colour in its substance, and sometimes altered in consistence. And in many cases the membranes are found to be adherent in some parts to the convolutions with which they lie in contact.

Such are the results of the experience of careful observers in respect to the morbid anatomy of epilepsy; of Morgagni, of Foville, and of MM. Bouchet and Casauvielh. The changes last described are such as are produced by chronic inflammation of the brain and its membranes. They are the *consequences* (I imagine) of repeated paroxysms of epilepsy; they are the very same as are frequently met with in cases of insanity complicated with paralysis, and they elucidate, therefore, the connexion of these affections; but they certainly teach us little or nothing of that actual condition of the nervous mass upon which the epileptic paroxysms depend. And, in truth, to expect to find in the brain the traces of convulsions that have passed away, would be as unreasonable as to expect to find there the traces of former voluntary movements.

Of those organic changes which may be regarded as strong predisposing causes of the paroxysms, my own experience accords with that of Dr. Bright; who states, that most frequently they are such as affect the surface, rather than the deeper-seated parts of the brain: tumours external to the cerebral matter; alterations in the membranes that envelope the organ, or in its bony case; the skull being very often unnaturally thick, heavy, and uneven. Various altered states of the spinal marrow have also been recorded.

But besides the morbid appearances that are *sometimes only* visible in the nervous centres themselves, there are others, which it is of great importance to attend to, situated in other parts of the body, and at a distance from those centres: diseased states of the liver; biliary concretions; granular kidneys; renal calculi; stones in the bladder; worms in the alimentary canal; diseases of the uterus; and of various other parts. And these morbid conditions have often, no doubt, an intimate connexion with the epileptic paroxysms. Accordingly some authors make almost as many varieties of epilepsy as there are organs of the body; they specify the cerebral, the spinal, the cardiac, hepatic, gastric, intestinal, renal, genital uterine, and so on. It will be sufficient, however, to consider two species only: that, namely, in which the disease originates in the nervous centres themselves, and especially in the brain; and that in which it originates in some other part. Most persons who have written on epilepsy make this distinction, although they employ different terms to express it: cerebral and occasional; primary and secondary; idiopathic and sympathetic; centric and eccentric. The last two terms are the best. But let us clearly understand them. The disease may, in one sense, be considered eccentric, even when it is situated in the brain; eccentric, *i. e.*, in respect to the true spinal marrow. But I apply the epithet centric to epilepsy when its cause lies in either of the two great nervous centres; the brain, or the cranio-spinal axis. In the eccentric species a morbid influence is conveyed by afferent nerves from the seat of the local disease to the spinal cord, and the convulsions mark its responsive action. This distinction we shall find to be an important one, both as regards the prognosis and the treatment. Let me, however, first say a word respecting the causes and the diagnosis of epilepsy.

There is no doubt that a tendency to epileptic disease is frequently *hereditary*. It

may be derived from parent to child; or it may skip over a generation or two, and appear in the grandchild or great-grandchild; or it may be traceable only in the collateral branches of the ancestry. This is just what takes place in other hereditary maladies. You may often notice also that other forms of nervous disorder prevail in the same families.

MM. Bouchet and Casauvielh found that among 110 instances of epilepsy, 31 were hereditary. Of 321 persons afflicted with epileptic insanity, and seen by Esquirol, 105 were descended from insane or epileptic parents.

Again, a tendency to epilepsy is very often found to go along with an *unnatural form of the head*, which is pinched up like a sugar-loaf; or misshapen and unsymmetrical, one half being unlike the other; or in some way or other oddly configured. Epilepsy is no uncommon attendant of chronic hydrocephalus.

And thirdly, the *scrofulous diathesis* is a strong predisposing cause of epilepsy. Dr. Cheyne even holds that epilepsy is as certain a manifestation of the strumous disposition, as tubercular consumption, or psora abscess. Now, of the two predisposing circumstances last mentioned, it may be observed, that they commonly merge in that which preceded them: the strumous diathesis, and a particular conformation of the head, are both very likely to descend from parents to their progeny.

Whether the sex has any influence in determining a predisposition to epilepsy, is a question that remains to be settled. Foville thinks it is most common in females; Dr. Elliotson, in males. I have certainly seen more epileptic boys and men, than girls and women. But the casual experience of a single observer is not enough to determine the point. We want numerical statements on a large scale. At the close of the year 1813 there were 162 male epileptics in the Bicêtre; 289 female cases in the Salpêtrière. Jos. Frank observed that, of 75 patients, 40 were females.

LECTURE XXXVI.

Epilepsy, continued. Recapitulation. Exciting causes. Simulated Epilepsy. Diagnosis. Prognosis. Treatment: during the fit; during the intervals; during the warnings.

AT our last meeting I began to speak of epilepsy. Let me rapidly retrace the ground we then passed over.

An epileptic seizure may be very severe; or very slight. The very severe attacks are characterized by a sudden cry, immediate loss of consciousness, general and violent convulsions, and subsequent coma or heavy sleep. The very slight attacks consist in a momentary abeyance of the mental faculties, sometimes with and sometimes without slight and partial convulsion. These extreme forms of epilepsy we judge to differ only in degree, inasmuch as they both attack the same persons at different times; or the one form conducts to the other. Between these the gradations are innumerable. We call the extremes the *epileptic fit*, and the *epileptic vertigo*; the French name them the *grand mal* and the *petit mal*.

These *fits* may last from a few seconds to half an hour. Paroxysms apparently longer than this commonly consist of a succession of fits. The average duration is from five to ten minutes.

The fits recur at variable intervals; which are sometimes periodic, mostly irregular. There may be many in a single day; there may be only one for many years. They are commonly more severe in proportion as they are less frequent.

The epileptic seizures sometimes begin in early infancy: another period at which they often commence is about the age of seven or eight: another about fourteen or sixteen, or for some few years after that age. They more frequently begin before puberty than after it. Sometimes the first fit takes place in the middle period of

life: sometimes even in declining age. They often occur in the night, especially in the outset and towards the close of the disease; usually when the patient is between asleep and awake; *i. e.*, at the commencement or at the termination of his slumber.

In the majority, perhaps, of cases, the fit is unexpected, and preceded by no warning. But in other instances there is some alteration perceptible by the patient himself, or by his friends, giving notice of its approach; some change in the temper, feelings, appearance; some disturbance of the senses; ocular spectra; or what is called the epileptic aura, a creeping sensation arising in some part of the surface, generally of the extremities, and gliding towards the head. Some of these warnings precede the paroxysm by a day or two, or by a few hours; some by two or three seconds only. Sometimes the blow is threatened by their appearance, but it does not fall.

The fit is almost always, in its severer forms, attended and followed by coma; sometimes, after the coma, by temporary confusion of mind; deafness; slight paralysis; delirium; inarticulate speech. There is seldom any appreciable permanent damage effected by a single fit.

A repetition of the fits leads, in a large majority of instances, first, to a defect of the memory, and of the general intelligence; and at length to a peculiar expression of countenance, to decided imbecility of mind, to complete fatuity; and with this there is often associated some paralysis or muscular debility.

The convulsions take place, necessarily, through the medium of the spinal cord and nerves—just as voluntary movements do; but the suspension of sensation, thought, and volition (which suspension is seldom absent, while the irregular muscular action often is wanting) shows that the *brain* is essentially involved in the disease.

Accidental organic lesions are sometimes (and sometimes only) found in the encephalon, or in the spinal cord, of persons who have suffered epilepsy uncomplicated with any permanent mental or paralytic affection; tubercles, for example, or bony growths from the interior of the skull; but as these are *constant*, while the paroxysms are *occasional*, and as in the intervals they give no signal of their presence, we can only regard them as being probably predisposing causes of the seizures.

When the epilepsy *has* been complicated with permanent alienation of mind, or with some degree of paralysis, evidence of chronic inflammation of the brain and its membranes is generally discovered. This has been the consequence of the repetition of the paroxysms. This explains the frequent connexion of fatuity and palsy with epilepsy of long standing.

The diseased condition which excites the paroxysms may be situated in the nervous centres themselves, or in some other part of the body. In the one case we call the disorder centric, or idiopathic; in the other, eccentric, or sympathetic. We cannot always be sure with which species of the disease, the centric, or the eccentric, we have to deal; but the distinction, when it can be made, is of considerable importance, in respect to the prognosis, and in respect to the management of the case.

The predisposition to this fearful complaint is often hereditary. Malformation, or defect of symmetry in the two sides of the head, is a frequent predisposing cause. So, pre-eminently, is the scrofulous diathesis. And these two, *viz.*, the scrofulous diathesis, and a peculiar conformation of the head, are both liable and likely to be propagated from parents to children. But the predisposition is found to be hereditary even when the shape and structure of the body is, to all appearance, quite perfect and natural; and when no outward indication of the strumous diathesis is perceptible.

At the very close of the lecture I informed you that it is an unsettled question—and it is not a question of very great importance—whether the disease be more common in females or in males: whether the *sex* have anything to do with the predisposition.

Taking epileptic people as a class, you will find them to be generally characterized by weakness and irritability of mind and body, and not by steadfastness and vigour; by a lack rather than an excess of vitality. They are much more commonly pale than florid, anæmic than plethoric, feeble than robust, melancholic than sanguine, timid than bold. And these indications of defective vitality become still more manifest in those who have been long or frequently subject to the disorder. The inherent debility of the system augments as the paroxysms multiply in number.

There are certain vices, which are justly considered as influential in aggravating, and even in creating, a disposition to epilepsy: debauchery of all kinds; the habitual indulgence in intoxicating liquors; and, above all, the most powerful predisposing cause of any, not congenital, is masturbation—a vice which it is painful and difficult even to allude to in this manner, and still more difficult to make the subject of inquiry with a patient. But there is too much reason to be certain that *many* cases of epilepsy owe their origin to this wretched and degrading habit: and more than one or two patients have voluntarily confessed to me their conviction that they had thus brought upon themselves the epileptic paroxysms for which they sought my advice.

Among the *exciting causes* of epilepsy, fright is conspicuous. And any *strong mental emotion* is apt to produce the fit, in a person who is already subject to the disease. This fact alone would be enough, I conceive, to forbid our ascribing the paroxysms exclusively to an affection of the spinal cord. Bodily pain; manifest and great disturbance of almost any of the principal functions of the body; may act also as exciting causes. Sometimes the cause is obvious, sometimes it is quite inscrutable. If the attack occur every night, Dr. Bright thinks it may be attributed to the “congestion” of sleep: if it take place at monthly intervals in women, we may “often trace it to nervous irritation in sympathy with the uterus: and when long periods have intervened we may usually trace each distant paroxysm to the repetition of some excess, or to a neglected state of the bowels.” In these latter cases, the epilepsy is of the sympathetic, or eccentric kind; the irritation being seated in some part at a distance from the nervous masses, in the stomach, or intestines, or uterus. Now I would suggest the expediency of observing what muscles or sets of muscles are *first affected* by the spasm in such cases, and in what part the warning aura (if there be any) arises: because by accurately noting these particulars, we may, perhaps, be led to a knowledge of the part or organ in which the irritation operates: and if we know the *seat* of the irritation, we shall be more likely to know its *nature*, and its *cure*.

[M. Lamonthe relates, in the *Journ. de Méd. de Bourdeaux*, a case in which the epilepsy was caused by a foreign body in the ear, and ceased upon its removal. The patient was a man thirty years of age, in whose external meatus a pebble had been accidentally introduced. He at first experienced only a slight diminution of hearing, afterwards suppuration occurred, and finally epileptic attacks supervened; from which he had suffered for two years before he consulted M. Lamonthe. This gentleman, being informed of the probable existence of a foreign body in the ear, made an examination, and detecting it, by proper means, succeeded in removing from the meatus a rough pebble of nearly a triangular shape, and from that period the patient had no more attacks of epilepsy. In the same Journal, another case of the same kind is related by M. Roussilhe. — C.]

Among the exciting causes of epileptic fits are also enumerated—and I believe, from what I have myself noticed, with great justness—the repulsion of eruptions, and especially of eruptions about the head, when proper artificial evacuations are not at the same time obtained; the cessation of habitual discharges; and, on the other hand, profuse and unusual discharges. Hæmorrhage certainly does often bring on convulsions and a state of insensibility, exactly like certain forms of epilepsy. Persons who are bled till they actually faint, are sometimes, while fainting, convulsed also. And animals that are killed by loss of blood, are always affected with convulsions before they expire.

There is yet another very singular occasional cause of epilepsy that deserves to be mentioned, viz., the sight of a person in a fit of that disease. This has been noticed over and over again. Not only will a patient who has already suffered such attacks often fall into one upon seeing another so affected; but people will even sometimes do so who have never before shown any symptom of epilepsy. In this way the disease will now and then run through a boarding-school; or through a ward in a hospital. There is a very good example of this recorded in the 11th volume of the *Medical Gazette*, by Dr. Hardy, of Bath. A strong healthy young man was hired to take care of an older patient, who suffered frequent and exceedingly violent paroxysms of epilepsy. He remained with the patient night and day; and at the end of seven weeks became himself epileptic in a very high degree. An acquaintance of his, of

equally robust make, but some years older, occasionally visited the two. In a fortnight from his first visit he also was seized with similarly violent attacks. Dr. Hardy quotes the following short case also from Baglivi:—"Vidimus, anno 1690, in Dalmatiâ juvenem gravissimis correptum convulsionibus, propterea quod inspexerat solummodo alium juvenem dum epilepsiâ humi contorquebatur."

[M. Meyer has recently given an account of a number of the pupils in a female school that were attacked with epilepsy, in consequence of seeing one of their number under the influence of the disease. Most of the subjects were approaching the period of puberty; whilst they were all of a highly excitable temperament. — C.]

Dr. Cullen, who, as well as many others, had noticed the same thing, starts the question whether this mode of propagation of the disease be imputable to dread and horror; or to the mere force of imitation, which is often so strong, in health as well as in disease: and he decides in favour of the force of imitation. In fact, there are many other sights equally horrifying with that of a person in convulsions; yet there is *no spectacle* of horror so efficacious in producing a fit of epilepsy in others, as that of a person suffering under epilepsy.

This principle of imitation holds good in many of the spasmodic diseases: and in some of them, especially in hysteria, its influence is more remarkably seen than in epilepsy: I shall therefore have to recur to it again. There is one very curious fact, however, which relates to epilepsy in particular. You are aware that this disease is often feigned by impostors. Now I believe it is ascertained, beyond the possibility of doubt, that fits and actions which were at first, in these pretenders, strictly voluntary, have at length become involuntary and uncontrollable, and have passed into paroxysms of real epilepsy. The rogue is caught in his own trap.

[The following statistics are presented by M. Leuret, in an interesting paper on Epilepsy in the *Archives Générales* for May, 1843. Among 106 epileptics, in 24, or nearly one-fourth, the disease commenced between the tenth and fourteenth years of their age; in 18, or nearly one-sixth, between the fifteenth and nineteenth years, and in 16, between the fourteenth and twenty-fourth years. Thus 58 patients, or more than one-half, were first attacked between their fourteenth and twenty-fourth years. Of the whole number of cases, in *six* only was it ascertained that the disease existed in one or other parent, and in but *eight* was it found that one of the parents had died of any disease of the brain, namely, three of insanity, two of apoplexy, one of paralysis, one of suicide, and one of meningo-cephalitis. Of the 106 patients, thirty had been drunkards, twenty-four masturbators, and fifteen addicted to women. In *fifteen* cases, the actual or presumed cause of the first attack of epilepsy was ascribed to terror; in *twelve* to onanism; in *six* to drunkenness; in *two* to anger; in *two* to distress; in *two* to falls; in *one* to *libertinage*, &c. *Thirty* of the patients had an attack very regularly once a fortnight: 17 suffered attacks once a month; 13 once a week; 9 every three or four days: 4 almost every day; 2 every day; 1 every two months; 3 every three months; and 24 at very irregular intervals. In 35, the attacks took place in the night especially; in 29, they were as frequent in the day as in the night; in 12, they frequently occurred in the day; in 8, they occurred during the day only; in 8, during the night only; in 3, in the morning only; in 3 others generally in the morning; and in 4, in the evening only. — C.]

And the mention of these impostors leads me to consider the *diagnosis* of epilepsy. First, how are we to distinguish the feigned disease from the true? Secondly, are there any other real diseases which may be mistaken for epilepsy, or for which epilepsy may be mistaken?

In the number of *feigned* diseases epilepsy is one of the most common. Soldiers and sailors pretend to have epileptic fits, in the hope of obtaining their discharge from the service. Cases of simulated epilepsy occur also continually in our streets among mendicants and impostors, who think to excite the compassion and pecuniary charity of the credulous; and are even sometimes actuated, I believe, by a desire to obtain admission into hospitals, where they live tolerably well, and quite idly. It is easy enough, they think, to throw their legs and arms about, and to grin; and many of

them get up a capital show of foaming at the mouth, by placing a bit of soap between the gums and cheek. The means of detecting these vagabonds are of some importance to us all; and it is more particularly necessary that they should be well known to those who are likely to join the medical department of our fleets or armies.

It is of course desirable, in questionable cases, to witness a fit. But pretenders are not very willing to perform when they know that a medical man is looking on. You may sometimes convict them, in the absence of the fits, by cross-examination. A cheat will seldom be consistent in the account which he gives of his fits; as to whether they are regular or irregular; and as to the times and places in which he has suffered them. An impostor chooses such situations for his exhibition as are most suitable to his own purposes; a crowded street, or a well-frequented public walk. True epileptics almost always select retired places to take exercise in; especially if they have any warning or expectation of the approach of a paroxysm. You will find also that the impostor is not attacked at his own home; but always fixes upon some spot in which he is not only sure to be seen by others, but in which he is not likely to sustain any injury by tumbling down. True epileptics are often seriously hurt by their falls; feigned ones generally come off without much bodily damage. However, when the fits are alleged to be frequent, and when also they occur regularly, you may soon expect one, and must make a point of being present; and then you will seldom fail to remove or to verify your suspicions. In the first place, the muscular power of epileptics is far beyond what is natural. It will sometimes take four or five stout men to hold a weak emaciated lad, in a fit of epilepsy. Of course no impostor can command more than his natural strength. In the second place, a real epileptic fit, if it last long, is seldom violent; whereas impostors, for obvious reasons, make their fits both long and violent. You may often get much information from the state of the eyes, which usually in true epilepsy are partly open, with the eye-ball visibly rolling and distorted. In feigned epilepsy the actor almost always prefers to shut his eyes completely. Sometimes, if he be closely watched, and no suspicion be expressed, he will be seen to open his eyes occasionally, to ascertain the effect of his exhibition upon the bystanders. In real epilepsy, too, the pupils are often considerably dilated, and do not contract when stimulated by light. This is a very sure criterion; for no impostor can prevent his iris from acting on exposure to vivid light. The pulse, in true epilepsy, is not only frequent, but often irregular also; a circumstance which never can be imitated. The skin of an epileptic, during the fit, is commonly cold; but that of an exhibitor is hot, and covered with sweat, obviously the consequence of his violent and voluntary exertions. In this respect, also, it is scarcely possible for him to deceive us. Again, an impostor will not bite his tongue, as epileptics often do; nor very willingly void (like them) his excrements and urine during the fit; indeed, it would not be very easy for him to do so, and at the same time to carry on the necessary pretence of convulsions. Besides, epileptics, during a fit, are quite insensible to external impressions; and hence the vulgar modes of detection, though harsh and not to be recommended, are often effectual ones; such as dropping melted sealing-wax upon the patient; putting some gin into his eye; burning him with a hot poker; or (what I believe is more fashionable among beadles and police constables, when they have to administer to such patients) the pressing your thumb-nail violently under that of the supposed impostor. This causes exquisite pain, yet inflicts no lasting or serious injury; and I believe that few pretenders stand out against this expedient. It is astonishing, however, how much torture some of them will bear before they can be brought to confess their imposition. If we speak of having recourse to some of these painful tests in the hearing of the pretender, we shall find that the fit will soon come to an end. Dr. Cheyne mentions an instance in which one table was placed upon another, and a soldier, who was supposed to be shamming, was laid upon the upper one, while his paroxysm was on him; and the fear of falling from such a height soon stopped the convulsions. Mr. Hutchinson relates the case of a sailor who was suspected to be a cheat, in whom the convulsions were instantly removed by blowing some fine Scotch snuff up his nostrils through a quill. This brought on *another* kind of fit, viz., a fit of *sneezing*, which lasted nearly half an hour; and there was no return of the epilepsy so long as Mr. Hutchinson remained in that ship. He tried the same expedient in cases of *real* epilepsy, but never could produce any similar effects, although the

patients were not snuff-takers. There was a beggar in Paris, who often fell into epileptic fits in the streets; one day some compassionate spectators, fearing that he might injure himself in his struggles, got a truss of straw and placed him upon it: but when he was in the height of his paroxysm, and performing remarkably well, they set fire to the straw; and he presently took to his heels.

There is another ingenious plan, very likely, I should think, to detect an impostor, and yet not calculated, like the one last mentioned, to injure a real sufferer; which is to propose gravely, in his hearing, to pour *boiling* water upon his legs, and then to proceed actually to pour *cold* water upon them.

Of the *real* diseases which are apt to be confounded with epilepsy, *hysteria* is the chief. The question whether a given case be one of epilepsy, or of hysteria, very often arises. By a careful attention to several circumstances, the discrimination is generally to be made. In the first place, the total suspension of consciousness, which is so constant an accompaniment of the epileptic paroxysm, does not take place in the hysterical. In epilepsy there is no globus hystericus, no alternations of laughter and tears. The solitary cry which ushers in the epileptic attack so frequently, and which is so characteristic, is not heard in hysteria. Not that hysterical girls do not scream, for they often do; but then it is repeatedly and continuously. The heavy comatose sleep that succeeds epilepsy is not common in hysteria. Hysterical patients contrive also to avoid hurting themselves by their contortions: they do not bite their tongues nor foam at the mouth. Dr. M. Hall teaches that, in epilepsy, there is a forcible closure of the larynx, and expiratory efforts which suffuse the countenance, and probably congest the brain, with venous blood. In hysteria the respiration, on the contrary, is rapid and sobbing.

It is interesting to remark how early and how strongly the muscles that lie about the throat are implicated, in each of the three terrible spasmodic disorders which we have been contemplating. Tetanus begins with cramp in the muscles of the back part of the neck, and of the lower jaw. The pharyngeal muscles, as well as those subservient to respiration, are intimately concerned in the paroxysms of hydrophobia. In epilepsy it is Dr. Hall's belief that the platysma myoides plays an important part, preceding often, in spasmodic contraction, the muscles that shut up the larynx, compressing the jugular and other veins of the neck, and so producing congestion of the parts within the cranium. In all three the stress of the malady is first visible in muscles which obey the influence of the medulla oblongata, or of the upper portion of the spinal cord.

Observe that I have been speaking, all along, of what has been sometimes called *habitual* epilepsy. It is not every attack of convulsions with insensibility which ought to be so named. Such attacks are apt to follow sudden injuries done to the brain; stunning blows on the head, fractures of the skull, the eruption of blood in sanguineous apoplexy, and even overwhelming emotions of the mind. The retention of urea in the unpurified blood, occurring in connexion with a peculiar renal disease which I have frequently alluded to already, and which I shall hereafter describe, appears to be a frequent cause of similar seizures. They happen also in parturient women, in persons poisoned with lead, in hydrocephalic patients, in persons affected with hypertrophy of the brain, and in the outburst of some of the eruptive fevers. With these casual occurrences of epileptiform convulsion I do not here meddle.

Epilepsy is one of those complaints concerning the *probable issue* of which the patient, and still more the patient's friends, are sure to make repeated and anxious inquiries. It is seldom that we can pronounce with any confidence a favourable prognosis; but there are some cases in which the prospect is much worse than in others.

If we have reason to believe that the disease is *centric*, and connected with any organic derangement of the nervous centres themselves, the prognosis must be bad. *Cæteris paribus*, it is rendered worse by the coexistence of any sign of scrofulous disease, or of the well-known bodily characteristics of the scrofulous diathesis: it is rendered worse, also, when the disease has happened in the parents, or among the more immediate ancestors, of the patient; whenever, in short, there is reason to think the disposition to it is inherited. The prognosis is bad when the complaint occurs in persons who have slanting foreheads and misshapen skulls; and when the epileptic physiognomy has become established. The prognosis is always the more unfavourable

the longer the disorder has lasted; the oftener the fits have been repeated; and the more habitual they have become. And when the memory is permanently enfeebled, or fatuity has come on, or the disease is complicated with any form or degree of paralysis, the case is hopeless; so far, at least, as a perfect cure is concerned.

On the other hand, the prognosis is better when the disease is *eccentric*: i. e., when there is any obvious exciting cause of the paroxysms, manifest in structural or functional disorder of some part of the body other than the nervous matter. And when this eccentric cause is removeable—a stone in the bladder, for instance, worms in the intestines—then the prognosis still further improves. On this account the prognosis is better in children than in older persons, for the exciting cause is often clearly eccentric, and likely to be transitory; the irritation of teething for example; and besides this, it is stated by many practical writers that even repeated and habitual attacks of epilepsy in children often go off as the patients grow older, and especially at the age of puberty. The experience of Heberden, however, was against this. He says that he had known several persons *become* epileptic at that time; but that he had never met with one who had *then* got rid of the disease. He had seen a few who had recovered before, and soon after, the age of puberty. Dr. Elliotson mentions a case in which a girl had epilepsy prior to the first period of menstruation: then the fits stopped; and she remained free from them until in advanced life the catamenia ceased to recur; and then the epilepsy returned. In all those cases in which we can assign some evident cause for the fit—such as the use of improper food, uterine irritation, mental emotion, and so on—the prognosis is somewhat better than usual.

[In the predisposed, one of the most frequent causes by which the paroxysm is brought on, is errors in diet, either in regard to the quality or quantity of the food taken. We have known cases in which the patients remained free from an attack so long as they abstained from a particular article of food, but invariably experienced one on partaking of it. — C.]

“The eccentric epilepsy (says Dr. Hall) is to be viewed as *curable*, however *difficult* of cure.” And however unfavourable the prognosis may be, there is nothing that can excuse any apathy or neglect on the part of the practitioner. Though few cases of habitual epilepsy admit of a *cure* under any treatment, yet there are few which may not be *relieved* by treatment, so far as regards the *frequency* or the *violence* of the fits, or *both*.

The *treatment* of epilepsy resolves itself into the measures to be adopted during the fit; and the measures to be adopted during the intervals between the fits.

In the paroxysm itself we have to provide against the risk of injury from the struggles and contortions of the patient; and, if possible, to mitigate the violence, and to shorten the duration of the fit. The patient should be placed in the centre of a large bed; his neckcloth, and any ligatures about his person, should be loosened; his head should be somewhat elevated. When the risk of his hurting himself cannot be avoided in any other way, his limbs should be restrained by the bystanders, or secured in a waistcoat. Some persons have advised that a piece of cork or soft wood should be placed between his teeth, to prevent him from biting his tongue, or breaking his teeth. But it is not easy to manage this expedient cleverly. If the head be visibly congested and hot, cold wet cloths may be applied to it with propriety; and if, at the same time, the extremities be cold, means of restoring warmth to them should be adopted.

I do not know whether art can abbreviate the paroxysm. Some years ago the late Barry O'Meara sent a letter to one of the newspapers, saying that he fancied he had seen a popular remedy useful in such cases; that, namely, of cramming salt into the patient's mouth: he thought he had succeeded in bringing the patient about by that expedient. In the epileptic patients that come into hospitals, the physician, not being always on the spot, does not see all, nor even many of the paroxysms; but after reading that letter, I desired the nurses to treat all my patients who might be seized with epilepsy in the wards upon that plan: and on comparing the length of the paroxysms when the salt was used, with their ordinary duration as reported by the friends of the patient, or as previously observed in the hospital during some of the earlier fits, it certainly did seem to curtail the convulsions. Probably it is more calculated to relieve

a hysterical than an epileptic fit. In the epileptic fits of children much benefit often results from immersing them in warm water: particularly if there be any coldness of the extremities.

It is very much the fashion to bleed persons who are seen in a fit, of whatever kind; and to bleed them largely. I have already given you my opinion respecting the indiscriminate use of this strenuous measure in apoplectic attacks. If it be clear, from the phenomena, or from the known history of the patient, that the case is one of epilepsy, bleeding, *during the fit*, will seldom be necessary or proper; unless, indeed, the evidence of cerebral plethora is very strongly marked: and even then I would advise you not to do more than take a moderate quantity of blood, by cupping, from the neck or temples. The convulsions and the sopor may be expected soon to pass off; as soon, probably, and as completely, without, as with, any abstraction of blood. Whereas the difference of the alternative is not trifling, in respect to the condition in which the patient may be left when the fit is over. The injurious effect of excessive blood-letting upon the system at large, is manifest, sometimes, for months afterwards.

During the intervals between the attacks we seek to prevent their recurrence; and this end is to be attained, when it is attainable at all, by getting rid of the predisposition to the disease on the one hand, and by protecting the patient against its exciting causes on the other. Now there are certain kinds and causes of predisposition which we *cannot* get rid of; such are the tendency that is *inherited*; the strumous diathesis; malformation of the head; the presence of some organic lesion in the brain or spinal cord. Vicious and dissolute habits are also difficult, but not impossible, to eradicate. It will be our duty when such are discovered, to set strongly before the unhappy patient the dreadful end towards which he is hastening; the certain loss of reason to which, when once the disease has shown itself, the continuance of his baneful indulgences will drive him; and to urge upon him the necessity for a short and sudden turn on his part, if he would expect any aid from medicine. Where no physical cause of the proclivity exists, or can be detected, it is of much importance to ascertain whether there be any deviation from the standard condition of health; towards general plethora in the one direction, or towards emptiness and asthenia in the other. The first of these unnatural states may be redressed by regimen and exercise; by abstinence from stimulating food and drink; by a slender diet also; and, if need be, by direct depletion. The second, which is the most common of the two, and which often leads (as I have explained before) to *local* plethora, may be removed or lessened by a tonic treatment. The object in both cases is to give stability and firmness to the nervous system; to diminish that mobility, or readiness to be impressed, which is so strong a characteristic of the class of patients affected with epilepsy, although it may not be very apparent in some few individuals among them. It is upon this principle, that *mineral* tonics sometimes do good in epilepsy, and not by any *specific* virtue which they possess in restraining the fits.

It is owing, perhaps, to a neglect of these two somewhat opposite conditions of general plethora and general debility, or to the difficulty which sometimes is met with in distinguishing them, that such a variety of opinions have been expressed concerning the proper treatment of habitual epilepsy. Plethora is to be reduced without causing hurtful debility: tone is to be given without inducing dangerous fulness. It requires some nicety to carry the balance even; to attain the hoped-for good, and at the same time to avoid the evil that is apt to wait upon it. In very many cases the requisite extent and measure of the tonic plan on the one hand, or of the lowering system on the other, can only be learned by careful trials. But sometimes the indications of treatment are more plain. When the patient is young and strong, and full of blood, and not of a particularly moveable temperament; when he has a hard pulse, and any degree of feverishness; when the disorder has supervened upon the suspension of some customary discharge, so that there is an obvious cause of plethora; and when the disease is in its early stage, and the recurrence of the fits has not yet been established by habit: in any or all of these circumstances it may often be proper to abstract blood from the patient, and it will always be right to purge him actively, and to insist upon an abstinent regimen. When former paroxysms have been preceded by signs of fulness of the vessels of the head — by headache for instance, throbbing of the temporal arteries, distension of the superficial veins, a flushed or loaded coun-

tenance—you may sometimes, by a timely use of the lancet or the cupping-glass, avert an attack that was apparently impending.

On the other hand, if the patient be pale and weak; or unduly susceptible; or if his malady have been fastened upon him through many repetitions of the fit; you will generally find that any form of active depletion is injurious, and learn to place your best hope in measures which are calculated to invigorate the frame.

One of the most useful of the particular remedies employed for strengthening the body, is the cold shower-bath. This tends, more perhaps than any single measure, to give permanent firmness and steadiness to the system. The best test, in all cases, of the tonic and bracing effect of this remedy, is the occurrence of a pleasant and general glow after each application of it. It is the only safe mode in which the cold bath can be used by an epileptic person.

You will find, in books, a great many tonic medicines recommended for this disease, which medicines you will have opportunity and ample time for trying. Of the mineral tonics, the salts of silver, zinc, copper, and iron, have been chiefly praised. The nitrate of silver used to be highly thought of; but there is one very serious objection to it which must never be forgotten: viz. that it is apt to produce a permanent discolouration of the skin, a frightful lead-colour. There is a footman in a house near Cavendish Square who has been thus blackened: and there is a gentleman of property resident at Brighton, in the same predicament; his face looks as if it had been thoroughly and carefully pencilled over with plumbago. A barrister, a friend of my own, had a narrow escape from a similar misfortune: in fact, his skin has acquired a just perceptible tinge of gray. Now, if the remedy were sure to cure the disease, I am not certain that every one would accept of a cure on such terms. It would be proper, even on that supposition, to tell the patient that though he (or, *a fortiori*, she) would get rid of the epilepsy, there was a likelihood that this unamiable complexion might ensue. But the truth is, that, in giving this nitrate of silver, we run a great risk of obtaining its disfiguring effect, for the sake of a very small chance of curing the epilepsy. I have been assured, by one of his friends, that the Brighton gentleman has carried a dark outside for a quarter of a century at least; and that he is as subject to epileptic fits now as ever he was. To do good, the lunar caustic must be given for some time together, and the probability is that it will not do good even then: and if it be given for some time together, there is great danger of its changing the colour of the skin. For these reasons I never give it myself, and therefore I cannot recommend it to you. If you wish to try it, or if you have a patient who insists on trying it, as some will, you may begin with half a grain in a pill three times a day; and the dose has sometimes been carried as high as fifteen grains. And it is worth observing that, in the larger doses, this drug proves purgative. It is possible that its good effect, when it has any, may be attributable to its operation in that way.

There is no danger of spoiling the beauty of your patient by administering the oxide or the sulphate of zinc, or the cuprum ammoniatum. The liquor arsenicalis has been thought useful, but it requires to be exhibited with great caution. Of all the metallic remedies, I prefer some preparation of zinc, or of iron. They appear to do good by giving what is called tone to the nervous system, and rendering it less prone to be affected by the slighter exciting causes of the disease. The salts of iron probably do this by improving the condition of the blood. I cannot pretend to weigh the merits of the long list of substances which have been lauded as efficacious in keeping off and curing the disease; and which, when they have been useful at all, have operated, I conclude, in diminishing the disposition to epilepsy, by corroborating the nervous system. The most renowned of them are valerian, assafœtida, wormwood, the mistletoe of the oak, the cardamine pratensis, rue, the cotyledon umbilicus, the sedum acre, indigo; narcotic vegetable preparations, stramonium, belladonna, hemlock, lettuce; animal substances, musk, castor, ox-gall; and the number might be many times multiplied. This long array of drugs, all of which have been known, or supposed, to accomplish a cure, affords, in truth, one of the strongest evidences of the intractability of the disease under any plan of treatment. There is a shrewd remark of Esquirol's, which I believe to be quite true, however difficult it may be to account for the fact, and which is, that epileptics are apt to improve for a time under every new plan of treatment.

Whatever drug you may see reason to select (and the patients will have drugs, and

you must be prepared to ring the changes upon them), there are certain other points in the management of the disease which are of essential importance. The patient who is subject to epilepsy should live by rule, and be temperate in all things. His diet should be simple, nutritious, but not stimulating. Except under special circumstances, he should renounce all strong liquor, and become, in the new-fangled and vulgar phrase, a tee-totaller. He should rise early, and take regular exercise in the open air; keeping his head cool, and his extremities warm. He should avoid all mental excitement, and the fatiguing pursuit of what is called pleasure; all probable sources of sudden anger, surprise, alarm, or deep emotion of any kind; all striving and contention of the intellect. The student, of whatever age and sort, in whom epilepsy has declared itself, should shut his books; the man of business, abandon or abridge his professional toil: at least they must be instructed to abstain habitually, in their respective callings, from such application as would task and strain their powers, whether mental or bodily; and endeavours should be made to engage their thoughts, and to interest their minds in less engrossing objects of attention. No minute rules can be laid down on these points, but, keeping the general indication in view, it will seldom be difficult to follow it up in practice.

When the fits appear to have been brought on by a species of moral contagion, or by imitation of the same disease seen in others, care should be taken to exclude as much as possible those objects or trains of thought which produce the mental emotion or the morbid propensity. In these cases, and, indeed, I may say in almost all cases, it is more rational to expect benefit from such measures as tend to calm the mind and fortify the nerves, than from this or that substance thrown at random into the stomach.

There is ground for believing that epileptic fits sometimes depend upon a syphilitic affection of the bones of the skull; I am much mistaken if I have not seen such cases. When that suspicion arises, it may be proper to give mercury a full and fair trial. Such a plan has been followed by success. I should always premise, however, in such cases, the iodide of potassium; the specific efficacy of which in dispersing syphilitic nodes is unquestionable. I am accustomed to recommend a gentle and long-continued course of mercury whenever organic disease of the brain is suspected; the influence of that remedy being carefully watched. It will be right and proper also to try the effect of counter-irritation; of blisters, a seton in the neck, or the croton-oil liniment. But I must confess to you that, often as this expedient is employed, I have seldom witnessed any such result from it as would encourage me to expect benefit from repeating it in another case. There is one form of counter-irritation which I have never seen put to the test, but which has of late been strongly recommended by a very able and observing physician, Dr. Pritchard; and of which I have heard very good accounts from a gentleman who had seen it extensively employed in Bristol; I mean the making a long issue in the head itself, dividing the integuments down to the bone by means of a scalpel in the direction of the sagittal suture, and keeping the incision open and discharging for some time, by means of issue peas. The formation of the issue is said to be not so painful as one might suppose.

Dr. Quain, in his edition of *Martinet's Pathology*, relates the following case:—"Some years ago I saw a boy who was epileptic from infancy, and who, in one of his usual fits, fell over a cliff by the sea side, and received a very severe lacerated wound of the scalp, which healed slowly and with a copious suppuration. While the discharge continued he was free from any epileptic attack; but as soon as the wound healed, the fits returned as before."

Twice I have seen similar good effects from the insertion of a seton in the neck. Twenty times that measure has disappointed my hopes.

When the disease is ascertained or believed to be of the eccentric kind, we must search diligently to find the seat of the distant irritation, in some disturbance of function; and apply our remedies accordingly. The irritation may be found, as I have already intimated, in almost any organ of the body. Painful or irregular dentition is perhaps one of the commonest of the eccentric sources of epilepsy. Sometimes the attacks are attended with symptoms of disease in the liver; slight yellowness of the skin, uneasiness and tenderness in the right hypochondrium, and lowness of spirits. In such a case we must rectify that state of the liver, by such means as I shall have to specify hereafter. If the disorder depend on a stone in the bladder, the

cure must be committed to the surgeon. I long had a patient under my occasional inspection, who from time to time suffered slight fits of epilepsy; on most occasions he passed about the same time a small calculus by the urethra. I make no doubt that in his case the exciting cause of the epilepsy lay in the kidney.

You will find that most persons, in respect to such diseases as that which we are now considering, have some favourite or usual mode of treatment; and if I were called upon to name any single drug, from which, in ordinary cases of epilepsy, I should most hope for relief, I should say it was the oil of turpentine. And I find that other physicians have come to the same conclusion. Dr. Latham the elder was, I believe, the first person who made known its efficacy in this disorder. Fovilla states that he has seen excellent effects from it. It is highly spoken of by Dr. Percival, in the Dublin Hospital Reports. It is not to be given in large doses, but in smaller ones, frequently repeated; from half a drachm to a drachm every six hours. You are aware that it sometimes produces strangury, and therefore the patient must be forewarned of this, or carefully watched. Occasionally turpentine has done good in virtue of its anthelmintic properties. I know that a physician of my acquaintance cured a case of epilepsy in this way, somewhat to his own surprise. Without having in his mind any notion of worms, he thought it might be well to purge his patient, who had laboured under epilepsy for some time, with the oleum terebinthinæ. The patient, who is the brother of a person holding at present a high office in this country, was residing two or three miles out of town. In the middle of the night the doctor was summoned to him in a great hurry; the messenger said he was supposed to be dying. He was only intoxicated, however, by the free dose of turpentine he had swallowed: the next morning he voided into the close-stool a large tape-worm; and he has never had epilepsy since. A nobleman residing in Cambridgeshire was long epileptic; and he too got rid of his epilepsy and of a worm at the same time. I believe that the cure was effected by turpentine in his instance also; but I am not certain of that. Such cases are remarkably interesting: they show that irritation of the stomach or intestines may be sufficient to cause the fits; they illustrate excellently well the eccentric form of the disease; and they deserve to be always borne in mind when we are asked to prescribe for an epileptic patient. A cure from so dreadful a complaint, by such simple means—the cause of his malady, and the certainty of his having got rid of that cause, being both so obvious and intelligible to the patient—may be enough, sometimes, to make a practitioner's fortune. But I think you will sometimes find the oil of turpentine very useful, even though it expels no worm, and when there is no worm to expel. If the bowels should be costive, the oil of turpentine and castor oil, in equal proportions, go exceedingly well together.

When the patient has a distinct warning of an approaching paroxysm, can anything be done to ward it off? Why, in some cases, by interrupting the precursory symptoms, it certainly may be prevented. A pupil of the class informs me that a brother of his, twelve or thirteen years old, has been subject to epileptic fits for two years. They occur in the night, especially if he be waked, even though the awakening cause may have no tendency to startle him. He often is dull and drowsy the evening before, and if his friends rouse him from this lethargic state by conversation or amusements, the attack expected that night sometimes does not happen. Another student knows a young girl, in whom the occurrence of very high spirits is always precursory of the paroxysm. When this extreme vivacity is moderated by those about her, the threatened fit is sometimes averted. I mentioned before an instance in which the aura, proceeding from one of the thumbs, was frequently checked by tying a ligature tightly round the thumb. Other examples of exactly the same kind are on record. Mr. Wardrop cured a case beginning with an aura in one finger, by amputating a joint of the finger. Dr. M. Hall states that the immediate accession of the paroxysm may sometimes be prevented by dashing cold water on the face, or by exciting the nostrils by snuff. In this manner the disposition to closure of the larynx, and to expiratory efforts, is exchanged for sudden acts of inspiration. Another patient of my own, an old college friend indeed, who is afflicted with epilepsy, feels convinced that he sometimes staves off a fit by applying smelling-salts to his nose: and he always carries a bottle about with him for that purpose; but unfortunately the warning (which consists chiefly in giddiness) is often so short, that he has not time to have recourse to his preventive before he falls down. It is a question whether the fit may not be ob-

viated by a strong mental effort in some cases. I make no doubt that it may, especially in the imitative form of the disease, which originates in, and depends upon, mental and moral causes.

It is scarcely necessary that I should do more than advert generally to those precautions which every one who is subject to epilepsy ought to observe, and which it is the business of his medical adviser to enforce, both upon the patient himself, and upon his friends. His bed should be large, and low; or if not large, it should be enclosed with some netting or other defence against his falling out of it. If he sleep in a room by himself, care should be taken that in the winter a proper temperature is kept up, for should he get out of bed in an attack, and remain upon the floor, he may be seriously injured by cold. He should not, however, be left alone if it can be helped. Guards should be placed over every fire-grate near which the patient may come. He should avoid ascending and descending stairs as much as he can. He should not ride on horseback; nor on the outside of a coach; nor even in a gig; nor go about, especially in solitary places, without an attendant. A patient of Dr. Cheyne's, a young man of twenty, was drowned in his own garden by falling forwards into a little runnel of water, which was not four inches deep. Neither, on the other hand, will it be proper or safe for him to frequent crowded or hot rooms; or the streets of a populous town, in which the multiplicity and distraction of objects are apt to produce, even in a healthy person who is not accustomed to them, a degree of vertigo and confusion. Dr. Cheyne advises that when the patient's circumstances will admit of his having a constant attendant with him, the latter should be provided with some diffusible stimulus: a potion, for example, composed of camphor mixture and æther, by the swallowing of which the impending paroxysm may sometimes be repelled.

LECTURE XXXVII.

Chorea. Symptoms; Pathology; Complications; Causes; Treatment. Chronic Chorea. Other nervous Disorders to which the same name has been applied.

ANOTHER disease of a spasmodic kind, and essentially belonging to the nervous system, is *Chorea* — *St. Vitus's dance*. This, in its ordinary form, is far less serious than the complaints which we have recently been considering; but it is always a very unpleasant disorder to suffer, and occasionally it assumes a very frightful and even a fatal form. It has several points of analogy with the other nervous and spasmodic ailments. Its prominent symptom is an irregular and involuntary clonic contraction of some of the voluntary muscles, which, however, are not wholly or constantly withdrawn from the government of the will. In tetanus we had rigid spasm, while the mind was clear and free; volition was unaffected, but the muscles which should have obeyed the effort of the will, were seized upon and mastered by some stronger overruling power. In epilepsy, with convulsive spasm, there was suspension of the mental functions: a temporary interruption of consciousness, and therefore of volition. But in *Chorea* we have a different state from either of these. There is no loss of consciousness; no defect of volition. The ordinary movements of the body can be performed in some degree, or sometimes, under the direction of the will; but it would seem as if some other power, thwarting the will, wantonly interfered to excite them when they are not needed, to render their action unsteady and imperfect, to arrest the natural movement, and give a new direction to the limbs, and to cause the patient to gesticulate and grimace like a Merry-Andrew. Moreover, these apparently absurd motions do not occur in paroxysms, but continue throughout the day, sometimes for weeks together; but they generally cease during sleep: for the most part, but not always, the agitated limbs are still, while the senses are shut up in slumber. The complaint is not attended with fever.

This disorder was first distinctly described by Sydenham, whose account of it is

very graphic and excellent, and has been copied by most subsequent writers. Without reference, however, to the portrait which he has left us, I will sketch the disease, as it has occurred under my own observation. It usually begins with slight twitches of a few muscles in the face, or in one of the upper extremities; and by degrees the spasmodic action becomes more decided and more general. All the voluntary muscles are liable to be affected by it. Those of the face seldom escape. The features are twisted into all sorts of ridiculous forms; you might suppose that the patient was what is called pulling a face, or making mouths at you: but there is neither mirth nor mockery in the contortion; it is a little convulsion. It is succeeded by a vacant look, and then it begins afresh. The disease occurs much oftener in young girls than in any other persons. If you ask the patient to put out her tongue, she makes sundry attempts to do so before she can accomplish it; and then the tongue is suddenly thrust out, and as suddenly withdrawn, and the jaws snap together as if she were resolved that you should have as short a glimpse of it as possible. She writhes and contorts her shoulders. She cannot keep her hand or arm for half a minute in the same position. Alternating movements are common. The hand is turned palm upwards upon her lap, and presently reversed; the fingers are extended, and again bent; the mouth and eyes are opened and closed without apparent purpose. When, at meals, she desires to carry her hand to her mouth, it is arrested midway, and suddenly pulled back again, or pushed off in some other direction; and it is only after many deviations and fruitless efforts that she succeeds. The lower extremities are apt to be similarly affected. When the patient intends to sit or stand still, her feet scrape and shuffle on the floor, or one of them is suddenly everted and then twisted inwards, or perhaps is thrown across the other; and if she endeavour to walk, her progress is most uncertain; she halts and drags her leg rather than lifts it up, and advances in a rushing or jumping manner by fits and starts. In short, the voluntary muscles are moved in that capricious and fantastic way in which we might fancy they would be moved if some invisible mischievous being, some Puck or Robin Goodfellow, were behind the patient, and prompted the discordant gestures. With all this the articulation is impeded: there is the same perverse interference with some of the muscles concerned in the utterance of the voice. By a strong figure of speech, the disorder has been called "insanity of the muscles."

Such is a picture of the main symptoms of this strange malady, as they have presented themselves to me; and such, I venture to say, you will often see in your future practice. You will find, moreover, that the jactitations are usually more marked and general on one side of the body than on the other: and sometimes they are confined to the muscles of one side. Here, therefore, we have a trait of resemblance to epilepsy and to hemiplegia. Again, the irregular movements always appear first, and are most decided in the upper extremities. Sometimes the lower limbs are not affected at all: they are never exclusively affected. Generally speaking, the disease is the more severe, and the more difficult of cure, in proportion to its extent, to the number of muscles implicated. I believe that in every fatal case hitherto recorded, the muscles of the whole body have been involved.

[Two cases of Chorea are referred to by Romberg (*Diseases of the Nervous System*, vol. ii. p. 55), and they were the only ones he had ever met with, in which the muscles of the external and internal respiratory apparatus were involved. In one of these cases, a child eight years of age, the choreic movements of the right half of the body were associated with dyspnoea, whistling inspiration, and palpitation. In the second case, a boy of eight years, only the muscles of the extremities, the face, and the eyes, were at first attacked; subsequently, the choreic movements ceased in these parts, and the muscles of respiration became affected. — C.]

It is a curious fact, but one which I have often ascertained, that when one limb alone happens to be thus agitated, if that limb be laid hold of, and kept still by main force, some other limb or part will take on the spasmodic action. The persons who are subject to chorea are always inordinately sensitive, and what is popularly called "nervous." They are easily stirred by new ideas and sudden feelings, and pass readily, and upon slight occasions, from one mood of mind to another. The mind is affected, as Dr. Cullen remarks, in the same way, and often shows the same varied,

desultory, and causeless emotions, as in hysteria. You see the indication of this nervousness in the fact that the fidgetty catching of the muscles increases when the patient is spoken to, especially by a stranger — by the physician, for example. The nurses of the hospital constantly tell me that such or such a patient, who has chorea, is much more composed at other times than she is during my visits, when she is surrounded by students, and made the object of their attention. In most cases the jacitations are partly and in some degree under the influence of the will. Sometimes the patient seems to give way to them, indulges in or exaggerates them: at other times she can, by making an effort, control them. Many of the patients, especially such as are old and intelligent enough to understand our directions, and to make the trial fairly, can suspend for some seconds the convulsive movements, by taking a deep inspiration, and resting upon it, without expiring, for a little while. Like other spasmodic diseases occurring in moveable constitutions, chorea is liable to be propagated also by a species of contagion, or rather of involuntary imitation. These diseases constantly approximate, and touch each other, in some of their characters.

Chorea, in this its standard form, is essentially a disease of youth. Sydenham and Cullen, who closely follows him, state that for the most part it attacks boys and girls, who have not reached the time of puberty; between the tenth and fourteenth years of their age. These limits are, however, too narrow. It is very common between the eighth and sixteenth years; a period which corresponds pretty accurately with that included between the second dentition and puberty. The disorder sometimes comes on as early as four or five; and now and then it begins in adult life, or in old age: the instances, however, in which a first attack occurs before the age of eight, or after the age of twenty, are comparatively few in number. I have already intimated that chorea is much more frequent in girls than in boys. Dr. Hughes has published, in the *Guy's Hospital Reports*, two instructive digests of cases treated at that hospital during a long series of years, and amounting in all to 309. Taking the aggregate of his lists, and of six others recorded by various observers, I find that among 1029 patients affected with this malady, 733 were females. This gives a proportion of nearly five to two. Yet, according to the tabular statements of Dr. Hughes and of others, the two sexes are equally liable to chorea before the age of nine.

[In 429 cases referred to by Dufossé and Rufz, 130 occurred in boys, and 299 in girls. — C.]

If I may trust to my own experience, it is much more common in children having dark hair and eyes, than in those of a light complexion; and I think I have seen the same remark in some book, but I forget where.

[According to Mr. W. H. Bell—(*Dict. des Etudes Médicales*) Dufossé (*ibid.*), and Rufz (*Archives Générales de Méd.*, iv. 239)—the subjects of chorea have chiefly light hair. — C.]

Temperature appears to influence its development. Dr. Hughes found it to be more frequent in the six winter months (reckoning October as the first) than in the six summer months, in the ratio of three to two. This tallies with Romberg's remark, that the disease becomes rarer as we approach the equator, and is almost unknown in the tropics.

When the disease is strongly marked, or lasts long, there is usually some imbecility of mind manifested; a slight degree of fatuity, and a foolish expression of the features. But this goes off with the other symptoms. The child generally recovers, but the malady is apt to recur, and that more than once. In this respect we may trace a distant resemblance to epilepsy: if we regard each attack as a long and mild paroxysm, then these paroxysms are liable to repetition. No doubt the duration of the disorder is often abbreviated by proper treatment: there are cures in this disease as well as recoveries.

In its milder form, which fortunately is by much the most frequent form also, the grimaces and gesticulations of the patient are apt to be regarded by thoughtless lookers-on as objects of mirth and amusement; but in its severer and fatal form there are few diseases more terrible to witness. Sleep is obtained with difficulty; and in

the waking state no intervals of calm arrive, such as divide the fearful paroxysms of tetanus or of hydrophobia. In spite of all care and protective contrivance, the loins, hips, and elbows of the unhappy patient are chafed and inflamed under unceasing friction with the bed-clothes; the limbs, in their perpetual contortions, are bruised and wounded; the bitten lips bleed; the countenance Dr. Hughes describes as wearing a piteous and imploring expression; occasionally an involuntary cry or squeak is heard, until at length the vital power is exhausted, and death comes to the sufferer's relief.

It is my good fortune never to have had a fatal case of chorea to treat, and I have seen but one. The subject of it was the patient of one of my colleagues, in the Middlesex Hospital. Under any circumstances, death from this disorder is uncommon: it is still more uncommon during childhood. Of 16 deaths attributable to the disease itself, and occurring among Dr. Hughes's 309 cases, 12 were of patients who were near, or had reached, or had passed the period of puberty; their ages varying from 15 years in the female, up to 25 in the male.

Inspection of the dead body, in the cases that have proved fatal, has thrown no light upon the pathology of the disease. We shall seek in vain, I believe, to discover the nature of chorea through the revelations of morbid anatomy. In the patient who died in our hospital, great vascularity of the uterus was noticed; there were earthy concretions in the pancreas, omentum, and mesentery, and tubercles in the lungs. In a fatal case that fell under Dr. Bright's observation, the uterus and its appendages were diseased. Dr. Hughes informs us that, of 14 cases examined after death *from chorea*, disease of some kind was observed within the cranium in 10, while the brain and its membranes appeared quite healthy in 4. In 6 of these 14, the spine was not opened; the cord was found healthy in 2; and softened in 2; and otherwise diseased in itself or its membranes in the remaining 4. Of 16 cases examined after death *during the existence of chorea*, the organs within the thorax were not mentioned in 2; were reported healthy in 2; and were more or less diseased in 12. In 11 of these 12, the sigmoid or the auriculo-ventricular valves of the heart were in a diseased condition. In 5, if not in 6, of these cases, the disease consisted in vegetations upon the edge of the mitral valve; and in 1, similar vegetations were present upon the tricuspid valve.

The abdominal organs were healthy in 4 of the 16 cases; the liver was said to be large, or congested, in 6; the kidneys to have been congested in 4; and the stomach to have been ulcerated in 2.

Of 7 females who had arrived at or near the age of puberty, the organs of generation were not noticed in 3; the ovaries were turgid, or congested, or evidence of undue uterine excitement existed, in 4.

What inferences may fairly be drawn from statements such as these? Some of the morbid conditions revealed by dissection had probably no connexion with the chorea at all. Some may have aided its accession by increasing that irritability and mobility of the nervous system, which subjects it readily to the exciting causes of various nervous ailments: or (as I most incline to believe) they may have operated upon afferent nerves of the spinal cord as eccentric exciters of the irregular movements, in constitutions already predisposed to chorea. This last supposition derives support from the fact that chorea affects some women during their pregnancy, and ceases spontaneously after parturition. Dr. Lever has recorded five instances of this kind; and Dr. Hughes refers to two others.

A very remarkable part of the summary which I have just quoted from Dr. Hughes is that which concerns the *heart*. It has long been observed that some occasional but obscure relation subsists between chorea and acute rheumatism. During the presence of the latter disease, the lining (as well as the investing) membrane of the heart is very liable to inflammation, by which its valves are permanently injured: and such valvular injury almost always gives rise, in the living patient, to some cardiac murmur. Now, valvular disease of the heart was found to exist in eleven of the twelve cases (the whole number being but sixteen) in which the heart was any way implicated: and in another part of his report, Dr. Hughes says that "out of 104 cases in which special inquiries were made respecting rheumatism and heart affections, there were only 15 in which the patients were both free from cardiac murmur, and had not suffered from a previous attack of rheumatism."

Making large allowance for the possibility that, in some instances, the cardiac murmur may have been due simply to the anæmia which is so common in chorea, there is convincing evidence in these statements, of a frequent though not a necessary connexion between that disease and acute rheumatism.

On former occasions, I have been in the habit of mentioning two conjectures which had occurred to me on this subject. Rheumatism (as we shall see by-and-by) is especially a disease of fibrous structures, and it usually invades various fibrous parts at the same time. I deemed it, therefore, not improbable that in the cases in question, some morbid condition of the membranes of the spinal cord might have arisen simultaneously with the cardiac inflammation. Of this, however, we find no evidence in the teachings of morbid anatomy. Again, the cardiac injury might, I thought, like some other structural diseases, be an eccentric exciting cause of the spasmodic disorder of the muscles: and this appears to be Dr. Hughes's opinion. But the most probable theory is that suggested by Dr. Todd and by Dr. Begbie. Acute rheumatism is a blood-disease; and it is most likely that the unhealthy blood, circulating through the several organs, is the common source and cause of the articular, the cardiac, and the spinal symptoms; and the bond of connexion between them. This theory explains also the occurrence, which Dr. Begbie has noticed and recorded, of chorea in some, and of acute rheumatism in others of the same family.

From an analysis of 36 cases in which chorea was observed in connexion with articular rheumatism alone, with acute cardiac disease alone, or with these two disorders in combination, Dr. Kirkes deduces the following conclusions. That choreal symptoms are oftener associated with endocardial than with pericardial disease; and that they are more likely to arise when articular rheumatism and disease of the heart exist together, than when either of those affections occurs separately.

There is a speculation of some of the French writers respecting the seat and nature of chorea so ingenious, that I cannot refrain from mentioning it.

It is held by modern physiologists, as you probably know, that one of the functions, the principal office indeed, of the *cerebellum*, is to preside over and regulate the faculty of locomotion; to keep the muscles in due subordination, as it were, to the will. No voluntary movement, almost, can be executed without the combined and consenting action of many muscles: it is the business of the cerebellum, they say, to maintain this consent and community of purpose; to prevent any mutiny of individual muscles, and to make them unanimously co-operate in producing an intended movement. How far this doctrine may be true I do not now inquire; but supposing it well founded, then they very ingeniously assign the cerebellum as the seat of that change, whatever it is, which gives rise to the phenomena of chorea. And it is most certain that the irregular movements by which chorea is characterized can neither be considered as the effects of imperfect paralysis, as some have stated, nor of convulsion, in the proper sense of that word, as others have asserted; but rather as consequences of the want of due harmony and agreement between the various muscles, which should combine to produce the desired state either of rest or of motion. There is a defect of the requisite association in the actions of the different muscles; and it is in this sense that chorea has been denominated *insanity* of the muscles. There is a certain portion of the brain which ministers to the intellectual functions; there are certain altered states of that portion, which lead to mental aberration; the persons so affected form false judgments; cannot associate their ideas aright. So also there is a certain portion of the encephalon which presides over the locomotive functions; and there are altered states of that portion, which lead to a loss of the due association of the muscular contractions. That portion is the cerebellum. Such is their theory; and it is a very plausible and pleasant, but withal an unsatisfying theory. The disorder really belongs, I apprehend, to the excito-motory department of the nervous system. From some infirm or unnatural state, either of the cord or of the incident nerves that convey impressions to it, its reflex function is called into irregular play, and voluntary muscles contract independently of volition. Sometimes, at the same instant, the patient *wills* certain definite movements through the instrumentality of the very same muscles. But the authority of the will is impaired, and the automatic motions are proportionally strong and unruly. The consequence is, that the same muscles, receiving at the same time contradictory orders from these two sources, obey neither

mandate completely, but give rise, by their discordant action, to the grotesque and seemingly antic gestures which these patients exhibit.

But to leave these seductive theories, and to return to duller matters of fact. Chorea is a complaint that is seldom attended with any bodily pain. I have in several instances, however, known it to be accompanied by pain of the head; and in some of them, with pain on that side *only* of the head which was opposite to the agitated limbs. I mention this as being of some practical importance; for I have found the disease to become sensibly less severe, and very soon to cease, upon drawing blood by leeches, or cupping, from the painful side of the head. In a greater number of cases, however, no such pain is experienced. Sometimes you will find that in all respects, excepting the nervousness, and the irregular movements, the patient is in the enjoyment of perfect health. But neither is this very common: generally there is something manifestly wrong in the state of the stomach and bowels, either before or during the complaint; a capricious appetite, costiveness, a tumid abdomen, offensive breath, a foul tongue: or irregular menstruation.

Probably anything which makes a forcible impression upon the nervous system may act as an exciting cause of chorea. Strong mental emotion, or a sudden mental shock, is very likely to bring it on, in those of a moveable constitution who are predisposed to it. Of its ascertained or alleged exciting causes, *fright* is beyond all comparison the commonest. And, what is very curious, fright has been known to effect its cure. Dr. Hughes tells of a girl who having recovered from chorea, and suffered a relapse, was on her way to Guy's Hospital, for the purpose of seeking readmission there. As she passed over London Bridge, she was terrified by seeing a person knocked down and run over. Before she reached the waiting-room of the hospital, her malady was gone.

The disease has been observed to follow blows and falls on the head; but even in these cases the alarm may have had a greater share in producing it than the blow itself. It sometimes seems to depend upon irritation of the stomach or bowels, by improper diet, by accumulated fæces, or by worms; and it is found to be connected, in not a few cases, with difficult and painful menstruation. It frequently begins about the period of the second dentition: the late Dr. Gregory, of Edinburgh, was in the habit of relating instances of that kind. In one case, the old teeth were remaining while the new ones were appearing by their sides. The old teeth were drawn, and the removal of the chorea was complete. This Dr. M. Hall would justly call eccentric chorea. But even in such cases the state of the gums cannot be regarded as the sole cause of the chorea: there must be the predisposition, as well as the accidental exciting cause; for the complaint is apt to recur under the agency of some new irritation, and may then be removed by other means.

[We have not found the disease to be much influenced by the season of the year or the condition of the atmosphere. Dugès, Rufz, Spangenburg and Blache state that it occurs most frequently in summer. According to the statements of Rochoux, Chervin, and Danste, it is a rare affection in the southern hemisphere. It is not a very frequent disease in Philadelphia.—C.]

Chorea, such as I have been describing it, may last from a week or two to some months. The disorder often terminates—at any rate much more often than epilepsy does—at the period of puberty; especially upon the first coming on of the menstrual discharge in the female.

[Chorea, like epilepsy, according to Andral, may be excited by irritation; the fact, however, is denied by Rufz and Blache.—C.]

I had occasion, in the last lecture, to remark, that when a vast number of different drugs are recommended as specifics in any given disease, we may sometimes infer from that very circumstance that the disease is difficult of cure, and generally intractable under all plans of management. But there is another class of diseases which a variety of drugs are supposed capable of curing,—those, namely, which tend to terminate in health. I believe that many cases of chorea—most cases—would at length get well without any aid from physic: I believe also that many of the boasted spe-

cifics have been quite innocent of any share in the recovery of the patients to whom they were administered; at the same time I am quite certain that treatment has a great influence over the disease.

It was Sydenham's practice first to bleed and purge his patients, and then to administer bitters, aromatics, and antispasmodics, with the view of strengthening the nerves. After his time the blood-letting and purgatives fell into disuse, until the publication of Dr. Hamilton's well-known work again brought the latter deservedly into favour.

The treatment of chorea embraces two definite objects. The first, and chief, is to give stability to the unduly moveable nervous centres. The second is to remove or avert whatever may be likely to produce unnatural excitement of the incident nerves.

Now the complaint in its genuine form is seldom dependent upon any organic or inflammatory change. The instrument is not broken anywhere; but slackened, jangling, and out of tune: and (to pursue the metaphor) we often can restore its harmony by bracing it up again.

I can confidently recommend you to abstract blood locally in those cases in which there is a fixed pain in the head; but with this exception, blood-letting is neither useful, nor even (in my opinion) justifiable. There is oftener a deficiency than a redundancy of red blood in the system.

I shall not attempt to distract your attention by discussing the various remedies that have been vaunted against chorea; but shall take the liberty of referring you to books (to Dr. Copland's *Dictionary*, for example) for further information on that subject, and content myself with telling you what modes of treatment I have been in the habit of employing, with very satisfactory results. I think, then, setting aside the complication with headache just mentioned, you will be able to deal successfully with most of the cases of chorea which you may have to treat, if you have at your command purgative medicines, the shower-bath, preparations of iron and of arsenic, and the oil of turpentine.

It will be right, in all cases, to begin by clearing out the bowels with calomel and jalap, or some active aperient; and you should persist in the regulated use of purgative medicines, if they continue to bring away much or foul fecal matter. You are to be guided less by the amount of the doses than by the effects they produce; at any rate one full evacuation of the bowels should take place every day. But though purgatives are good auxiliaries, we cannot trust to them alone for the cure of the complaint.

One of the most effectual of the tonic remedies is the cold shower-bath. If the patient be of a feeble constitution, the water may at first be used tepid; by degrees it should be used cold. This remedy should be employed every morning, or every other morning, early, as soon as the patient gets out of bed. Of the best indications of the propriety of its continuance I spoke in the last lecture only; I need not tire you, therefore, by repeating the observations I then made.

With this external tonic it will be right to combine some internal one; and for the most part, the best for the purpose is some preparation of iron. The carbonate of iron is an exceedingly good form, and it may be given in the way recommended by Dr. Elliotson, one of whose *pets* it is—namely, mixed with twice its weight of treacle, so as to form an electuary. You may begin with it in half-drachm doses, and presently increase the quantity to a drachm, or a drachm and a half, or two drachms. Much larger quantities indeed have been given, and that for a long time together; but I am not in the habit of so pushing this drug. Patients do not like to swallow from half an ounce to an ounce of the powder and twice as much treacle three or four times a day; and some of them cannot get so much down. And I mentioned on a former occasion that the iron is apt to accumulate in the large intestines, and to be expelled at last, often with difficulty and pain, in large, hard, red masses, like what is called, I fancy, slag, or the dross of iron ore from a furnace. The treacle may be dispensed with if you employ the *Ferri carbonas cum saccharo* which has been introduced of late into the London pharmacopœia. When one or at most two drachms of the carbonate given three or four times a day, make no impression on the disease, you had better change the form of the medicine. Give two or three grains of the sulphate of iron for a dose, or half a scruple of the citrate, or an ounce and

half of Griffith's mixture (*mistura ferri composita*), or twenty or thirty minims of the *tinctura ferri muriatis*.

[We have seen the best effects result from the use of the ammoniated tartrate of iron in five grain doses, repeated three or four times a day or oftener, according to the age of the patient and the extent of the choreic symptoms. — C.]

Dr. Bright says he has found the sulphate of zinc answer when the carbonate of iron had failed, and the iron succeed when the zinc had done no good. One most severe case, about which I was consulted, and which had resisted other remedies, got well under the use of the sulphate of zinc; the dose of which was gradually increased to ten grains, given three times a day. Whenever the medicine was pushed beyond this point it became emetic. This seems to be the favourite remedy in Guy's Hospital, where the dose has sometimes been carried, Dr. Hughes informs us, as high as half a drachm, two scruples, and even in one instance forty-two grains, thrice daily. It cured forty-five out of sixty-three cases; five in every seven.

[The valerianate of zinc, in doses of from three to five grains, according to circumstances, will often arrest the disease with great promptness. — C].

Certainly the disease is often very obedient to arsenic; but, for plain reasons, it is better to effect a cure, when we can, by less hazardous substances. The gravest case I ever had to treat occurred in one of my hospital patients. I tried the carbonate of iron in vain. The shower-bath so terrified and agitated the girl that I could not persist with it. I then gave her arsenic, under which she improved at first, but it ultimately was very injurious; her bowels were greatly irritated by it, she became paralytic in her lower extremities, and sank into a typhoid state; and I really was afraid that I should lose her. But she recovered from this condition, which I could not but ascribe to the arsenic; and as soon as I dared venture, I began to give her the muriated tincture of iron, twenty drops at a time, every six hours. Under this treatment she steadily and rapidly improved, and was soon quite well.

Dr. Begbie, in an experience of nearly thirty years, has never known arsenic fail. He gives five drops of the *liquor potassæ arsenitis* twice a-day, an hour after meals, adding one drop every third day, until the specific effects of the mineral upon the system begin to be observable: when he withdraws it for a while. "The earliest manifestation of these effects are itching and swelling of the eyelids, redness of the conjunctiva, nausea, and uneasiness at the pit of the stomach, and particularly a peculiar white silvery appearance of the tongue, seldom accompanied with tenderness."

The oil of turpentine also is certainly a valuable medicine in this disease; whether there be worms at the bottom of it or not. When the bowels are torpid and the girl is of that age when menstruation may be conjectured to be at hand, its arrival seems sometimes to be accelerated, and great relief to be produced, by the turpentine. The best way of exhibiting it in such cases is in combination with an equal quantity of castor oil; two drachms or half an ounce of the *mixture* may be given every morning, or every other morning, according to its effect upon the bowels; and when they are very sluggish, or the stools are unnatural, it will often be serviceable to give a couple of grains of calomel also, twice or thrice a week, at bed-time.

[Very decided testimony has been presented by Young, of Pennsylvania, Lindsly, of Washington, Hildbreth, of Ohio, Kirkbride and Professor Wood, of Philadelphia, Beadle of New York, and other American physicians, in favour of the efficacy of the *cimicifuga*, or black snake-root, in cases of chorea. It may be given in the dose of half a teaspoonful of the powdered root three times a day; or from one to two drachms of the saturated tincture, or a wineglassful of the decoction. The cyanuret of iron, in the dose of three grains three times a day, in the form of a pill, has been strongly recommended by Dr. Zollickoffer, of Maryland. The cyanuret of zinc, in the dose of one-third of a grain, twice a day, gradually increased to fourteen grains in the twenty-four hours, has recently been highly spoken of by the physicians of Berlin and elsewhere. See *Condie on Diseases of Children*. The *nux vomica*,

either in the form of extract or tincture, in as large doses as can be safely given has in our hands proved a valuable tonic in cases of chorea. — C.]

It is scarcely necessary for me to say that due attention must be paid to the diet. This ought to be plain and simple, but at the same time nourishing, and even generous. Exercise, short of that which produces fatigue, in the open air, in fine and dry weather, will also conduce much to the patient's recovery. And all kinds of immoderate *emotion* should be guarded against: for the contest often seems to lie between the emotional and the voluntary impulses to action. The stillness of the muscles during sleep is in accordance with this belief.

There is an affection (it scarcely deserves to be spoken of as a disease) which is sometimes called chorea, of a chronic nature, and resembling the disorder I have just been speaking of, inasmuch as it commonly is met with in nervous persons, and consists in the irregular, unmeaning, and involuntary contraction of certain muscles, especially in the limbs, neck, or face: but differing from it in this, that the same muscles are always affected, and in the same way; that it lasts long, almost always for life, and implies no accompanying derangement of the general health. In its slighter form the irregular movements are rather *awkward tricks* than spasms: little shakes of the head, or rapid and repeated elevations of the eyebrows, or corrugations of the nose, or sniffings and snortings through the nostrils, or shrugs of the shoulders — movements of which the person seems scarcely conscious. At other times, however, the motions are more extensive; a limb starts out, or the head is turned awry; and the individual who performs these evolutions is quite aware that he does so, and vexed and annoyed at the ridiculous figure he makes, but he cannot help performing them; or if he can prevent it, the necessary effort is worse than the disease. One young man who was subject to this infirmity told a friend that he could stop the movement by a strong exertion of the will; but *that exertion* was extremely painful, and was followed by languor and much discomfort. In some instances I make no doubt that the continuance of the affection is the result of a long-established *habit*. It occurs more frequently in men than in women. I had for a long time, as an out-patient at the hospital, a girl about seventeen years old, in all other respects the picture of health, but who was annoyed by an involuntary shake of the head, which took place two or three times in a minute. She received no benefit from medicine. A lad in my own service was affected in a similar manner. He seemed to be giving me, and my friends, from time to time, a familiar nod: and I was obliged to part with him. Others are subject to twitchings of the face. I am acquainted with one gentleman who is perpetually wrinkling his nose; and he has assured me that he was subject, when young, to an involuntary shake of the head, like the two persons just mentioned; but a blister having once been applied to his throat for some disorder in his air-passages, the shaking of the head was thereby rendered painful and difficult, and the movement there ceased: but (as he expressed it) it broke out in his nose, where it triumphs to this day. This *chronic chorea*, as it has been called, I merely mention to prevent your confounding together two affections which, though they have received the same name, and are in some respects analogous, yet differ in still more points, and those points of more importance. I believe that medicine has no power over any of these tricks. They are distressing and unsightly; but in no way dangerous.

The word chorea, which you know signifies a *dance* — and the trivial term, St. Vitus's *dance* — are not very appropriate to *either* of the modifications of the nervous affection which I have been noticing. In fact that term was originally applied, and much more suitably, to another set of symptoms of a most singular kind, concerning the real occurrence of which we might well be sceptical, if we had not authentic narratives of many instances of such disorder from different persons of credit, as well in this country as in others. What has happened many times before, may happen again; and you ought not to be in ignorance of the histories to which I allude. They relate to an affection characterized by movements that cannot be called spasmodic, but are rather owing to an irresistible propensity to muscular action, increased sometimes to a sort of mania by the force of imitation, or by the sound of music. It is the *voli-*

tion that, in these cases, is morbid and perverse. You might fancy the patient to be possessed and coerced by an evil spirit, like the *δαίμονιζόμενοι* of the Gospel history.

Some of the subjects of these extraordinary affections, impelled by a strange and unintelligible necessity, execute measured and regular movements with surprising energy, rapidity, and perseverance. When music is performed in their hearing, the movements become an actual dance; and where crowds are collected together, the dancing mania is apt to spread from person to person by a sort of imitative infection; realizing the fable of Orpheus, and giving origin (it may be presumed) to those romantic legends met with in the literature of most ages and countries, of universal, involuntary, and unceasing saltation, at the sound of a magic pipe. To these feats the term *chorea* is apposite enough. Indeed I have seen it somewhere suggested that the phrase *chorea Sancti Viti* is but a vulgar corruption of *chorea Sancti inviti*; and took its rise in the misfortune of some holy person who chanced to be afflicted with one of these unwilling but invincible impulses to caper. The common explanation makes this holy person to have been a certain German Saint Weit, to whom a chapel is said (I know not with how much truth) to be dedicated at Ulm, in Suabia.

Sometimes, instead of dancing on their feet, these patients drum and beat with their hands, either upon their own knees, or upon the objects near them. This variety has received the bombastic title of "malleation." Sometimes they circumsolve with great rapidity; or they turn their heads repeatedly from side to side with great velocity: this is "rotation." When they are irresistibly impelled to move in a given direction, the term "propulsion" is employed. The very invention of these names attests the reality of the disorder.

You will find one of these singular cases related by Mr. Kinder Wood in the seventh volume of the *Medico-Chirurgical Transactions*.

The patient was a young married woman. After having suffered severe pain in one side of her face, she began to be troubled with involuntary movements. They commenced in the eyelids, which were opened and shut with excessive rapidity. Then the muscles of the extremities became affected. The palms of the hands were beat rapidly upon the thighs, and the feet upon the floor. The motions soon extended to the trunk and pelvis. The patient was suddenly half raised from her chair, and instantly reseated. This was repeated as quickly as one action could possibly succeed another. Sometimes she had a propensity to leap upwards, and strike the ceiling with the palm of her hand; or to touch little spots or holes in the furniture of the room. Or she would dance on one leg, holding the other in her hand. These attacks were accompanied by headache, sickness, and vomiting. At last she took to making steps about the room, regulated by an air, or by a series of strokes on the furniture as she passed, her lips moving as if words were articulated, but no sound escaping them. A person thinking he recognised the tune which she beat on the furniture, began to sing it; and she danced directly up to him, and continued dancing till he was out of breath. A drum and a fife were now procured, and the same air played upon them. She immediately danced up to the drum, and as close to it as possible, till she missed the step, when the motions instantly ceased: and this was found always to be the case. The motions stopped also when the measure was changed; or was increased in rapidity beyond her power to keep pace with it. A continued roll on the drum had likewise the effect of putting an end to her movements. This being discovered, their approach was watched; and by always rolling the drum as soon as they threatened to begin, the chain of association which seemed to constitute the disease was at length broken. The bowels were in an unnatural state during the complaint; and the menstrual discharge appeared on the evening of the day on which it ceased. One might conceive the conduct here described was an indication of folly or of insanity; but Mr. Wood declares that the patient's spirits were good, and her perception and judgment accurate and just; that during the absence of the paroxysms she went about her household affairs as usual; and that she had a correct knowledge of her situation, and of the advantage she derived from the drum, with an anxious desire to continue its use. She stated "that there always was a tune dwelling upon her mind, which at times becoming more pressing, irresistibly compelled her to commence the involuntary motions."

In a lady, whom Dr. Abercrombie saw, the following symptoms, among others,

occurred:—After she had been ill with various nervous affections for two years, she began to suffer convulsive action of the muscles of the back, and involuntary twitches of the legs and arms, producing a variety of movements of the whole body very difficult to describe. These were much increased by touching her, especially on any part of her back. This is a symptom quite in conformity with Dr. Hall's doctrine of eccentric irritation. At one time there was difficulty of deglutition, so that attempts to swallow produced spasms, resembling those of tetanus. At other times, after lying for a long while quiet, she would in an instant throw her whole body into a kind of convulsive spring, by which she was jerked entirely out of bed: and in the same manner, while sitting or lying on the floor, she would fling herself into bed, or would leap, as a fish might do, upon the top of a wardrobe fully five feet high. These are feats that surpass the powers of a person in health: and I say we should hesitate to believe them if they were not related by a physician of such sober judgment and unquestionable veracity as Dr. Abercrombie. He tells us that during the whole of these symptoms her mind continued entire: and the only account she could give of her extravagance was, a secret impulse which she could not resist.

But, after a time, motions still more wonderful commenced, affecting the muscles of the upper part of the back and neck, and producing a constant semi-rotatory motion of the head. This sometimes continued without interruption night and day for several weeks together: and if the head or neck were touched, the motion was increased to a most extraordinary degree of rapidity. These paroxysms were relieved by nothing but cupping on the temples to the amount of ten or twelve ounces, when the affection suddenly ceased, with a general convulsive start of the whole body. She was then immediately well, got up, and was able to walk about in good health for several weeks, when the same symptoms returned, and required a repetition of the same treatment. All this went on, at intervals, for four years; the menstruation during that time being irregular and scanty, and the bowels torpid. She was pale and bloodless from the frequent bleedings, but not reduced in flesh. At last, in the spring of 1829, she had a severe paroxysm of the rotatory motion of the head; and it was then determined to allow the attack to take its course, and to direct the treatment entirely to the menstruation. Sulphate of iron, and Barbadoes aloes, were prescribed. She went on for three weeks, the convulsive motion of the head continuing without intermission night and day. At length, in the middle of the night, the paroxysm ceased in an instant, with the same kind of convulsive start of the whole body with which it used to cease after cupping. At the same instant menstruation took place in a more full and healthy manner than it had done for many years. From that time she remained well; at least up to the period when Dr. Abercrombie wrote the account.

The alternating rotatory motion of the head is by no means an uncommon feature of these singular cases. It occurred in a patient of Dr. Conolly's; in whom the menstruation was irregular, and about to cease altogether. It came on in paroxysms which were repeated many times a day, and it was attended with inordinate loquacity. The head was turned from side to side about eight times in a second, and each paroxysm lasted three or four minutes. The patient got well after being cupped and leeches, and thoroughly purged. I have seen precisely the same thing in a hospital patient. Dr. Crawford met with an instance of involuntary rotation of the head, without pain, but attended with intolerance of light. And there is a striking example of it described in the twenty-third volume of the *Edinburgh Medical and Surgical Journal*, by Mr. Hunter, of Glasgow, who speaks of it under the name of "rotatio or chorea." The motions are said to have been furious and alarming: they were executed with such extreme rapidity, that it was difficult even for the eye to follow them. She appeared, Mr. Hunter says, absolutely to be looking backwards and forwards, and in every direction, at the same moment. This woman had sometimes fifty paroxysms of this kind in a day: they greatly exhausted her; but she was perfectly rational in the intervals. A modification of the same kind of affection took place in a most extraordinary case recorded by Dr. Watt, of Glasgow, in the fifth volume of the *Medico-Chirurgical Transactions*. His patient was a girl ten years old. First she had headache, accompanied by vomiting, and increased by the slightest deviation of the body from the erect posture, either backwards, or forwards, or to one side. These symptoms lasted about a month; and during that time she lost the power of speech and of walking. At the end of that period she was seized with a propensity

to twirl round on her feet, like a top, with great velocity, always in one direction; and was pleased when those about her assisted in increasing the rapidity of her movements. After continuing nearly a month, these motions ceased, the headache returned, and she became unable to move her neck, or support her head. Soon after she was visited with a new kind of motion: she would lay herself across the bed, and turning over like a roller, move rapidly from one end of it to the other. At first the fits of this kind lasted two hours; but they gradually extended to six or seven hours every day. On being carried into the garden she rolled rapidly from one end of a gravel walk to the other; and even when laid in the shallow part of a river, though apparently on the point of being drowned, she began to turn round as usual. The rotations were about sixty in a minute. She made little or no use of her arms in revolving. In about another month or six weeks an entirely new set of movements began. She lay upon her back, and by drawing her head and heels together, bent herself like a bow, and then allowing her head and heels to separate, her buttocks fell with considerable force upon the bed. She repeated these movements ten or twelve times in a minute, first for six hours daily, and at length for fourteen. After another space of about five weeks had elapsed, the most singular freak of all ensued; she became possessed with a propensity to stand upon her head with her feet perpendicularly upwards. As soon as the feet were elevated in this manner, all muscular exertion seemed to be withheld, and the body fell down as if dead; her knees striking the bed first. This was no sooner done than she instantly mounted up as before; and continued to do so from twelve to fifteen times in a minute, for fifteen hours a day. After a variety of fruitless treatment, a spontaneous diarrhœa came on, and she recovered.

The spinning motions observed during a part of this case have been observed in other instances.

In Magendie's *Journal de Physiologie*, the two following singular forms of disease are referred to. A man, after some other symptoms of cerebral disorder, was seized with an irresistible inclination to move forwards, stopping only when exhausted. He would sally forth into the streets, and continue walking straight forward until he dropped down from fatigue, and was obliged to be brought home in some conveyance. This man at length died, and several tubercles were found in the anterior hemispheres of his brain. Dr. Laurent, of Versailles, exhibited to the Academy of Medicine a young girl, labouring under the exactly opposite necessity. In the attacks of a nervous disease she was irresistibly propelled backwards, and with some rapidity: being unable to avoid obstacles or hollows, she received many falls and bruises in her course.

I say that histories such as I have been giving you some samples of, and those mostly in an abridged form, would sound very like romances, if they were met with in the old authors alone, or if they were not attested by unimpeachable authority. They resemble chorea in this respect, that they are examples of muscular actions performed by persons in possession of consciousness, and performed in spite of themselves. But in most other respects they differ from what we now-a-days mean when we speak of chorea. Perhaps they may rank among *hysterical vagaries*. It is remarkable that the majority of them occur in young women, in whom the menstrual function is suspended or irregularly performed. Some persons may consider them as varieties of insanity. The patients certainly did not *feign* to be ill, for the feats of strength and agility which many of them enacted were much beyond their natural power and endurance. The truth seems to be, that there are innumerable modifications of the nervous functions, and that some of them are more common and more capable of being arranged into groups than others; but that they all offer points of resemblance, like (as I observed before) the different members of a large family, in which the individuals have the same general cast of features, and yet preserve each his particular identity.

I advert to these odd forms of disease with the view of directing your attention to such of them as may come in your way. We are yet terribly in the dark about morbid affections of the nerves, both organic and functional. Hereafter some medical Newton will arise, and reduce all these apparently complicated phenomena under one simpler law. At present all that we can do is to collect, and, as far as we may, to arrange facts, in the hope that at length some better light will be shed upon the subject. And it must be observed that some of the modern researches into physi-

ology do throw a little glimmering of illumination into these dark corners of pathology.

In certain of M. Magendie's experiments on animals the following curious facts were ascertained:—When a vertical section of the cerebellum of a rabbit was made, leaving one-fourth of the whole adhering to the crus of the right side, and three-fourths to that of the left, the animal rolled over and over incessantly, turning itself towards the injured side. The same phenomenon occurred upon the division of the crus cerebelli. The animal lived for eight days, and continued during the whole of that time to revolve upon its long axis, unless stopped by coming in contact with some obstacle. How like is this to the symptoms exhibited at one period in the girl whose case is related by Dr. Watt! Nor is Dr. Watt's case a singular one; M. Serres has described another much resembling it. A shoemaker, sixty-eight years old, of intemperate habits, after one of his debauches exhibited a kind of drunkenness which surprised his friends. Instead of seeing objects turn round him, as a drunken person is apt to do, he thought he was himself turning, and soon began to revolve; and this lasted till he died: and when his head was examined, extensive mischief was found in one of the peduncles of his cerebellum.

Again, M. Magendie noticed that when the upper part of the cerebrum is gently removed in birds and mammalia, they become blind; but no affection of the locomotive powers is produced. No further result is occasioned by the removal of a portion of the grey matter of the corpora striata: but when the striated part is cut away, the animal immediately darts forward with rapidity, and continues to advance as if impelled by some irresistible force, until stopped by an obstacle; and even then it retains the attitude of one advancing. The experiment was tried with the same result upon various species of animals—dogs, cats, hedgehogs, rabbits, Guinea-pigs, and squirrels. It seems that there are horses that cannot *back*; although they make good progress enough in a straightforward direction. Now Magendie says that he has opened the heads of such horses; and has always found, in the lateral ventricles of their brains, a collection of water, which must have compressed and even disorganized the corpora striata. It has been further ascertained, by the same experimenter and by others, that certain injuries of the cerebellum cause animals to move backwards contrarily to their will. If the tail of the animal so mutilated be pinched, he still persists in his retrograde course. Injuries of the medulla oblongata had the same effect. Pigeons into which he forced a pin through that part, constantly receded for more than a month, and even *flew* backwards. A section of the medulla oblongata, where it approaches the anterior pyramid, gives rise to a movement in a circle, like that of a horse in a mill; the animal, in its walk or its flight, bearing round continually to the injured side. Surely we have, in these facts, supplied by experiments on living animals, and by observation of the phenomena of disease in the living human body, some of the *materials* for a more exact knowledge, both of the physiology and of the pathology of the nervous system, than we have yet reached. M. Magendie supposes that different portions of the encephalon are endowed with energies which tend to cause motion in various directions; that in the healthy state these balance each other, and that a preponderating impulse can be given to any one of these forces by the will; but that when the equilibrium is destroyed by disease, the will is not sufficient to counteract the tendencies which are then brought into play. Mr. Mayo offers a different explanation of the phenomena. He supposes that the injuries inflicted on the nervous matter produce a sensation analogous to vertigo; and that the animal conceives itself either to be hurried forward, and makes an exertion to repel the imaginary force; or to be moving backward, or turning round in one direction, and endeavours to correct this by moving the corresponding muscles. Whatever may be the true explanation, the facts themselves are abundantly curious and interesting, and I recommend them to your attention.

Some of the affections that I have been describing, fall, perhaps, under the category of those to which the appellation of the *leaping ague* has been given in some parts of Scotland. There is a class also of convulsive spasmodic affections which resemble epilepsy on the one hand, and chorea on the other, or rather form a link of alliance between the two, and which are especially remarkable for this, that they are capable of being propagated by that kind of imitative contagion of which I have several times spoken. This point might be well illustrated by the history of various

sects of religious enthusiasts. One or two of those enthusiasts have apparently at first worked themselves up into a state approaching to epilepsy, accompanied even by insensibility sometimes; and then this state has been communicated by sympathy to the more susceptible of their auditors. I must not, however, go into any further details on this subject; and perhaps I have prosecuted it too far already. Those among you who are inclined to pursue it further may find some curious accounts of an epidemic which occurred in Lanarkshire, in Sir John Sinclair's *Statistical Account of Scotland*, under the head of the "Conversions of Cambuslang;" and in one of the early volumes of the *Edinburgh Medical and Surgical Journal*. Dr. Robertson has described in an inaugural dissertation *De Choreâ Sancti Viti*, a similar epidemic, which occurred in the States of Tennessee and Kentucky, in the western districts of America. This is also referred to in the same volume of the journal. Among other things Dr. Robertson says, that while extravagant sounds, and actions, and gesticulations, were in the first instance *wilful*, the actors "at length, to their own astonishment, and the diversion of many of the spectators, continued to act from necessity the curious character which they had commenced from choice." I will only remark further of such forms of nervous disease, that as they spring often from moral causes, so they admit, in a great degree, of moral remedies. The pranks played by the Scotch enthusiasts were brought to an end by threatening to duck every one who should thereafter be attacked; and, I believe, a few of them were horse-pounded, by way of example. With respect to the solitary instances of perverted locomotion, our business must be to correct whatever is wrong in the state of the bowels; in women, to amend the disordered uterine functions; to invigorate and confirm the system generally; and, in addition to the measures proper to effect these objects, I suspect that the cold sousing would in many cases be found of most material service.

LECTURE XXXVIII.

Paralysis Agitans. Mercurial Tremor. Hysteria: Two Forms of Hysterical Paroxysm; Diagnosis from Epilepsy; Class of Persons most liable to Hysteria; Diseases apt to be simulated by Hysteria; Treatment: Prevention.

IN the last lecture I spoke of chorea, and of some singular forms of disorder that have sometimes been included under the same appellation; and I shall begin the present with a few observations concerning a disease very closely allied to some of those which we were then considering, and yet distinct enough to deserve and require a separate notice. I refer to what has been called the *shaking palsy* — *paralysis agitans*. Allusions to this form of disease are to be found in many of the older systematic writers on physic; but it never was much attended to in this country until Mr. Parkinson published an essay upon it in the year 1817; and a very interesting little pamphlet it is. He defines the disease thus: — "Involuntary tremulous motion, with lessened muscular power, in parts not in action, and even when supported: with a propensity to bend the trunk forwards, and to pass from a walking to a running pace: the senses and intellects being uninjured." The latter symptoms constitute the *scelotyrbe festinans* of Sauvages; and the former symptoms of the definition are not always attended by the latter. In old persons you may often observe incessant and involuntary nodding and shaking of the head, without any tendency to run forwards. There is an old woman whom I see regularly sitting in the aisle at church every Sunday: she walks to her seat slowly and steadily enough, and sufficiently upright; but her head never ceases to nod, and wag, and tremble in various directions. It may be that she is in the less advanced stage of the malady; but I have remarked her for three or four or more years, and I see no change.

Mr. Parkinson's notice was first called to the disease during his professional attend-

ance upon a person affected by it. From observation of that case, and of several others that he subsequently met with, his account of the disorder was drawn up. He states that its first approach is insidious, and its progress often so slow and imperceptible that the patient cannot recollect precisely when it began. A sense of weakness, and a disposition to trembling, fastens on some particular part: sometimes it is the head, but more commonly it is one of the hands or arms. These symptoms gradually become more decided; and at length the morbid influence is felt in some other part. At a still more advanced period the patient is found to be less strict than usual in preserving an upright posture, even when standing or sitting, but especially when walking. By degrees he finds a difficulty in making the hand obey the dictates of the will when he is engaged in any delicate manipulation—in writing, for example; and he is obliged to walk with circumspection and care: his legs are not raised to that height, nor with that promptitude, which the will directs; so that much attention is necessary to prevent frequent falls. Then, as the malady proceeds, the propensity to lean forward becomes more strong—the patient is forced to step on his toes and forepart of his feet, while the upper part of his body is thrown so far forward as to render it difficult for him to avoid falling on his face: in some cases he is irresistibly impelled to take much quicker and shorter steps than common, and thereby to adopt unwillingly a running pace. When once this state has been pointed out, I make no doubt that some of you may recognise it, in old persons, whom you may have seen walking about. But the disorder does not stop here; the unhappy patient becomes unable to feed himself; or to walk at all without an attendant, who steps backwards before him, and prevents his falling forwards by the pressure of his hands against the forepart of the patient's shoulders: his powers of speech and deglutition fail; and the saliva dribbles from his mouth: he can no longer retain his urine or fæces; and at length death closes the miserable scene.

Mr. Parkinson conjectures that this complaint results from some chronic change of the upper part of the spinal cord, or of the medulla oblongata; but dissections are wanting to support or to refute that conjecture. Some of the patients whose cases he has given had been intemperate livers; hard drinkers: others had not been guilty of any such excesses: several had suffered a good deal from rheumatism, which he thought might have laid the foundation of their lamentable disease. But a more exact pathology of the shaking palsy is still needed. Dr. M. Hall observes that the symptoms have, in several particulars, a marked resemblance to the effects observed by M. Serres (and related in his *Anatomie du Cerveau*) of disease of the tuber annulare, or of the tubercula quadrigemina.

Nor have we any ascertained means of curing this disease; or rather, this state of decay. Dr. Elliotson indeed says that he succeeded in one instance (of which, however, the particulars are not given), with the carbonate of iron; but that he had tried the same medicine in vain in several other cases. We must administer to symptoms, and endeavour to set those functions right which may be obviously wrong: to regulate the bowels, to procure sleep, to nourish and uphold the patient without unduly stimulating him: and this is all that I can tell you of the shaking palsy.

Another analogous disorder, meriting a moment's notice, is that peculiar kind of trembling which is apt to occur in persons who are much exposed to the poisonous fumes of mercury: *mercurial tremor* it is called; and popularly, *the trembles*. It consists in a sort of convulsive agitation of the voluntary muscles, which is most violent whenever efforts are made to move the limbs by the help of those muscles; whenever, in fact, volition is brought to bear upon them. It differs therefore from the shaking palsy, inasmuch as the tremor ceases when the muscles are supported, or are not called into action. It is also more susceptible of relief by medicine. The last person in whom I have witnessed this curious affection has been twice my patient in the Middlesex Hospital, and has twice got well there. John Chatten, 33 years old, was first admitted in August, 1837. He was led into the room, walking with uncertain steps, his limbs trembling and dancing as though they had been hung upon wires. While sitting on a chair he was comparatively quiet; you would not have supposed that he ailed anything; but as soon as he attempted to rise, and to walk, his legs began to shake violently with a rapid, incessant, and irregular motion. He could neither hold them steady, nor direct them with precision. Indeed without

support he must have fallen down. His arms were agitated with similar involuntary movements. His tongue was tremulous, and he spoke in a hurried, abrupt, interrupted, staccato manner, not natural to him. He had no fever. His pulse was 66, and soft; his skin was natural; his bowels were costive. He complained of slight nausea. At the end of six weeks he went out well, or with very slight remaining weakness of his knees, and a little occasional tremor upon unusual exertion. In June, 1839, he again presented himself, in a similar state of agitation and helplessness.

This man was a water-gilder; and had been employed in that business for 18 years. Till somewhat more than a twelvemonth prior to his first appearance at the hospital, he had been free from disease. Then he began to tremble a little; but for a fortnight before his admission the shaking had become so much worse that he could not go up stairs, nor even walk upon uneven ground. The trembling, when once brought on by efforts to move, did not cease until he sat down, or got one of his fellow-workmen to grasp his limbs tightly.

This singular disorder is produced by the agency of mercury as a poison upon the body; and especially by the absorption of that metal when raised into vapour by heat, and inhaled in breathing. It is accordingly very common among water-gilders. Water-gilding is the gilding of metals, and of silver in particular, by means of fire. It is called *water-gilding*, I believe, to distinguish it from other kinds of gilding, called gilding in *oil*. The silver to be gilded is covered with an amalgam of gold and mercury, and then is placed over a charcoal fire, by which the mercury is raised in fumes, and driven off, and the gold alone is left adhering. To these fumes the workmen are necessarily exposed; and numbers of them become affected with this tremor, which is not a common result of mercury applied to the system in other ways. The same complaint is frequent among the workmen in the quicksilver mines of Friuli and of Almaden, where the crude ore is purified by the aid of heat. Dr. Bateman relates, in the 8th volume of the *Medico-Chirurgical Transactions*, some cases like that which I have been describing. But the best account of the disorder that I have seen is given by Merat, in an appendix to his book on the *Colique Méallique*.

The malady comes on sometimes suddenly, more often by degrees. The patient is less sure of his arms than usual; they become tremulous, and at last shake, and, if he continue to pursue his employment, the force of the trembling goes on increasing, till at length it is so general and violent that he can persist no longer. His power of locomotion is impaired; his mastication, his speech, all his manual operations, are interfered with; he becomes unable to convey food to his mouth, and is obliged to be attended to and fed, like an infant; and by and by, if he do not quit the poisonous atmosphere, graver symptoms supervene—wakefulness, delirium, loss of memory, loss of consciousness.

As the tremor increases, the digestive organs become disordered: the appetite falls off, nausea is felt, the tongue becomes furred, and gas collects in the intestines. The patients acquire a remarkable, brown, hue; and their teeth turn black. The pulse is generally full and slow.

The time required for the production of these effects varies much in different cases; from two years to five and twenty. Something depends, no doubt, upon the quantity and intensity of the fumes. Chattrin told us that the workmen became ill whenever they had a *large job* on hand. In both his severe attacks (and very often besides, both in him and in his companions) the mercury produced salivation. This was unfrequent in the patients observed by Merat. The duration of the complaint is considerable: it may last two or three months, or longer; and sometimes it is not completely recovered from at all. Yet it is not a fatal disorder.

Although the visible affection is of the *muscles*, the mischievous operation of the poison is really upon the *nervous centres*, weakening and interrupting their natural influence. When the will is directed upon the muscles, they contract unsteadily, and with frequent remissions; their action is not sustained; and it is a general observation by all who have written upon the disease, that it is aggravated by all kinds of mental emotion, by alarm, anger, surprise. My patient's shaking was, at first, augmented by the shock of the shower-bath: and always became excessive in thundery weather. So, on the other hand, it has been noticed that whatever tends to stimulate and fortify the nervous power, does temporary good: a glass of wine for example. Chattrin

informed us that, while the malady was coming on, he could not get up stairs to his work without first swallowing half a quartern of gin: and that he was obliged to drink porter two or three times a day.

Tremor is *always* a token of debility. What is called tone of the muscular system is a sustained state of gentle contraction, due probably to the reflex power of the spinal cord. It is believed to be maintained by a continuous stream of nervous force; or by a succession of nervous impulses so rapidly repeated that their effect appears to be uninterrupted. When the stream fails to be continuous, or when the impulses no longer succeed each other with the requisite rapidity, the muscular fibres relax and contract alternately and briefly, and the phenomena of tremor are presented. Some of the German physiologists have illustrated this view of the matter by experiments performed upon living animals. If a motor nerve be divided, the muscles supplied by it become flaccid and lax. A strong electric force applied to the separated nerve produces convulsive contractions of those muscles: but their natural tension and firmness may be restored by a weak current of electro-magnetism. When the rotatory movement of the machine is sufficiently rapid, the stimuli follow each other so closely as to leave no perceptible interval between them, and the palsied muscles resume and retain their *tone*. But if the revolutions of the instrument are performed more slowly, so that the successive stimuli are separated by appreciable intervals, the muscular fibres relax in accordance with those intervals, and *trembling* occurs.

The treatment, then, of tremor, considered generally, must consist in the adaptation to the particular case, of those natural agencies which tend to restore the lost tone of the body—well-chosen food, fresh air, regular exercise, and the like—and in the administration of tonic drugs, such as steel or quinine. I should expect—indeed I may say that I have witnessed in such cases—beneficial results from very small doses of strychnia. But when the tremor depends upon some known exciting cause, as in the mercurial tremor which is now before us, there is something preliminary to be done. We must, if we can, remove the patient from the further operation of the poison, and remove the poison already imbibed from the body of the patient. The first of these objects is secured by withdrawing him from the injurious atmosphere. The second may, I believe, be greatly facilitated and made complete by giving him suitable quantities of the iodide of potassium.

This is comparatively new doctrine. It was put forth in 1849 by M. Melsens in a paper in the *Annales de Chimie et de Physique*, of which paper a translation, with some valuable prefatory remarks, was published by Dr. William Budd, in the *British and Foreign Medical Review* for January, 1853.

“In all cases (says Dr. Budd) of mercurial and saturnine poisoning (for the paper comprehends slow poisoning by lead, as well as by quicksilver), M. Melsens assumes, and no doubt rightly, that the metallic substance is in actual union with the affected part or parts, and is retained there in the form of some *insoluble* compound.

“According to his view, the iodide of potassium, after its absorption into the blood, combines with the metallic poison, and forms with it a new and *soluble* salt; liberates the poison from its union with the injured part; dissolves it out, so to speak, from the damaged fibre, and sets it once more afloat in the circulation.

“The new compound thus set at liberty (under the form, it is presumed, of a double iodide of mercury and potassium) he supposes to be eliminated through the kidney almost as soon as formed, in combination with any excess of iodide of potassium that may happen to be present. So that poison and remedy being both cast out together, the cure may be said, in a peculiar sense, to be radical and complete.”

M. Melsens gives some striking examples of the rapid cure of mercurial tremor by this specific treatment. In one of these cases mercury was sought for, and found, in the patient's urine, while he was taking the iodide. It is remarkable, too, that this man recovered perfectly, although he continued all the while to work at his trade as a gilder.

Surely facts of this kind, if future experience shall authenticate them, are pregnant with important suggestions. In all probability it is in the very same way that the iodide of potassium combines with and carries off the syphilitic poison, and removes, often with a rapidity almost marvellous, large venereal nodes.

There is one possible source of peril to be guarded against in this sort of cure. The poison, again set afloat, is no less a poison: and if it be suddenly reconveyed into the

blood in large quantity, it may compromise the patient's safety by converting his chronic disorder into a case of acute poisoning. Dr. Budd relates an instance of what I suspect to be a not very uncommon event. A patient of his in the Bristol Infirmary, was put under treatment by the iodide of potassium in free doses, for the cure of secondary syphilis. He had taken mercury largely some time before, but none for several months. In a few days he presented all the well known symptoms of severe *mercurial* pyalism, although not a grain of mercury had been given after his admission.

The caution required is that of carefully adjusting the dose, and carefully watching the effects of the remedy. You must begin with small quantities of the iodide, and gradually feel your way to larger.

When all the poison has been expelled, it may be necessary to give tonics. Even without the previous employment of the iodide of potassium, quinine has been found useful. But I have most faith in preparations of iron. My patient Chatten mended decidedly and rapidly when he began to take steel. It was not the mere avoidance of the cause of the complaint that produced the improvement, for he had been away from his work for a fortnight before he applied for admission.

To prevent this effect of mercury, the workmen should be instructed to avoid, as much as possible, inhaling the poisonous fumes, to ventilate the room thoroughly, and to pay great attention to cleanliness. I believe the furnaces may be so built that the metallic vapour shall not reach the operator. If he cannot avoid being involved in it, perhaps some sort of respirator might afford protection.

[A very peculiar form of convulsive disease has recently been described. It is characterized by repeated bobbings of the head forward, at first slight and occasional, but becoming, in process of time, so frequent and powerful, as to cause a heaving of the head forwards, towards the knees, succeeded by an immediate return to the upright position, somewhat similar to the attacks of *emprostotonos*. In one case, related by J. W. West, these bobbings were repeated at intervals of a few seconds, ten, twenty, or more times, in each attack, which continued from two to three minutes, and recurred twice, thrice or oftener in the day; the attack occurring whether the patient was sitting or lying. During the attack, the child retained his consciousness. The other cases that have been since recorded by Drs. Barton and Bennett, in their general symptoms, differ in no degree from that of Mr. West, with the exception of that of Dr. Bennett, in which the disease was of a more aggravated character. Sir Charles Clarke has seen four cases of the disease, and from the peculiar bobbing of the head, has named it the *Salaam Convulsion*; Dr. Locock has seen two cases. One of Sir Charles Clarke's patients recovered perfectly, the other became paralytic and idiotic, and died at the age of seventeen. Mr. West has heard of two other cases — one of the patients lived to the age of seventeen; the other to nineteen, — both became idiotic. Faber (*Semidt's Jarbuch*. vol. lxvii.) relates two cases; one in a girl of three, and the other in a boy of six years of age. The patients whose cases are on record, were chiefly boys; their ages varied from three to six years — death did not occur in either; in some the disease appears to have ceased spontaneously.

Of this strange form of convulsions, the pathology is still a subject for future investigation, and until that is ascertained, its treatment must be tentative and experimental. — C.]

I proceed to the subject of *hysteria*: a subject highly interesting and important, as well as obscure and difficult. I scarcely know how to arrange what I have to say, so as to present the disorder to your notice in the most intelligible manner. Hysteria has characters peculiar to itself: but it is apt also to assume the form, and mimic the symptoms, of various other diseases of a much graver nature. If we are not capable of distinguishing the true malady from that which is its double, we shall be constantly committing most serious mistakes in the prognosis, to our own damage and discredit; and in the treatment, to the injury of our patient. I shall first attempt to describe to you the phenomena which are peculiar to hysteria; and then to point out the class of persons who are most subject to it; and afterwards I shall briefly advert to the imitative freaks which we are almost daily witnessing in hysterical constitutions, and to some other points connected with this extraordinary complaint.

I need not tell you that the hysterical *paroxysm* is almost, though not exclusively, confined to women.

[We have repeatedly seen all the phenomena characteristic of hysteria in the male subject. The fact of their frequent occurrence in males is also stated by Sydenham, Louyr Villermay, Georget, Ferriar, Froten, Conolly and others. See also the admissions of Dr. Watson towards the close of his remarks on the pathology of the disease. — C.]

It occurs under a great variety of forms, but they may all be reduced, for convenience of description, to two. The first of these has a general resemblance to an epileptic fit. The trunk and limbs of the patient are agitated with strong convulsive moments: she struggles violently, like a person contending; rises into a sitting posture, and then throws herself back again; forcibly retracts and extends her legs, while her body is twisted from side to side: and so powerful are these muscular contortions that it often is all that three or four strong persons can do to restrain a slight girl, and prevent her from injuring herself or others. The head is generally thrown backwards, and the throat projects; the face is flushed; the eyelids are closed and tremulous; the nostrils distended; the jaws often firmly shut; but there is no *distortion* of the countenance; the cheeks are at rest, unless when, as often happens, the patient is uttering screams or exclamations. If the hands are left at liberty, she will often strike her breast repeatedly and quickly, or carry her fingers to her throat, as if to remove some oppression there; or she will sometimes tear her hair, or rend her clothes, or attempt to bite those about her. With all this, her breathing is deep, labouring, irregular; and the heart palpitates. After a short time this violent agitation is calmed: but the patient lies panting and trembling, and starting at the slightest noise or the gentlest touch; or sometimes she remains motionless during the remissions, with a fixed eye; till all at once the convulsive movements are renewed: and this alternation of spasm and quiet will go on for a space of time that varies considerably in different cases; and the whole attack frequently terminates in an explosion of tears, and sobs, and convulsive laughter.

There is a *variety* of this form of hysterical paroxysm, in which the patient suddenly sinks down insensible, and without convulsions: with slow and interrupted breathing, a turgid neck and flushed cheeks; and she recovers from that condition, depressed in spirits, fatigued, and crying.

You will observe that the symptoms I have been enumerating belong to the nervous system; and indicate great derangement in the functions of animal life. In the other of the two forms to which all the various modifications of the attack may be reduced, the principal marks of disturbance are referrible to some of the viscera. The patient experiences a sense of uneasiness in some part of the abdomen, frequently towards the left flank; a ball appears to roll about, and to rise first to the situation of the stomach, and then to the throat, where the patient feels a choking sensation; the action of swallowing is frequently repeated; the abdomen becomes distended with wind, loud rumblings and sudden eructations take place; there is much palpitation of the heart, the patient is sad and sorrowful, and prone to shed tears.

After the paroxysms, these patients commonly void a large quantity of limpid, pale urine, looking almost like water; and this is sometimes expelled during the fit.

Such is a brief, and, I am aware, incomplete account of the hysterical paroxysm. It sets forth, however, in outline, the two principal varieties of the attack: and you are to observe that the last, the quieter form, is often the prelude to the convulsive; but it not seldom also occurs alone, and then is as indicative of hysteria, as the *petit mal*, to which it is somewhat analogous, is of epilepsy.

And before I go any further, let us again inquire into the circumstances which distinguish the *paroxysms* of those two diseases, epilepsy and hysteria. I have shortly adverted to these discriminative circumstances before; but we shall be better able to appreciate them, now that the main features of each diseased state have been under our consideration. It is of great importance to be able to render the diagnosis certain and accurate. It is a dreadful announcement to have to make to a father or a mother that their child is epileptic; whereas, hysteria, though it is sufficiently distressing, is attended, in nine hundred and ninety-nine cases out of a thousand, with

no ultimate peril, either to mind or body. In some instances the diagnosis is perfectly easy: in others it is dubious and full of anxiety. Whenever you fail to satisfy yourselves completely as to the nature of a given case, you will do well, in legal phrase, to give your patient the benefit of your doubt, and acquit her of epilepsy; or pronounce her guilty of the minor offence of hysteria.

The points of resemblance, and the points of distinction, belonging to the hysterical and the epileptic paroxysm respectively, have been very clearly summed up by Foville.

There are two principal forms of each disorder. In each, one of these forms is convulsive, and the other is not. The non-convulsive form of epilepsy relates exclusively to the sensorium: it is characterized by vertigo, and by a suspension (however brief and transitory) of the mental powers. The non-convulsive form of hysteria has little apparent connexion with the animal functions: its palpable phenomena consist in derangement of the organic functions of the thorax and abdomen. It is the ganglionic portion of the nervous system that seems chiefly disturbed.

In the epileptic *fit* there is an entire loss of consciousness. The patient, on emerging from the paroxysm, recollects nothing of what has been going on during its continuance. It is not so in the hysterical *fit*. The loss of consciousness is very seldom complete: and it never occurs at the outset of the attack. The patient often is able to repeat (though she may not always choose to confess it) what has been said by the bystanders during the period when she seemed insensible. This is a point of distinction well worth remembering, for more reasons than one. It not only helps the diagnosis when the fact comes out; but it suggests certain cautions to ourselves. We must take care not to say anything by the bed-side of a hysterical patient, which we do not wish her to hear: and we may take advantage of her apparent unconsciousness, and pretend to believe in it, and speak of certain modes of treatment which she will not much approve of, but the very mention of which may serve to bring her out of the *fit*.

In the epileptic paroxysm the face is usually livid; and foam, which is frothy with air, or red with blood, escapes from the patient's mouth. These are symptoms which we do not see in the fits of hysteria. The convulsive movements even, offer some characteristic shades of distinction. In epilepsy they are often more marked on one side of the body than on the other, and less irregular: the same movements are rapidly repeated: there is a strangling rattle in the breathing: while in hysteria the forcible flexion and extension of the limbs, and the contortions of the trunk, are more sudden, and, as it were, capricious; the respiration is deep, sighing, mixed with cries, and sobs, and often with laughter. But, perhaps, the convulsive motions differ most in the face. The epileptic expression is usually frightful: the eyelids half open, the eyeballs rolling, the mouth drawn to one side, the teeth grinding, the gums exposed by the retraction of the lips, the tongue protruded and bleeding, the complexion leaden; while in hysteria the cheeks are red, but at rest; the eyelids are closed and trembling; if you raise the upper one, you will see the eye fixed, perhaps, but it is bright, and very different from that of the epileptic, which, if it be not rolling, is dull, projecting, and the pupil usually dilated.

Foville states that when, besides a sudden loss of consciousness with convulsive movements, there are also lividity of the face, and an escape of frothy saliva from between the lips, and the convulsions are more pronounced on the one side of the body than on the other, the disease is epilepsy, and not hysteria: and I think he is right.

By Dr. Marshall Hall the grand distinction between the two diseases is affirmed to be this:—that in hysteria, much as the *larynx* may be affected, it is never closed; in epilepsy, it is closed. Accordingly in the former we have heaving, sighing inspiration; in the latter, violent ineffectual efforts at expiration. In the very outset of the epileptic paroxysm the respiration, I believe, is thus suspended.

The hysterical seizure may be over in a quarter of an hour, or in less time than that; or it may last many hours, or even several days.

The hysterical seizure is almost peculiar to women: and it seldom occurs in them except during that period of their lives in which the menstrual function of the uterus is, or ought to be, in activity. In this country it is most apt to occur between the ages of fifteen and forty; and in the vast majority of patients who do suffer it, you

will find some marked derangement of that particular function. These facts alone afford a strong corroboration of the ancient theory, which ascribed the whole of the phenomena to uterine disorder; and *named* the disease accordingly. You will hear or read of disputes as to whether the womb, with its appendages, or the nervous system, is the seat of hysteria. But such disputes are merely verbal, I conceive. No doubt the convulsive movements, and the mental affection, and the unnatural sensations, depend upon some altered condition of the brain and nerves; but it does not follow that the disease originates in that altered condition. We know that the uterus, or the ovaries, cannot of themselves determine the muscles to contract; but if they be in an unhealthy state they may act upon the muscles through the *medium* of the nervous system: and such I take to be the fact. *How* they do so we no more know than we know how the little finger is bent when we resolve to bend it.

But, say some, we every day meet with diseased conditions of the uterus and ovaries — amenorrhœa, dysmenorrhœa, menorrhagia, even disorganization — without any of these nervous symptoms. True; and we cannot always fathom the mystery of this. But one thing is certain, that there exists in some persons a much greater readiness to take on the disease, upon the application of the exciting cause, than in others. This predisposition I have had occasion to advert to again and again, since I began to speak of the *spasmodic diseases* of the nervous system. Such diseases occur in certain individuals only; and in these individuals there pre-exists a peculiar condition of the nervous system, “for which,” says Dr. Alison, “we have no more precise or definite expression than *nervous irritability*, or *mobility*; a condition which is more common in women and children than in men; and more common in all persons when in a state of weakness, than when in the full enjoyment of muscular strength; in women, particularly, more common about the menstrual periods, and immediately after delivery, than at other times; more common likewise in those in whom the monthly discharge is habitually *excessive*, or *altered*, as in *leucorrhœa*, or suddenly *suppressed*, or more gradually obstructed in the different forms of *amenorrhœa*, than in others. In this condition of mobility, both sensations and emotions are intensely felt; and their agency on the body is stronger and more lasting than usual; continued voluntary efforts of mind, and steady or sustained exertions of the voluntary muscles, are difficult, or impossible; the muscular motions are usually rapid and irregular, and the ‘*animus, nec sponte, varius et mutabilis.*’” In persons of this moveable temperament, spasmodic complaints are easily excited: and the tendency to their recurrence is increased by each repetition of them.

Now the persons who suffer hysteria are of this class. They are commonly young women, in whom the process of menstruation is in some way or other disordered; and who either are naturally of a feeble constitution, or have been debilitated by disease, or by their habits of life. Often they are pale; have cold hands and feet; are subject to chilblains; eat but little, and do not fancy meat, which they sometimes absolutely dislike and refuse; or their taste is depraved and capricious; they will devour wax candles, wafers, chalk, sealing-wax, slate pencil, and such trash. And what is very curious and characteristic, although they often abstain almost entirely from animal food for weeks or months together, and take very little nourishment of any kind, they do not in general emaciate. You might expect that, under such a mode of life, they would waste away; but they continue round, and plump, and smooth. Some of them are even ruddy.

And belonging to women of this peculiar constitution there is one other very remarkable character, which it behoves us to make ourselves thoroughly acquainted with. Almost any part of the nervous system, in these persons, is liable, under the influence of slight causes, and even without any obvious cause, to fall into a disordered state of action and suffering, more or less resembling that which inflammation or organic disease might excite in the same part.

This is a most important fact; because if we erroneously ascribe symptoms which really result from inflammation to mere nervous or hysterical disorder, we may suffer the patient to perish for want of active measures that might have saved her; and on the other hand, if we apply to these nervous, imitative, hysteric complaints, the treatment proper for inflammation, we shall generally, indeed, relieve our patient for the time; but we shall leave her more prone to the nervous affection than before, and permanently damaged by our mischievous activity.

[On the subject of the pathology of hysteria the reader is referred to the very judicious paper of Dr. Conolly in the 2d vol. of the Cyclopædia of Practical Medicine, Philadelphia edition, 562, *et seq.* — C.]

I say that almost every kind of serious disease may be mimicked by what we must call hysteria. And your skill will sometimes be severely tasked to determine the true import of the symptoms, and the real nature of the case.

One of the diseases which is most often copied by hysteria, is *inflammation of the peritoneum*. You will find a patient complaining of acute pain of the abdomen, aggravated by the slightest pressure; and she shall have, perhaps, a hot skin, a quick pulse, and a furred tongue. When you meet with such symptoms in a young female, in whom there is any derangement or irregularity of the uterine functions, you will do well, before you bleed her to syncope, and cover her abdomen with leeches, to ask yourselves whether all this suffering may not be simply nervous. Search into her previous history as narrowly as you can. If you find that she has had similar attacks before; if she have been known to suffer hysterical fits; and if the tenderness be excessive, and, as it were, superficial, felt upon the slightest touch as much as when firmer pressure is made, you may generally spare the blood-letting, purge the patient well, and cause an asafetida enema to be thrown into the rectum; and in a few hours you will find that the peritonitis has vanished.

Among the pains which infest females of the hysterical constitution, and which are apt to be erroneously ascribed to inflammation, *stitches and pains in the hypochondria* are probably the most common. They are oftener complained of in the left hypochondrium than in the right. These things are much more generally understood now than they used to be even a few years ago. I cannot tell you how many persons I have seen who had been diligently treated with leeches, and blisters, and blue pill, for supposed chronic inflammation of the liver or spleen, or still more actively depleted for presumed pleurisy or pericarditis, when no such inflammation existed, and when the treatment, by reducing the strength, tended to rivet that mobility of system which was the chief predisposing cause of the pains.

You would scarcely suppose that *palsy* — decided hemiplegia or paraplegia — could be simulated by hysteria: yet this certainly is the case; and I have seen instances of it even among hospital patients. They are difficult and perplexing cases. The sudden occurrence of the paralysis, with no corresponding affection of the face or tongue, and without any of the other symptoms which commonly mark the real disease, its sudden disappearance, and, above all, the supervention of a hysterical paroxysm, will often disclose the true nature of the disorder. Dr. Todd asserts that in hysterical hemiplegia (which is generally incomplete) the patient drags the palsied leg along the floor after her as if it were dead, without endeavouring to lift it, and without that swinging-round movement of the limb observable in those who are hemiplegic from organic disease of the brain. Paraplegia, again, coming and departing with like suddenness and caprice, is no unfrequent shape assumed by this changeful malady. The sensibility of the lower limbs is often impaired in these cases as much as, or even more than their muscular power. And having thus been led to touch again on the subject of paraplegia, let me take the opportunity of repairing, parenthetically, an oversight and omission of which I was guilty when that disease was more directly under our consideration. I ought to have made you acquainted with a useful classification of the phenomena of paraplegic disorders, suggested by Dr. Gull. Dealing with acknowledged facts, and referring to a large number of recorded examples, he remarks that when paraplegia results from local injury or local disease of the cord, the motor functions are always more involved than the sensitive; the palsy is always more decided, and more abiding, than the anæsthesia; and frequently there is no loss of sensibility at all. No circumscribed or segmentary affection of the spinal cord ever produces loss of sensation only, without loss of motion also.

On the other hand, there are many cases of paraplegic disorder in which the anæsthesia preponderates, or even sometimes exists alone. Experience teaches us not to expect to find, in these cases, after death, any local or limited disease of the cord. There are, in fact, two forms of paraplegic malady in which the loss of sensibility thus occurs alone; or precedes, or surpasses in degree, the loss of muscular power. In one of these the affection is primarily peripheral. Its causes (exposure of the sur-

face to cold and wet being one of the commonest causes) act upon the sentient extremities of the nerves themselves, the muscular movements of the limbs being subsequently impaired, if they are impaired at all. Of this I gave you some remarkable instances. In the other form the paraplegia and the diminished sensibility seem to have an encephalic origin; and its apparent causes are such as operate upon the nervous system generally—mental and moral causes—influences which tend to lower the nervous energy. No definite or appreciable change presents itself in the spinal cord; but sub-arachnoid effusions, collections of fluid in the cerebral ventricles, with general wasting, perhaps, or general softness, or general induration, of the nervous substance, are frequent concomitants of this condition. This disorder appears, I say, to be frequently the result of mental anxiety, of depressing circumstances, reverses of fortune, overmuch study, irregular habits of life, and similar exhausting agencies, long continued.

I am desirous of placing this classification before you, even thus irregularly, because I think it calculated to enhance the interest of every fresh instance of paraplegia; to facilitate our future study of that very difficult and obscure disorder; and to conduce to greater exactness in the framing of its natural history.

To return from this digression to our current theme of mimic disease. Hysterical affections referred to the throat are very common. *Aphonia*, for example: the voice being lost on a sudden, and recovered as suddenly. *Mock laryngitis*. I remember being asked by Sir Charles Bell some years ago, to see a young woman in the Middlesex Hospital under his care. She had recently arrived, and was breathing with the stridulous noise peculiar to inflammation of the larynx. She had twice before, in the country, had tracheotomy performed for similar attacks; and there were the scars of the operations on her neck: but both Sir Charles and myself were satisfied, upon considering all the circumstances of the case, that the difficult inspirations were spasmodic and hysterical; and she recovered under the remedies which do good in hysteria. Inability to swallow, *dysphagia*, is another of the hysterical vagaries relating to the parts about the throat. Dr. Bright has a very instructive case of that kind. A patient was sent to Guy's Hospital for stricture of the œsophagus. It was stated that the difficulty of deglutition had existed for several weeks, and was increasing. The surgeon under whose care she was admitted was instantly struck by certain circumstances which did not seem to consist very well with the notion that there was organic disease. Her appearance belied it, and her age. But he thought it right to examine the œsophagus by means of a probang; and no sooner was the instrument introduced, than the patient went into a hysterical fit, which was followed immediately by hysteria in several females in the same ward. The complaint turned out to be nothing but a hysterical constriction, and was soon completely removed.

Surgeons are familiar with the "hysterical breast." The mamma becomes painful, tender, enlarges somewhat perhaps. The girl fears that a cancer is breeding. She communicates her alarm to her friends, and a medical man is consulted. If he happen to be timid and inexperienced, he makes matters infinitely worse by applying leeches and fomentations; by examining the breast at every visit; and by keeping the patient's attention anxiously fixed upon it. Whereas the treatment ought to be directed to the state of the general system; and the local uneasiness spoken lightly of, or disregarded.

Among the hysterical affections of the air-passages, there is a peculiar kind of cough which you ought to be acquainted with. It is loud, harsh, dry, more like a bark than a cough. Sometimes it is incessant, sometimes it occurs in paroxysms which, I verily believe, are more annoying to hear than to suffer. Hysterical affections of the diaphragm, again, are by no means rare. I had a very obstinate case of that sort in one of my hospital patients. She would sit in her bed all day long, uttering every eight or ten seconds a loud and most discordant *hiccup*. And I remember an out-patient, who presented a picture of perfect health, and who came week after week, to be cured of what I could consider nothing but a hysterical *eructation*: it was continual and distressing, and prevented her from obtaining any employment as a servant. Hysterical *vomiting* is also frequent, simulating cancer of the stomach. Nay, hysterical *hæmatemesis*. A romantic girl was for some months under my care in the hospital with that complaint. She vomited such quantities of dark blood (which did not coagulate, however), as I would not have believed if I had not seen them. Day after

day there were potfuls of this stuff: yet she did not lose flesh, and she menstruated regularly; and what was very curious, the vomiting was always suspended during the menstrual period, and recurred again so soon as the natural discharge ceased. I said she was romantic; but I should rather have said that she had that peculiar mental constitution which belongs to hysterical females. She used to write me long letters of thanks for my attention, though I was heartily tired of her; and these were couched in all the fine language of the Minerva press. At last I sent her away: just as bad as when she came into the hospital. This was five or six years ago; and last year she called at my house with a present of some game, and told me she had got married to a hair-dresser, and was quite recovered.

There is a kind of sanguineous *expectoration* belonging to females of this class, and very likely to mislead the unwary. I meet with two or three instances of it every year. The patient excretes daily, or at irregular intervals, a thinnish fluid something like saliva, more or less tinged and streaked with brown or florid blood. A young hand investigates diligently the source of the bleeding, and puzzles himself to determine whether the case be one of *hæmatemesis* or of *hæmoptysis*. Nine times out of ten it is neither the one nor the other. The blood comes from the mouth, or the fauces.

Hysterical affections of the *joints* are common also. A young girl became my patient in the hospital for some trifling ailment, and after a short time she began to complain of great pain in her knee and hip; she could not stand upon the limb, nor bear to have it moved or touched. I got Sir Charles Bell to see her: he was so satisfied of the nature of the case—so convinced that it was a genuine example of inflammation and ulceration of the hip-joint—that he gave a little lecture to the pupils who stood round the bed, upon the characteristic position in which the patient lay; and he took her into one of the surgical wards to be under his own care. Some time afterwards I had occasion to go into that ward, and there I found my former patient with her heel drawn tight up against her buttock. It turned out that she had had no serious disease of the hip at all: both it, and the rigid contraction, gave way under measures which could have done no good to an ulcerated joint. I think the first clue to the real nature of her malady was the occurrence of a fit of hysteria. Sir Benjamin Brodie says, that among the higher classes of society, at least four-fifths of the female patients who are commonly supposed to labour under diseases of the joints, labour under hysteria, and nothing else.

Another prank belonging to hysteria, and one which it is very necessary that you should be on your guard against, is that of mimicking disease of the bones of the spine. The patient complains of pain and tenderness in her back, and of weakness probably in her lower extremities; and it is now become notorious that scores of young women have been unnecessarily confined for months or years to a horizontal position, and have had their backs seamed with issues, for supposed disease of the bodies of the vertebræ, who had really nothing the matter with them but hysteria, and who would probably have soon ceased to complain if, instead of being restricted to that unnatural imprisonment and posture, they had taken a daily gallop on horse-back.

It is curious enough to notice how the mind is apt to become affected in some of these cases. After the patient has been lying supine for some weeks, she *is* unable to stand or walk, simply because she *thinks* she is unable. The instant she makes a fair effort to use her limbs again, she can and does use them. Her condition is at once reversed. *Potest quia posse videtur*. Dr. Corfe, the present apothecary to the Middlesex Hospital, has no little trouble with patients of this kind; but he generally succeeds in *making* them walk, and in convincing them, as well as himself, that they may do so with impunity. Sometimes, though the authority of the Doctor may not be efficacious in this respect, some stronger influence prevails. A lady told me not very long ago that an acquaintance of hers, a member of a family of distinction, had been lying I know not how long on her back; that position having been prescribed to her by some medical man for a presumed disease of the spine. She lost all power of using her legs; but she got quite fat, as, indeed, well she might, for her appetite was remarkably sharp, and she lived chiefly upon chicken; and the number of chickens she devoured was incredible. She lived at some little distance from town, and at last Sir Benjamin Brodie was sent for to her. Now Sir Benjamin, to use a

vulgar phrase, is *up* to these cases; and he wished to see her *try* to walk: but she declared that the attempt to do so would kill her. He was resolute, however, and had her got out of bed; and in a few days time she was walking about quite well, and very grateful to him for his judgment and decision. A medical man of less name, or of less determination, would probably have failed. Dr. Bright has a good example of a somewhat similar kind; showing the power of another form of influence. He was asked to see a young lady who had been confined to her bed for nine months. If she attempted to move she was thrown into a paroxysm of agitation, and of excruciating agony, affecting more particularly her abdomen. She had almost lost the use of the lower extremities; and she and her friends seemed to have given up all hope of her restoration. But she presented no appearance of important disease; her countenance bore no marks of visceral mischief; nor was it possible to discover any proof of organic change. Dr. Bright set the case down in his own mind as one of hysteria. She was thought to have derived relief from some stimulating injection, and from certain pills. As her friends were in moderate circumstances, Dr. Bright talked seriously to the mother, and recommended that simple water should be employed for the injection, and that bread pills should be substituted for those the girl had been taking. The mother soon perceived that these means produced the same tranquillizing effects on her daughter which had hitherto been ascribed to the medicine. "My visits," he says, "became less frequent; I was absent a fortnight: on my renewing my visit, no change had taken place. I attempted to get her shifted gently from the bed to the sofa, but it was impossible; the paroxysm almost overcame her. Once (after having attended altogether about nine months) I called after an absence of nearly a month; her sister met me at the street-door with a smiling face to tell me that our patient was quite well: and on inquiry, she related how, three mornings before, under a *deep religious impression*, she had completely recovered all her powers; and I found her sitting up, working and amusing herself as if she were completely convalescent from some ordinary illness."

Southey, in one of his published letters, speaking of his mother, says—"While she was a mere child she had a paralytic affection, which deadened one side from the hip downward, and crippled her for about twelve months. Some person advised that she should be placed out of doors in the sunshine as much as possible; and one day, when she had been carried out as usual into the forecourt, in her little arm-chair, and left there to see her brothers at play, she rose from her seat, to the astonishment of the family, and walked into the house. The recovery from that time was complete."

These are the cases which suit the purposes of miracle-mongers. A few years ago all the journals belonging to a certain party in the religious world were full of an instance of miraculous cure. The patient was a young woman; her legs had been paralytic, or contracted, I forget which; some enthusiastic preacher had influence enough with her to make her *believe* that if on a certain day she prayed for recovery with a strong faith, her prayer would be successful, she would recover at once; and she did so. No one can doubt that it was just such a case as those I have now been mentioning. Many of these pseudo-diseases terminate suddenly under some strong moral emotion. A fall—a fire in the house—any overwhelming terror, will sometimes put an end to them. And where the joints have been the parts affected, several patients have declared that they felt a sensation as if something had snapped or given way in the part, immediately before the sudden recovery took place.

Some of the shapes assumed by this pathological Proteus are hideous and disgusting. Paralysis of the muscular fibres of the bladder, or spasm of its sphincter, sometimes really occurs, sometimes is only aped, in hysteria. It is a common trick with these patients to pretend that they labour under *retention of urine*; and that, although the bladder is full, they cannot make water. The daily introduction of the catheter by a dresser or apprentice appears to gratify their morbid and prurient feelings. Sometimes, no doubt, the difficulty is real; but it is oftener feigned or exaggerated. I have again and again known it disappear upon the patient's being left, without pity, to her own resources. But girls have been known to drink their urine, in order to conceal the fact of their having been obliged and able to void it. The state of mind evinced by many of these hysterical young persons is such as to entitle them to our deepest commiseration. The deceptive appearances displayed in the bodily functions and

feelings, find their counterpart in the mental. The patients are deceitful, perverse, and obstinate: practising, or attempting to practise, the most aimless and unnatural impositions. They will produce fragments of common gravel, and assert that these were voided with the urine: or they will secrete cinders and stones in the vagina, and pretend to be suffering under some calculous disease. A young woman contrived, in one of our hospitals, to make the surgeons believe that she had *stone in the bladder*: and she actually submitted to be placed upon the operating table, and to be tied up in the posture for lithotomy, before a theatre-full of students; and then the imposture was detected. Sometimes they simulate *suppression of urine*, and after swallowing what they have passed, vomit it up again, to induce the belief that the secretion has taken place through the new and unnatural channel.

It is impossible, I say, not to pity the unhappy victims of this wretched disorder, when their morbid propensities drive them to such acts as these. I mention them because you must expect to meet with such cases; and because while you take care not to express your suspicions prematurely, or on light evidence, you should be upon your guard against the mortification of being deceived, by the false signals held out, into active and ill-directed measures of treatment.

There is another very common hysterical *pain* which I ought to have mentioned, viz., a pain occupying some one point in the head; the patient speaks of it as a sensation like that which would be caused by driving a nail into the part; and the affection has therefore been called the *clavus hystericus*. It is often situated just above one eye-brow; and it sometimes comes on every day, at the same hour. Now in these cases it imitates very closely the hemicrania, which constitutes no uncommon form of an intermittent, and is called, accordingly, the *bravo ague*. The distinction between the two—whether the affection, I mean, be hysterical or aguish—is not of any great consequence: but in many of the previous examples of hysterical pain mimicking organic or inflammatory disease, the diagnosis is obviously of the greatest moment.

How, then, is it to be made? You may, generally, I believe, be led to a right judgment if you look to the several points that I have incidentally touched already. You may guess that the affection is hysterical if the patient be a young unmarried woman; if there be any disorder or irregularity in the uterine functions; if you can gather any history of former hysterical disease; and especially if she be subject to *fits* of hysteria. The suspicious symptoms may often be traced back, and found to spread themselves over a considerable previous period of time; yet there is no such wasting, or commensurate deterioration of the general health and strength, as might be expected in organic disease. When the complaint simulated is some acute local inflammation, and there is pain, increased upon your pressing the part, you will find that the pain is aggravated by the gentlest touch; it is more felt if you brush your hand over the surface, or slightly pinch up the integuments, than when firm pressure is made: and you will find also that this exquisite tenderness is not limited to the part complained of. Suppose it to be the abdomen, the patient will shrink and exclaim if you suddenly put your finger on her neck, or her arm. The suspicion that the disorder is nervous or hysterical will also be corroborated if the symptoms which resemble the symptoms of inflammation arise and subside rapidly, without obvious cause for such fluctuation; and if various organs appear to be attacked in succession. Between the several symptoms that mark real disease there is always (as we learn by experience) a certain congruity and relation; but in the simulative displays of hysteria the symptoms are apt to be irregular, inconsistent, contradictory. When, after the most careful investigation of the case, you still doubt, it will be right either to pause, or to treat it upon the most unfavourable supposition. The consequences of suffering active inflammation to go on unchecked would be far worse than the temporary and slight and remediable injury to the system, which might result from once applying the remedies of inflammation to a case of mere hysteria. There is another hazard also which you must be aware of, and seek to avoid; that of overlooking real disease, when it is mixed with, and masked by, hysterical symptoms. It is not easy to lay down positive rules of action for all these supposable cases; but I trust that I have said enough to convince you of the importance of making the diagnosis of hysteric complaints a careful object of your future study.

I have hitherto spoken of hysteria as if it were exclusively a malady of females. Etymologically, to apply that term to the diseases of males would be absurd. But

that peculiar modification of the nervous system which is observed in hysteric girls does certainly present itself, though rarely, in young men. I have seen two or three instances of what I could give no other name to than hysteria, in males. One of them was in the person of a young surgeon who had been house-surgeon to the Middlesex Hospital. I believe he applied to not less than a dozen medical men for advice: and in that batch I happened to have my turn. He had some of the symptoms that are ascribed to hypochondriasis; *i. e.*, he was exceedingly attentive to his own sensations, and fancied that he laboured under a number of diseases which had no existence but in his own imagination; he showed great unsteadiness and infirmity of purpose; was what is called "very nervous;" and had occasional bursts of choking, and tears, and laughter, exactly resembling those which we so often witness in the other sex. Many cases of hysteria in the male have been recorded by different writers. The same moveable state of the nervous system, and the same symptoms referrible to that system, may exist in both sexes. In females, in nine cases out of ten, or in a much larger proportion, the exciting cause of the hysteria is connected with the sexual functions; and that is all that can be meant when it is asserted that, for the female, the complaint is not badly named, but has an intimate dependence upon the uterine sympathies. At the same time it is quite true that the "uterus is not the only organ of which the irritation may so affect the nervous system as to produce hysteria."

As in epilepsy, so also in hysteria, the *treatment* to be adopted regards, first, the paroxysm itself; secondly, the condition of the patient during the absence of the paroxysm.

One object, during the paroxysm, is to prevent the patient from injuring herself, by her hands, or by her teeth, or in her convulsive movements. Her dress should be loosened; but it may be necessary to confine her hands and arms. The next thing to be aimed at, is the putting an end to the fit. Various measures are found more or less useful for that purpose. The patient should be surrounded, as far as that is possible, with cool fresh air. If she be able to swallow, you may sometimes shorten the attack by administering a couple of ounces of the *mistura asafœtidæ*; or half a drachm of æther, with fifteen or twenty minims of laudanum in camphor julep; or a draught containing a drachm of the ammoniated tincture of valerian. When the patient cannot or will not swallow, she may sometimes be brought about by stimulating volatile substances offered to the nostrils. Signal good may also be effected by fœtid or stimulant enemata: the enema *asafœtidæ*, for example, made by mixing two drachms of *asafœtida*, with half a pint of water, by means of the yolk of an egg; or the turpentine injection, made in the same manner, and containing half an ounce of turpentine; or the same quantity of ice-cold water thrown into the rectum, or applied to the pudenda, will often bring the fit to a speedy termination. Indeed I believe there is more virtue in cold water, in hysterical diseases, than in any other single remedy. In the paroxysm it may be freely and repeatedly sprinkled, or dashed with some force, upon the face and chest. Active purges are beneficial and requisite in almost all these cases. There is commonly a costive, sometimes an obstinate, and always an unnatural, state of the bowels.

In those long paroxysms — if they may be so called — in which some other disease is simulated by hysteria, the cold affusion is a most valuable resource: especially in those forms of the disorder in which a limb is permanently bent, or incapable of motion. In several instances, in which such contraction had existed for a long time, it has yielded in the Middlesex Hospital, to a few minutes' application of the cold douche. Dr. Corfe, as I stated before, takes much pains with these cases. He pours cold water from a tea-kettle, or any other convenient vessel, in a small stream, from a moderate height, upon the contracted limb. It has been bent up for weeks perhaps; no power that you are able to exert can extend it; and any *very forcible* attempts to straighten it give the patient extreme pain. After the stream of water has been kept up for a short time, the patient complains of it very much; but Dr. Corfe is inflexible — more so than the culprit limb — he goes on. Presently the limb begins to tremble, the tight state of the muscles is evidently on the point of yielding, and in no long time they are entirely relaxed and manageable, and the member becomes as lithe and moveable as ever. It often happens that the state of contraction recurs; but a repetition of the douche has always the same good effect, and by degrees the habit is broken, and the patient set free. It requires some determination to put this expe-

dient in practice. The patient looks upon you as a monster of cruelty: and, in private, the friends will not always allow such "rough" treatment, as they consider it. Sir Charles Clarke, who necessarily saw a great number of these cases—they are more common in the upper than in the lower classes of society—is a great advocate of this ducking system. A paper of his upon the subject was read before the College of Physicians a few years ago. He recommends a "sudden and lavish" application of water to the face; or the immersion of the whole body. He describes the class of patients, in whom the hysterical affection which is curable by that method occurs, as being generally females of a pasty complexion, fat, pale, and weak; or such as evince the ordinary signs of debility, a feeble pulse, cold extremities, and purpleness of parts distant from the centre of circulation. The age of the patients varied from ten to thirty years; in many of them menstruation was imperfect, or absent.

A medical practitioner whom I met lately at a patient's house, told me he had just come from another patient, upon whom he had seen a surprising cure performed. A young lady, for many days, had been affected with trismus. She was unable to open her jaws, and therefore could neither speak nor eat. At last Sir C. Clarke was called in to see her. He presently comprehended the nature of her ailment, had her placed with her head hanging over a tub by the side of the bed; and proceeded to pour pitchers of water on her face. Before he had emptied the second, the patient began to scream and complain, giving very audible indications that she could open her mouth. I say although these patients get great relief by the treatment, they do not like it; and if they are convinced that it will be put in force, they will generally contrive not to require it.

Of all the spasmodic affections, hysteria is that which is most readily propagable by what may be called moral contagion. When, in a large ward, one girl goes off in a fit, half a dozen others perhaps, all who chance to possess the hysteric diathesis, will experience a strong inclination to follow her example. But this chorus, as it were, of hysteria, is much more common in some wards than in others. A stern nurse, or a general order that the cold affusion shall at once be employed in every instance of a hysterical fit, will keep the complaint wonderfully in check: and on the other hand, great sympathy with such patients has a striking effect in encouraging the paroxysms. These facts show that the symptoms are, to a certain degree, under the patient's control. The fits are not wholly wilful; neither are they wholly unconquerable.

I have but little to say respecting the medical management of such patients in the intervals between the paroxysms. The objects to be aimed at are, to restore the nervous system to the requisite degree of stability: and to correct the disordered functions of the uterine system. Now much the same plan of treatment is applicable to both these objects; and I spoke of the remedies that are found most beneficial for giving tone and firmness to the system, when I was upon the subject of epilepsy, and other nervous spasmodic ailments. The following points must be kept in view. The regulation of the bowels, which are mostly sluggish, by aloetic aperients; the exhibition of some form or other of steel; the steady employment of the shower-bath; regulated exercise both on foot and on horseback; the avoidance of hot rooms and of late hours, both in respect of going to bed, and of rising from it; the avoidance also of strong moral emotions, of novel reading, and of all the other thousand modes of dissipation, mental and bodily, which always accompany, and abate the blessings of, a high state of civilization. Marriage often proves a cure: sometimes it does not.

The disposition to hysterical disorder may be more easily prevented than cured; but upon this point medical men are not consulted. Parents do not foresee the misery they are often laying up for their daughters by the unnatural mode of life to which they are subjected, for the sake of filling them with fashionable accomplishments. I cannot close this subject, and this lecture, better than by quoting Sir Benjamin Brodie's remarks on the same point, as I find them in a little work recently published by him, and containing many highly valuable observations and instructions in respect to *local hysterical affections*.

"You can render (he says) no more essential service to the more affluent classes of society, than by availing yourselves of every opportunity of explaining to those among them who are parents, how much the ordinary system of education tends to engender the disposition to these diseases among their female children. If you would go further, so as to make them understand in what their error consists, what they

ought to do, and what they ought to leave undone, you need only point out the difference between the plans usually pursued in the bringing up of the two sexes. The boys are sent at an early age to school, where a large portion of their time is passed in taking exercise in the open air; while their sisters are confined to heated rooms, taking little exercise out of doors, and often none at all, except in a carriage. Then, for the most part, the latter spend much more of their time in actual study than the former. The mind is over-educated at the expense of the physical structure: and, after all with little advantage to the mind itself: for who can doubt that the principal object of this part of education ought to be, not so much to fill the mind with knowledge, as to train it to a right exercise of its intellectual and moral faculties; or that, other things being the same, this is more easily accomplished in those whose animal functions are preserved in a healthy state, than it is in others?"

LECTURE XXXIX.

Catalepsy. Ecstasy. Neuralgia: Tic-douloureux; Sciatica; Hemicrania.

THERE are yet some strange forms of nervous disorder which require to be mentioned; but upon which I do not intend to dwell. *Catalepsy* is one of these; and what is called *ecstasy* another. These affections are very rare as well as very wonderful: so wonderful and rare, that weak and superstitious persons have referred them to the interposition of supernatural agents in human affairs; and stronger-minded persons, who happen never to have witnessed such diseases, deny their occurrence as fabulous, or laugh at them as the tricks and cheatings of imposture. They certainly do happen, however; and they happen mostly in the same class of persons in whom hysterical and nervous complaints of all kinds are most common. They often appear to be produced by similar causes with these: they resemble hysteria in being seldom attended with any danger to life: their pathology is, if possible, still more obscure than that of hysteria: and if I were to speak of the treatment which would seem to be most suitable for their cure or prevention, I should merely have to repeat what I said, upon the treatment and prevention of hysteria, in yesterday's lecture. I shall content myself, therefore, with a short description of these two affections, that you may be aware of their characteristic phenomena, and not be taken by surprise in case either of them should occur to you in your practice.

A fit of catalepsy implies a sudden suspension of thought, of sensibility, and of voluntary motion; the patient remaining, during the paroxysm, in the position in which she (for it is almost always a female) happened to be at the instant of the attack, or in the position in which she may be placed during its continuance; and all this without any notable affection of the functions of organic life.

This is certainly a very curious state, and one different from any that we have yet contemplated. We have had the muscles rigidly contracted with tonic spasm, while the powers of the mind, and the sensibility of the body, were unimpaired. We have had the same muscles shaken with clonic convulsions; both with and without coexistent disorder of the intellectual functions. But here we have a new phenomenon: the mental faculties are in abeyance, and the sensibility is abolished, and so also is the function of voluntary motion; but the limbs are not tied down by spasm: nor agitated by successive contraction and relaxation of their muscles; nor yet left, like portions of dead matter, passively obedient to the laws of gravity: they assume any posture in which they may be placed, and that posture, however absurd, however (to all appearance) inconvenient and fatiguing, they retain, until some new force from without is applied to them, or until the paroxysm is at an end. The patient so affected, with open staring eyes often, and outstretched limbs, looks like a waxen figure; or an inanimate statue; or a frozen corpse. Indeed, Hoffman seems to have formed the strange

conclusion that, as catalepsy, so far as he knew, occurred most frequently in winter, it must depend on congelation of the nervous fluid.

These singular attacks occur in paroxysms; and they have been known to alternate with well-marked hysteria; and to take place in connexion with insanity. I have never seen an instance of perfect catalepsy, which I now regret, as I once had an opportunity of doing so, of which I did not avail myself. Dr. Gooch has described a case of it, as he witnessed the disease in a patient who suffered puerperal mania. She had long been subject to the common forms of hysteria. This is illustrative of what I have often stated respecting the consanguinity of these nervous disorders. It had become necessary to confine this patient in a strait-waistcoat; she was attended by Dr. Gooch and Dr. Sutherland. I will quote Dr. Gooch's account of the cataleptic state, for it is authentic and modern. He says, "A few days after our first visit we were summoned to observe a remarkable change in her symptoms: the attendants said she was dying or in a trance. She was lying in bed, motionless, and apparently senseless. It had been said that the pupils were dilated, and motionless, and some apprehensions of effusion on the brain had been entertained; but on coming to examine them closely, it was found that they readily contracted when the light fell upon them; her eyes were open, but no rising of the chest, no movements of the nostrils, no appearance of respiration, could be seen; the only signs of life were her warmth and pulse; the latter was, as we had hitherto observed it, weak, and about 120.

"The trunk of the body was now lifted, so as to form rather an obtuse angle with the limbs (a most uncomfortable posture); and there left with nothing to support it; there she continued sitting while we were asking questions and conversing; so that many minutes must have passed. One arm was now raised, then the other; and where they were left, there they remained. It was now a curious sight to see her, sitting up in bed, her eyes open, staring lifelessly, her arms outstretched, yet without any visible sign of animation. She was very thin and pallid, and looked like a corpse that had been propped up, and had stiffened in this attitude. We now took her out of bed, placed her upright, and endeavoured to rouse her by calling loudly in her ears; but in vain. She stood up, but as inanimate as a statue. The slightest push put her off her balance. No exertion was made to regain it. She would have fallen if I had not caught her.

"She went into this state three several times. The first time it lasted fourteen hours, the second time twelve hours, and the third time nine hours; with waking intervals of two days after the first fit, and one day after the second. After this the disease resumed the ordinary form of melancholia; and three months from the time of her delivery she was well enough to resume her domestic duties."

There is a minor form of this affection described, in which the patient is incapable of moving or speaking, but is conscious of all that goes on around him at the time. I saw a lady last year, who was subject to these attacks of imperfect catalepsy: which have been whimsically, but very expressively, called also attacks of *daymare*. From her time of life, her habits, and some other points in the history of the disease, I concluded that in her case these seizures, of temporary loss of muscular power without loss of consciousness, were dependent upon a diseased state of the blood-vessels of the brain. She afterwards consulted Dr. Chambers; and he told me that he had formed the same opinion of the nature and cause of the symptoms.

In what is called *ecstasy*, the state is different. The patient is lost to all external impressions; but wrapt and absorbed in some object of the imagination. The muscles are sometimes relaxed; sometimes rigid as in slight tetanus; but the loss of voluntary power over them is not complete or universal, for these patients often speak in a very earnest manner, or sing. They are, as the term *extasis* imports, out of the body at the time, wholly engrossed in some high contemplation. This state is not uncommon as forming a part of religious insanity; and sometimes it runs into ordinary hysteria. Nervous and susceptible persons are apt to be thrown into these trances under the influence, whatever it be, of mesmerism: and grave authors assure us that the intelligence which then deserts the brain, concentrates itself in the epigastrium; or at the tips of the fingers: that people in that state read letters which are placed upon their stomach, or applied to the soles of their feet; answer, oracularly, enigmatical questions; discover and declare their own and other persons' internal organs

diseases; describe minutely and accurately distant scenes which they have never visited, nor previously heard of; and even foretell future events. *Credat Judæus Apella, non ego.* I take for granted that they who were in the habit of speaking, a few years since, in some of our places of worship, in what they called *unknown tongues*, were either gross impostors, who deserved to be publicly whipped, or persons labouring under this disease, and wanting physic. Dr. Copland mentions a curious fact in connexion with this subject. He says that many of the Italian Improvisatori are in possession of their peculiar faculty only while they are in a state of ecstatic trance; and that few of them enjoy good health, or consider their gift as otherwise than something morbid.

Do not suppose, from what I have just said, that I ignore the glaring *facts* of mesmerism. I have seen and scrutinized too many of them to doubt their genuineness, or to ascribe them to mere imposture or collusion. You may desire to know, and you have a right to know, my creed upon this vexed and much-abused subject. It is this. Of the brain and nerves there are many and various strange conditions, which we reckon as manifestations of disease or disorder. They occur, as it would seem, spontaneously; or during the progress of some better known malady. I have given you several wonderful examples of such unnatural and morbid states under the heads of hysteria, chorea, catalepsy, ecstasy, trance. I might have described to you the curious condition, or alternating conditions, of *double-consciousness*; and the familiar phenomenon of *sleep-walking*. Now, whatever condition of this kind may arise thus spontaneously, may also, I believe, be produced, in some persons, under the mesmeric practisings. Not however through any material or occult influence emanating from the mesmeriser; but subjectively, from the mental attitude (if I may use that expression) in which the person mesmerised is led to place himself. There are many simpler forms of unconscious mesmerism, which we witness every day, without regarding or recognising them: the propagation of a yawn for example; the irresistible slumber under the drone of a dull preacher. Into these moods or postures of the mind, into this discord of the nervous functions, this disturbance of their natural harmony or equilibrium, the mesmeriser may beguile his victim: but the experiments of Mr. Braid show clearly that the agency of the mesmerist is not essential to the manifested effect. Any one, having the requisite susceptibility, may put himself into the mesmeric sleep or the mesmeric trance, by fixing his eyes and his attention steadily and unremittingly upon an object made to project a little way from the centre of his own forehead. The phenomena are subjective phenomena. The determining influence is from within as much as, or even more than, from without. Derangements such as sometimes occur thus in disease, may also sometimes occur under the mesmeric atmosphere: but no other or more mysterious derangements. Thus much I perfectly believe. I believe too that sleep may sometimes be conciliated by the monotonous biddings of mesmerism, when drugs might fail to procure it: and that such sleep may become sometimes, and in certain diseases, a mode and an instrument of cure. But I go no further. All the transcendental phenomena—the miraculous diagnoses and revelations, the clairvoyance, the prophecies—I class with the spirit-rappings and the table-turnings, as evidences of imposture on the one side and of miserable credulity on the other, and as alike scandalous in an age and country which vaunt themselves to be enlightened.

Leaving these nervous disorders, in which the function of voluntary motion is so curiously modified; and in which there sometimes is no alteration of the intellectual faculties, and sometimes very great disturbance, or the complete suspension of them; I would beg to turn your attention to another class of complaints, in which the nervous system is still the part principally interested, but in which the deviation from the natural state is manifested chiefly in the function of *sensation*: the powers of thought and of voluntary motion being scarcely affected, or not affected at all. Complaints, I mean, in which the sensibility is perverted, and augmented; cases of nervous *pain*. We have considered before that modification of sensation which consists in numbness, or *anæsthesia*, *i. e.* in the diminution of the natural sensibility, or its total privation. We have noticed also incidentally many perversions of sensation; such as giddiness, nausea, faintness, and the like; and in the same incidental way the morbid *exaltation* of the sensibility, which is called *pain*, has come before us, as a

symptom of various other diseases; of inflammation, and of hysteria. But there are diseases which consist of pain, and of nothing else, that we can perceive. They are often attended by no inflammation, no detectable change of structure in the painful part, no fever. These affections are included under the general term, *neuralgia*. Now pain is one of the things which we are oftenest consulted about; and these neuralgic pains are apt to be excessively severe and troublesome; and it cannot but be of importance to understand what has been ascertained of their nature, and causes, and capability of cure.

That pain is owing to some morbid condition or to some irritation of a particular nerve, we may sometimes know, by finding that it is felt exactly in the course, and follows the distribution, of that nerve. But when, as often happens, the pain is confined to a certain spot, we then conclude it to be neuralgic, if and because we can find no other explanation of its existence.

What increases the difficulty of making out the cause and origin of these nervous pains, is that they may be produced by some source of irritation operating at a distance from the part in which the pain is felt. It may be placed in the brain itself, or in the spinal cord; or in the trunk of the nerve that supplies the affected part; or in one of the branches of the same trunk, which branch is distributed to another part. If you strike the inside of the elbow in a certain way, so that the blow lights upon the ulnar nerve, a peculiar tingling sensation is felt in the little finger: that is, *not* in the part struck, but in the sentient extremity of the same nerve; and the same thing happens continually in disease. There is an excellent paper on this subject, by Sir Benjamin Brodie, published in one of the earlier volumes of the *Medical Gazette*, in which he has collected numerous and striking illustrations of the production of nervous pain by irritation situated in a distant part. Thus, to take a case in point; a man was admitted into St. George's Hospital on account of severe pain on the inner side of his knee. The joint was carefully examined, but no mark of disease could be detected in it. On tracing the limb upwards, however, an aneurism of the femoral artery, as big as an orange, was discovered in the thigh. This the patient thought nothing of; his only concern was the pain in his knee. Sir E. Home performed the usual operation for aneurism: and the moment the ligature was drawn firmly round the artery in the upper part of the thigh, the tumour ceased to pulsate, and the pain in the knee ceased also. This man died four or five days after the operation; and upon inspection of the limb after his death, the aneurism was found reduced to one-half its former size; and some branches of the anterior crural nerve, which passed over it, and which must have been kept on the stretch previously to the operation, were seen to terminate in the part to which the pain had been referred on the inside of the knee. There is just such another case related by Dr. Denmark, in the *Medico-Chirurgical Transactions*. A sailor was wounded by a musket-ball in the arm. The wound healed; but the patient remained affected with agonizing pain, beginning in the extremities of the thumb and fingers, except the little finger, and extending up the fore-arm. His sufferings were so great that he willingly submitted to have the limb amputated: and the operation gave him complete and immediate relief. When the severed limb was dissected, a small portion of lead, which seemed to have been detached from the ball when it struck against the bone, was found imbedded in the fibres of the median nerve.

These examples teach us, when we receive complaints of pain in any part, and can discover no cause of pain in the part itself, to look for some possible source of irritation in the trunk of the nerve, from which the part in question is supplied with nervous fibrils.

But the source of irritation may be further back than this: it may depend upon a diseased state of the spinal marrow, or of the brain. Of this, having had so many examples before us already, I need not seek for any new illustrations.

Sometimes, again, irritation applied in the course, or at the extremity of *one* branch of a nerve, will give rise to pain at the extremity of *another* branch of the same nerve. The sensation appears to be reflected, as it were, along the branch which is not directly the subject of the irritation. Thus filaments of the phrenic nerve penetrate the diaphragm and communicate with the ganglia that lie around the celiac artery; other filaments are distributed to some of the muscles about the shoulder;

and in this way has been explained the well-known fact, that disease or irritation of the liver is very apt to be accompanied with pain in the shoulder.

Thus also we have pain in the glans penis, from irritation of the bladder, produced by a stone there: pain of the thigh and testicle, from irritation of the kidney: pain of the left arm, from disease of the heart: pain in the feet, from stricture and irritation of the urethra.

There are many pains also, plainly enough connected with irritation of distant parts, although no other nervous connexion can be traced between the parts, except that which is afforded by the nervous centres. In such cases we must suppose that the morbid impression travels to the brain, and then the sensation is referred to the part affected through another nervous channel of communication. Dr. Wollaston was accustomed to relate the following story of himself. He had eaten some ice-cream after dinner one day; and his stomach did not seem to be capable of digesting it. Some time afterwards, when he had left the dinner table for the drawing room, he found himself rendered lame by a violent pain in one ankle. Suddenly he became sick, the ice-cream was vomited, and instantaneous relief of the pain followed its ejection from the stomach. "A gentleman (says Sir Benjamin Brodie) awoke in the middle of the night, labouring under a severe pain in one foot. At the same time certain other sensations, to which he was not unaccustomed, indicated the existence of an unusual quantity of acid in the stomach. To relieve the latter he swallowed a large dose of alkaline medicine. Immediately on the acid in the stomach having thus been neutralized, the pain in the foot left him."

The lesson that we learn from all these facts is this: that when we can find no explanation of a pain in the very spot in which it is felt, we should look for some condition that may explain it, in the trunk of the nerve supplying that part; or in the parts supplied by other branches of the same nerve; or (if still we are unsuccessful), we seek for other indications of disease in the brain or spinal marrow: and if these be wanting, we should extend our search, and inquire whether there be any intelligible disorder or cause of irritation elsewhere, which, operating through the medium of the nervous centres, may have occasioned the sympathetic pain of which our patient complains.

I say we should institute this quest, because, if it be successful, it may teach us, on the one hand, that the cause of the pain is fixed and irremediable; or, on the other, it may enable us by some simple and obvious expedient to cure the pain. But sometimes we shall be quite disappointed in all this seeking. We shall find nothing, either in the living patient, or in the dead body, which throws the smallest light upon the cause of the neuralgia.

Now with respect to those neuralgic pains, for which we can discover no adequate cause, either in any diseased structure, or in any morbid action of the blood-vessels, there are certain general facts observable which I will mention before I specify any particular forms of neuralgia. They occur in all parts of the body; but they are more frequent about the head than in any other part; and next of all, probably, in the abdomen. In the head, or face, the branches of the fifth pair of nerves are very frequently the seat of neuralgia; and to such pain, in that situation, the name of *tic douloureux* is generally given. The painful affection called *hemicrania* is another example of neuralgia of the head. Certain forms of *angina pectoris*, and of *gastrodynia*, seem to belong to the same class of disorders: and *sciatica* — which depends on different causes in different cases — is often rather a neuralgic than a rheumatic pain. I have stated that the pains sometimes follow the track of certain nerves; but this is not, I think, very common. Inflammation of the nerve, or of its investments, generally causes pain having that property; but the truly nervous pains are much better characterized by the suddenness with which they come on, and the suddenness with which they sometimes go off also; by their intermittence in many cases, and the regularity of the period at which they often, though not always, return; by the total absence (in most cases) of heat and swelling, and often of tenderness too, when they are external, and of febrile symptoms when they are internal, even although their intensity be extreme; by their apparent dependence, in numerous instances, upon sudden changes of the weather; by their occurring chiefly in persons of a nervous temperament in whom the health is otherwise disordered; and by their frequently abating under tonic remedies, or what are called specifics, rather than under anti-

phlogistic treatment. (*ALISON's Outlines.*) There is another circumstance, characteristic of these pains, which has been mentioned by Sir Benjamin Brodie, and I do not know that the same thing has been noticed by other writers. These pains are often suspended by sleep. "A person suffering from tic douloureux in the face may for a time be prevented from falling asleep, but if once asleep, his sleep is likely to be sound and uninterrupted for many hours." He says that though there may be exceptions to this rule, they are comparatively rare. Now this, you will observe, is quite analogous to what takes place in certain spasmodic affections of the muscles also. The jactitations of chorea are almost always suspended during sleep. It is the same, with the spasmodic wry neck, in which the involuntary contraction of some muscle, commonly the sterno-cleido-mastoideus, drags the chin round, and the head awry. Persons affected with that sort of deformity when awake, have their necks flexible enough, I believe, while they are sleeping.

I mentioned just now one character of these neuralgic pains, viz., the total absence in most cases, when they occupy the surface, of heat, redness, swelling, or tenderness; and I said *in most cases*, because there are unquestionably exceptions to this. After these pains have been long-continued and intense, they may give rise even to a moderate degree of inflammation of the part: which may become tender to the touch, manifestly vascular, and even swollen a little. "In a gentleman," mentioned by Sir B. Brodie, "who suffered for a great length of time what was regarded as a most severe tic douloureux in the face, at first the parts to which the pain was referred retained their natural appearance, but ultimately they became swollen, from an effusion of serum into the cellular texture, and so exquisitely tender that they could not bear the slightest touch. In a patient who had laboured for some time under pain in the testicle, depending on a calculus passing down the ureter into the bladder, the testicle became tender and considerably swelled."

The attacks of neuralgia may recur at intervals of a few seconds only; or they may take place daily, or every other day; or they may be separated by much longer intervals, regular or irregular. Sometimes there is continual pain, but it is wonderfully exalted and aggravated by fits. It is described as being sharp, sudden, twingeing, like an electric shock in its momentary duration. Sometimes it is attended by a feeling of constriction and cramp, although no muscular contraction accompanies it. I suppose that is one reason why such pain is so often spoken of even by medical men, and almost always by the vulgar, as *spasm*. Whenever a patient tells me he has spasms here or there, I am obliged to request that he will explain himself further. I want to know how he construes spasm; and nine times out of ten I find that he intends a sudden and sharpish, and generally a transitory attack of *pain*: whereas the term spasm really signifies, and ought to be restricted to, involuntary muscular contractions. When a medical man prints a case in which he states that spasms occurred in such or such a part, it is impossible to tell what he means, unless that term is explained by the context. Pray avoid this inexactness.

The most common of these neuralgic pains, as I have said, is that which has been called *τὰ ἐξοχρῆ*, *tic douloureux*, and which is situated in the facial branches of the fifth pair of nerves; nerves, as you know, of sensation; and it is usually restricted to one of the three branches that emerge severally to supply the parts in their neighbourhood. Sometimes two, sometimes all of them, are implicated. The middle one of these branches, the infra-orbitary, is, I believe, the most commonly affected in the severer forms of the complaint. The torture occasioned by this dreadful malady is sometimes excessive. The sufferers speak of it as anguish that is scarcely endurable; and you see, in their quivering features and restless limbs, that the acute bodily pang is, indeed, hard to bear.

When the uppermost branch of the trifacial nerve is the seat of the complaint, the pain generally shoots from the spot where the nerve issues through the superciliary hole; and it involves the parts adjacent, upon which the fibrils of the nerve are distributed; the forehead, the brow, the upper lid, sometimes the eyeball itself. The eye is usually closed during the paroxysm, and the skin of the forehead on that side corrugated. The neighbouring arteries throb, and a copious gush of tears takes place. In some instances the eye becomes blood-shot at each attack; and when the attacks are frequently repeated, this injection of the conjunctiva may become permanent.

When the pain depends upon a morbid condition, or morbid action, of the middle

branch of the nerve, it is sometimes quite sudden in its accession, and sometimes comes on rather more gradually; being preceded by a tickling or pricking sensation of the cheek, and by twitches of the lower eyelid. These symptoms are shortly followed by pain at the infra-orbitary foramen, spreading in severe flashes (so to speak) over the cheek, affecting the lower eyelid, ala nasi, and upper lip, and often terminating abruptly at the mesial line of the face. Sometimes it extends to the teeth, the antrum, the hard and soft palate, and even to the base of the tongue, and induces spasmodic contractions of the neighbouring muscles.

When the pain is referrible to the inferior or maxillary branch of the fifth pair of nerves, it darts from the mental foramen, radiating to the lips, the alveolar processes, the teeth, the chin, and to the side of the tongue. It often stops exactly at the symphysis of the chin. Frequently it extends in the other direction to the whole cheek and to the ear. During the paroxysm the features are liable to be distorted by spasmodic action of the muscles of the face, amounting sometimes to tetanic rigidity, and holding the jaw fixed and immoveable.

The paroxysms of suffering in this frightful disease are apt to be brought on by apparently trivial causes; by a slight touch, by a current of air blowing upon the face, by a sudden jar or shake of the bed in which the patient is lying, by a knock at the door, or even by directing the patient's attention to his malady by speaking of it, and asking him questions about it. This was remarkably manifest in a patient who came into the hospital under my care for another complaint; but who had for some time been subject to tic douloureux. The necessary movements of the face in speaking, or eating, are often sufficient to provoke or renew the paroxysm. At the same time firm pressure made upon the painful part frequently gives relief, and causes a sense of numbness to take the place of the previous agony.

This cruel malady occurs most commonly in persons who exhibit, in other respects, the signs of an unsound, or deranged, or debilitated system. It is more apt to fasten upon those who are pale, and asthenic, and upon individuals whose powers have been broken by advancing years. It is not unfrequently attended with some obvious disorder of the digestive organs, and ceases or is mitigated when that disorder is corrected. Sometimes it is clearly connected with a disposition to rheumatic affections; coming on in persons who suffer rheumatism in other parts, and even alternating with rheumatism in other textures. It is observed to be common among fishermen, and the inhabitants of marshy districts; and in some of these sufferers it may be attributable to their habitual exposure to cold and moisture. The trifacial nerve, lying superficially, and being unprotected by any artificial covering, is more likely, perhaps, for that reason, to be affected by vicissitudes of temperature; but in many of these cases the disease seems to be produced by the *malaria* which is prevalent in those situations. The paroxysms are then not only intermittent, but periodical, and they will frequently yield to the remedies which have been ascertained to be specific against ague and its various modifications. Sometimes the facial neuralgia is evidently dependent upon some general state of the system: for it will cease in the face, and fix itself in some other place; and in this way it may come to occupy several distant parts of the body in succession. There are other cases again in which the disease has a local origin, and results from some diseased bone, or exostosis, in the neighbourhood of the painful spot. The late Dr. Pemberton afforded a well-known example of this. He was seized with tic douloureux when in the very zenith of his reputation, and in the fullest exercise of his profession in this town. It completely ruined him: compelled him to give up business. He ultimately died of apoplexy. When his head was examined after death, the os frontis was found to be unusually thick, and on the falciform process of the dura mater, at a little distance from the crista galli, a small osseous substance was discovered, nearly half an inch long, and almost as broad. Sir Henry Hallford has recorded several other instances in which the disease was connected with some morbid condition of the bones of the head or face.

Now tic douloureux is one of those complaints for the cure of which there exists a number of specific remedies. But what I have been stating of this disease will suffice to convince you that, as it depends upon different causes in different persons, it is absurd to expect that any single drug—or even any one plan of treatment—will always remove it. Our first care, in every example of it that comes before us, must be to investigate all the particulars of the case. We must not be satisfied with

learning that the complaint is tic douloureux, and then go on prescribing one after another the reputed specifics for tic douloureux. It may happen that the origin of the disease is plain, and the remedy obvious. We must endeavour to make out whatever is amiss in the system at large, or in the state of particular functions. Very rarely, I believe, tic douloureux is dependent upon a condition of general plethora. Mr. John Scott gives the case of a gentleman who suffered severely from it for some time; at length he had an attack of apoplexy, and for this last disorder he was copiously bled, and the bleeding seemed to cure the neuralgia. Much more frequently we find evidence of a feeble or a shattered state of the system; debility and paleness; and then we may expect to do good by the treatment so strongly recommended by Mr. Hutchinson, viz., by giving the carbonate of iron. This remedy has been put largely to the test, since Mr. Hutchinson wrote in commendation of it, by Dr. Elliotson, and subsequently by others. Dr. Elliotson states it as the result of his experience that, "in all cases of neuralgia, whether exquisite or not, unaccompanied by inflammation, or evident existing cause, iron is the best remedy." I have already explained the manner of administering the carbonate of iron, the quantity in which it may be given, and the limits within which I should be inclined to restrict the doses. Sir Benjamin Brodie thinks it probable that the carbonate of iron proves beneficial by its mechanical operation on the internal surface of the intestines: but I should rather ascribe its good effect to the well-known property of preparations of iron, of giving firmness to the nervous system; apparently by increasing the quantity of red corpuscles in the circulating blood. However, it is of the utmost consequence that the state of the digestive organs should be attended to. Mr. Abernethy used to relate, in his lectures, many instances of tic which he had succeeded in curing by measures which were solely directed to the improvement of the stomach and bowels. He had a notion, that in patients who suffer under this disorder, there were always two functions wrong; those of the nervous system on the one hand, those of the digestive system on the other. And I am sure you will commonly find indications of a faulty state of both these systems. "The two," he used to say, "were the common parents of a numerous progeny of very dissimilar local diseases. In tic douloureux, you must seek to put the digestive organs right, or to soothe the nervous system, according as the one or the other may seem to be the principal and primary cause of the disease. Take away one of the parents, and there will be no more propagation."

In these cases, the unhealthy state of the digestive apparatus may be marked by obvious signs: a furred tongue, loss of appetite, costive bowels: or it may reveal itself by no other symptom than the pain. It may depend upon the mere presence of acid in the stomach. Dr. Rigby tells us that having suffered in his own person an intense attack of tic douloureux, which opium did not assuage, he swallowed, at the suggestion of a friend, some carbonate of soda dissolved in water. The effect was almost immediate: carbonic acid was eructed, and the pain quickly abated. More often the cause of offence appears to lie in some part of the intestines; and purgatives do good. Sir Charles Bell—drawing a bow at a venture—achieved the cure of a patient, upon whom much previous treatment had been expended in vain, by some pills composed of cathartic extract, croton oil, and galbanum. He mixes one or two drops of the oleum tigllii, with a drach of the compound extract of colocynth; and gives five grains of this mass, with ten grains of the compound galbanum pill, at bedtime. I mention the exact proportions and dose, because other cases have been since reported, both by Sir Charles and by others, in which the same prescription was followed by the same success.

When the disease occurs in a rheumatic individual, and especially when, as is sometimes the case, it alternates with rheumatism of other tissues, the remedies which have been found useful in rheumatism deserve a fair trial: guaiacum; colchicum; calomel and opium; iodide of potassium.

When all has been done that can be done towards restoring or improving the general health, we may turn our thoughts to local remedies. It is plain that these must be inefficient when the local pain results from constitutional causes that are undressed, or perhaps incurable. Yet even then topical measures may soothe the pain for a while.

One of these topical expedients, which promised well when first thought of, is the division of the trunk of the painful nerve, so as to cut off the nervous communication,

through that main channel at least, between the painful part and the brain. This was originally proposed by Dr. Haighton, and was at first attended with some little success; but in a great number of instances it has signally failed, as indeed might have been expected. In Dr. Pemberton's case the several branches of the fifth pair were cut by Sir Astley Cooper: but in vain. When there is any reason to think that the disease has a constitutional origin, or a local *distant* origin, the division, or even the excision, of a part of the nerve must be perfectly useless. It would be as reasonable (as Mr. Abernethy has observed) to expect to cure gout by cutting the nerve that goes to the great toe: or to perform castration with the view of remedying that pain in the testicle which is apt to be produced by the passage of a calculus through the ureter. Nevertheless there are cases, in which the division of the nerve, or some other surgical operation, is required. If you can make out that there is any tumour pressing upon or adherent to some part of the nerve—or if some foreign body, as a splinter or a shot, should be ascertained to be in contact with the surface of the nerve, or to be entangled in its substance—the tumour or the foreign body may be removed by the knife, with the strong expectation that a cure will be thus effected. And if this cannot be done, or if the nerve itself be altered in structure, either from disease or injury, (I am referring now to neuralgia in general, and not merely to that in which the facial branches of the fifth pair of nerves are implicated,) under those circumstances it will become a very proper subject of deliberation whether the nerve should be divided, or the limb amputated.

In the *Medical and Physical Journal* there is a case described by Mr. Jeffries, of a violent facial neuralgia, cured by the removal of a small fragment of china, which had been lodging in the cheek for fourteen years. And Mr. Descot mentions an instance in which a very severe affection, of ten years' standing, was removed by the abstraction of a carious tooth. I saw, not many days ago, a young woman whose finger had been amputated for very acute neuralgic pain which she had suffered in it; and the amputation had been successful in liberating her from that pain.

Sometimes we may hope to afford relief to the suffering patient by means which tend to remove or lessen the *exciting* cause of the paroxysms. Of this I may mention one remarkable example, which fell in part under my own observation: although I had nothing to do with the treatment. I was asked, a few years ago, by an acquaintance, to go with him to call upon a relation of his, who laboured, he said, under tic douloureux: he did not wish me to see her professionally, but was desirous that I should witness what he considered an extraordinary complaint. I saw a young girl, about twelve or thirteen years old, very pale and delicate, lying on a sofa; and I learned from her and from her mother that she was subject to the most excruciating agony in one side of her face and neck. The pain came on whenever she swallowed anything: the act of deglutition proved invariably the exciting cause of the torment. She was at that time under the care of a practitioner, who had desired that she might eat mutton-chops three or four times a-day. Of course this was a sentence full of misery to her; but so desirous was she to get rid of her disease, that she resolved steadily to follow the enjoined directions. This plan was to be tried for at least a month; after that time, if she were no better, her mother had resolved to consult another practitioner who had been much recommended to her. I should say that she had already consulted a great number of medical men; for the malady had existed nearly two years. At the end of the month she was worse than at the beginning; and the new practitioner, Mr. Pennington, was called in. He acted, like a man of sense and sagacity, upon the fact that the act of swallowing always gave rise to the pain; and he advised that she should not attempt to swallow for twenty-four hours. That period passed without any return of the pain; but it immediately recurred upon her eating a morsel of bread. The result of this experiment, however, encouraged him to hope that the morbid habit might be broken through by a sufficiently long abstinence from swallowing. And as she had been subjected to a great variety of fruitless treatment, he gave her no medicine, but advised that she should refrain altogether from taking food or drink by the mouth. Nourishing injections, composed of beef tea with an egg beat up in it, or of milk, were thrown into the rectum, two or three times a-day. This plan was persisted in for a longer time than I should have supposed she could have endured it. No nutriment whatever was taken by the mouth for five weeks and three days, and no paroxysm of pain occurred. At the end of that

period the pulse sank suddenly, from between seventy and eighty, to thirty-five beats in a minute; and thereupon Mr. Pennington, thinking he had carried his experiment far enough, deemed it advisable to administer by the mouth a desert-spoonful of beef-tea twice a day. This was continued for four days without producing any return of the pain. A small piece of fish was then allowed, and afterwards some chicken; and proceeding thus cautiously, in the course of a month she was able to eat and drink anything, without the slightest inconvenience.

I should state, however, that some time afterwards, the neuralgia returned in another situation, affecting the left knee: and this was remedied by a different mode of treatment. She is since dead.

Dr. James Arnott bears strong testimony to the power of *violent body exercise*, as a prompt and very agreeable remedy in neuralgic (and rheumatic) affections. He is enabled to speak from experience of its value in his own person, as well as in other instances. Having at one period of his life been tortured for six months by a neuralgic pain in his face, he resorted, upon a subsequent return of the pain, to the remedy just mentioned; and he succeeded on *every* occasion of its accession, whether by night or by day, in putting a stop to the pain by exercise with heavy dumb-bells, and particularly by retaining them as long as he could with outstretched arms, in a horizontal position. Two or three minutes, he says, of such exercise will generally be enough; but the amount of it requisite will probably be proportionate to the degree of the morbid affection.

When other means fail, or in conjunction with other means, local applications to the affected part may be tried. Belladonna will sometimes materially palliate the pain; so will opium; but within the last few years a new anodyne has been brought into use; and it really seems to have been of essential service in several instances of this most painful disorder. I allude to *aconitine*: the active principle of the monkshood. The property belonging to this plant, of benumbing sensation, has long been known. Sir Benjamin Brodie found, many years ago, that after chewing its leaves, a remarkable numbness of the lips was left, which lasted some hours. We may understand therefore the beneficial operation of the aconitine upon a part of which the sensibility is unduly exalted. It is but recently that pure aconitine has been procured; and consequently it has not yet been very extensively employed, and the less so on account of its very high price; but what experience we have of it, as a benumbing of pain, is highly encouraging. It has been of singular benefit to a surgeon who formerly lived in Charterhouse Square, and whose case is well known, I believe, to the profession. Mr. Spry had suffered greatly, for eight years, under very acute neuralgia, affecting the parts supplied by the lowermost or mental branch of the fifth pair of nerves. After exhausting almost every expedient that ever has been recommended for tic douloureux, except that of dividing the nerve, he was induced to make trial of the aconitine. It was mixed with cerate, in the proportion of one grain to one drachm, and a small portion of this was smeared over the track of the painful nerve once or twice a day for six days. By that time he had entirely lost the pain. He states, I understand, that the application of the ointment produced a sense of numbness, which continued for twelve, or eighteen hours. Dr. Hue, who first told me of Mr. Spry's case, told me at the same time that he knew of two others in which the same application had been equally successful. This encouraged me to try it upon my patient, whom I mentioned before, and who happened at that time to be in the hospital. I bought, for ten shillings, five grains of the aconitine at Mr. Morson's, in Southampton Row, where I knew it would be genuine. One-third of a drachm of ointment, containing one-third of a grain of the alkaloid, was smeared two or three times a day over my patient's face, and the attacks presently diminished in intensity, and in a few days ceased altogether. He soon after left the hospital, so that I cannot tell whether the cure was permanent. I presume it was so, as he did not return. The particulars of Mr. Spry's case have been published by Mr. Skey in the nineteenth volume of the *Medical Gazette*. It is now (1841) six years since the aconitine was applied, and the pain (as Mr. Skey has recently informed me) has never recurred. It used to be excited by gentle friction of the hand, or by a current of cold air, but Mr. Spry "can now face any wind or temperature with impunity."¹ In the same

¹ This gentleman is since dead.

paper Mr. Skey relates another instance of the utility of this substance in facial neuralgia. It occurred in one of his patients at St. Bartholomew's Hospital.

This is a remedy therefore which is not to be neglected. Even if it only allayed the pain for a time it would be highly valuable. But, judging from the instances now referred to, we may hope that, of some forms of tic douloureux, the aconitine may be found equal to the cure. It seems probable that the recurrence of the pain is sometimes kept up by the influence of habit; and will cease if the habit can for a while be broken. You must take care, however, to obtain a genuine article. The manufacture of aconitine is difficult, and therefore the cost is considerable. Mr. Skey, in the Bartholomew case, failed with some aconitine that had been imported into this country, but succeeded at once when he employed the same quantity of Mr. Morson's preparation. A strong and efficacious tincture of aconite is now in use. Dr. Hake has informed me that in a severe case of tic douloureux of the face, a saturated solution of iodine in this tincture achieved a cure, after the tincture alone had been applied in vain.

A few years ago Mr. John Scott published a little book on the disease we are now considering, with the professed object of introducing to general notice a species of local treatment which he had found successful in several long-standing and previously obstinate cases. It is well to be aware of these things, though probably the aconitine ointment will beat Mr. Scott's. Mr. Spry used Mr. Scott's ointment, but without benefit. It consists of the iodide of mercury, mixed with lard, in the proportion of two scruples to the ounce: and it is rubbed into, or placed in contact with, the affected surface, until some degree of irritation is produced.

Much may be hoped for, in this painful malady, from the cautious use of chloroform: present ease under its torturing paroxysms in any case; permanent ease when their recurrence depends, mainly or altogether, upon the force of habit. We must not expect that this anæsthetic agent will effect a cure if the pain result from some abiding cause of local irritation, or from some unredressed fault in the constitution at large. But in the worst instances it may abbreviate the periods of suffering, and give scope for the operation of measures more strictly sanative.

[In several cases of neuralgia, of different parts of the body, which, for a number of years, had resisted a variety of plans of treatment, we have known almost immediate relief, and in a few, a permanent removal of the disease, result from acupuncture. In other cases, however, no benefit has resulted from the operation. — C.]

There is a kind of *face-ache* which cannot properly be reckoned as a species of neuralgia, for it does not occur in short stabbing paroxysms, nor is the pain acute enough to entitle it to the name of tic douloureux; but which is very common, very distressing, and under ordinary treatment sometimes very intractable. It is called by some a rheumatic pain; it occupies the lower part of the face, the jaw principally, and the patient cannot tell you exactly whereabouts it is most intense. It is often thought to proceed from toothache, and bad or suspected teeth are extracted, but with no good effect. Now I allude to this for the sake of saying that some years ago I was instructed by an experienced old apothecary, that this *face-ache* might be almost always and speedily cured by the muriate of ammonia; — a medicine that we seldom give internally here, although it is so much used in Germany. And I have again and again availed myself of this hint, and been much thanked by my patients for the good I did them with this muriate of ammonia. It does not *always* succeed; but it *often* does. It should be given in half-drachm doses, dissolved in water, or in almost any vehicle, three or four times a-day. If the pain do not yield after four doses, you may cease to expect any benefit from it. In two or three instances of a similar kind that I have recently had to treat, I have found the iodide of potassium, in doses of five or six grains, work a speedy and permanent cure. This induces me to suppose that the pain in some of these cases is periosteal. I so judge from the ascertained efficacy of the iodide in other periosteal affections attended with pain.

Tic douloureux is the principal form of severe neuralgia which you may expect to meet with, in regard to acuteness of suffering and difficulty of cure. Two other forms, more common, and luckily more tractable, are generally spoken of under the

same head; *sciatica*, namely, and *hemicrania*. I have very little to say in this place, of either of these. *Sciatica*, or pain radiating from the sciatic notch, and following the course of the sciatic nerve, is sometimes an inflammatory complaint, and yields to the remedies of inflammation — cupping and blistering: sometimes it is plainly a part of rheumatism; and then may often be relieved by calomel and opium, or by colchicum: sometimes, again, it results from irritation within the pelvis, affecting the nerve before it emerges externally; this irritation may be connected with a disordered state of the kidney, and I suspect that it is in such cases that the oil of turpentine is of so much use: lastly, it is sometimes a purely nervous and neuralgic pain: and then the treatment applicable to facial neuralgia will, *mutatis mutandis*, be applicable to it. I had, some time ago, a butler under my care at the hospital, whom I am afraid I did not manage well. He suffered severe *sciatica*, and I had him cupped and blistered, and gave him a variety of medicines, for some time to little purpose: at last he got what I ought, I suppose, to have given him at first, viz., the carbonate of iron, and was presently well.

In a complaint so painful, so crippling, and oftentimes so obstinate, it is well to be provided with a succession of accredited remedies. One physician of my acquaintance cures all his cases of *sciatica* by putting a blister as large as half-a-crown over the spot where the nerve emerges, and sprinkling the surface exposed by the removal of the cuticle with half a grain or a grain of morphia in powder. Sulphur, again, has long been a popular remedy for rheumatism; and Dr. Fuller expresses his belief that in cases of rheumatic *sciatica*, unattended with fever, there is no external application so serviceable. The affected limb is swathed in flannel which has been thickly sprinkled with precipitated sulphur: over the flannel is placed a covering of oiled silk, or of thin gutta-percha; and the flannel bandages are kept thus applied night and day.

Hemicrania is simply headache, confined to one side, and occupying generally the brow and forehead, but sometimes affecting very exactly one moiety of the head. It is the *migraine* of the French, the *megrim* of our vernacular language; each of these terms being obviously traceable to the same Greek root. It is often attended with sickness; and in many instances it is periodical, coming on every day at a certain hour, lasting a certain time, and then subsiding. Like the other forms of neuralgia, *hemicrania* may be produced by various causes, which are, however, almost all of them such as tend to debilitate the system: it sometimes occurs in connexion with hysteria; sometimes it plagues women who have suckled their infants too long; sometimes it acknowledges the same cause as ague; and sometimes also it occurs independently of all other disease, and when no obvious exciting cause can be traced.

Whatever may be its origin, it is usually a very manageable complaint. When it is associated with evident anæmia, steel and the shower-bath may be expected to cure it. When its visits are strictly periodical, it will yield to quina. Arsenic is considered by many to have a specific power over the complaint; and I believe that four or six drops of the liquor arsenicalis, given three or four times a day, with due attention to the state of the bowels, will be almost sure to remove *hemicrania* in nine cases out of ten in which it occurs. But steel or bark, being milder and safer drugs, are *cæteris paribus*, to be preferred.

I say this disorder often acknowledges the same cause as ague; namely, the miasm of marshes, or malaria: and as that cause, mysterious as it is in some respects, exerts apparently its primary or chief influence upon the nervous system, and as ague has no definite seat in the human body, if it be not in the nervous system, I shall not find a more convenient place in these lectures for the consideration of ague than here, at the close of the remarks which I had to make respecting the diseases of the brain and nerves. In the next lecture, then, I shall begin to speak of Intermittent Fever.

LECTURE XL.

Intermittent Fever. Phenomena of an Ague Fit. Species and varieties of Intermittents. Predisposing causes. Exciting cause. Malaria: known only by its effects; places which it chiefly infests; conditions of its production; its effects upon the human body; influence of soils in evolving it.

I AM now to enter upon the consideration of that disorder of which the trivial English name is *ague*, and which is called by nosologists *intermittent fever*. This is one of the diseases which are known to us only in their respective group of symptoms. Before we can inquire successfully into its history, it is necessary that we have the group of symptoms which identify it set fairly before us. I must first, therefore, describe the *phenomena* of ague.

You will observe that ague resembles several other maladies that belong essentially to the nervous system, in being *paroxysmal*. A certain series of symptoms occurs, and then the patient reverts to a state of health: but this alternation commonly happens (or would happen if the disease were left to itself) a great many times. You may therefore look upon this succession of attacks as so many repetitions of a short distemper; or you may regard the whole period during which the attacks continue to recur at brief intervals, as being occupied with one single disease.

An ague fit is composed of three distinct stages; and they are severally named, from the phenomena that characterize them, the *cold*, the *hot*, and the *sweating* stage.

A person who is on the brink of a paroxysm of ague, experiences a sensation of debility and distress about his epigastrium; becomes weak, languid, listless, and unequal to bodily or mental exertion. He begins to sigh, to yawn, to stretch himself; and he soon feels chilly, particularly in the back along the course of the spine; the blood deserts the superficial capillaries; he grows pale, his features shrink, and his skin is rendered dry and rough, drawn up into little prominences, such as may at any time be produced by exposure to external cold, and presenting a surface somewhat like the skin of a plucked goose: hence it is called *goose's skin*, and in Latin *cutis anserina*. Presently the slight and fleeting sensation of cold, first felt creeping along the back, becomes more decided and more general: the patient *feels* very cold, and he *acts* and *looks* just as a man does who is exposed to intense cold, and subdued by it; he trembles and shivers all over; his teeth chatter, sometimes so violently that such as were loose have been shaken out; his knees knock together; his hair bristles slightly, from the constricted state of the integuments of the scalp; his cheeks, lips, ears, and nails turn blue; rings which before fitted closely to his fingers become loose; his respiration is quick and anxious; his pulse frequent sometimes, but feeble; and he complains of pains in his head, back, and loins; all the secretions are usually diminished; he may make water often, though generally he voids but little, and it is pale and aqueous; his bowels are confined, and his tongue is dry and white.

After this state of general distress has lasted for a certain time, it is succeeded by another of quite an opposite kind. The cold shivering begins to alternate with flushes of heat, which usually commence about the face and neck. By degrees the coldness ceases entirely; the skin recovers its natural colour and smoothness; the collapsed features and shrunk extremities resume their ordinary condition and bulk. But the reaction does not stop here; it goes beyond the healthy line. The face becomes red and turgid; the general surface hot and pungent and dry; the temples throb; a new kind of headache is induced; the pulse becomes full and strong, as well as rapid; the breathing is again deep, but oppressed; the urine is still scanty, but it is now high coloured; the patient is exceedingly uncomfortable and restless. At length another change comes over him: the skin, which, from being pale and rough, had become hot and level, but harsh, now recovers its natural softness; a moisture appears on the forehead and face; presently a copious and universal sweat breaks forth, with great relief to the feelings of the patient; the thirst ceases; the tongue

becomes moist; the urine plentiful but turbid; the pulse regains its natural force and frequency; the pains depart; and by and by the sweating also terminates, and the patient is again as well, or nearly as well, as ever.

This is surely a very remarkable sequence of phenomena: and it would appear still more remarkable if it were less familiar to us. The earlier symptoms are all indicative of debility, and of a depressed state of the nervous system. There is the same sensation of exhaustion, with incapacity of exertion, which is produced by fatigue. The sighing, yawning, and stretching, all notify debility. The paleness of the surface, and constriction of the skin, and collapse of the features, are all owing to the retirement of the blood from the superficial capillaries. The skin shrinks, but the parts containing the bulbs of the hairs cannot contract so much as the other parts, and therefore the surface becomes rough, and the hairs bristle up, or become erected in some degree. *Horripilatio* is the learned term for this state of the surface. The coldness of the skin is another consequence of the emptiness of its blood-vessels; and the tremors, which are always indicative of debility, seem to depend upon the coldness. The chattering of the jaws has been (it is said) so violent as to fracture the teeth. This you may believe or not as you please, but certainly the whole bed is often strongly shaken by the shiverings of the patient. The necessary accumulation of the blood in the larger and internal vessels offers a reasonable explanation of the distressed and anxious breathing.

In their attempts to render a "ratio symptomatum," authors have sometimes spoken of the hot stage as though it were a necessary consequence of the cold. But if the cold fit be in any sense or degree the cause of the hot fit, it can only be so partially. There must be some other cause; for these reasons. The cold stage may occur and never be followed by the hot; or the hot stage may come on without any previous cold stage; and when they do both happen, they are not by any means proportioned to each other. When we thus see that a supposed cause is not always followed by the effect, and that the effect is sometimes produced without the agency of the supposed cause, and also that the supposed cause and the effect are out of proportion to each other, we cannot but conclude that the supposed cause is at most but a partial and accessory cause. We can more easily conceive how the hot fit may conduce to bring on the sweating stage. The stronger action of the heart and the more forcible propulsion of the blood will fill the superficial vessels, and in this way the natural secretions may be restored. We see exactly the same thing happen when the force of the circulation is increased by exercise: the extreme vessels receive a larger supply of blood, and sweat ensues.

There are many curious facts to be observed in respect to the paroxysm of an intermittent, such as it has been now, in general terms, described. In the first place the paroxysm *returns*. Cullen makes this a part of his definition; and quibbling objections to his statement have been made, which are scarcely deserving of mention. Thus it is said that this circumstance should not have been introduced into the definition, because it is not *necessarily* or *universally* true; that the patient *may die* in the *very first* paroxysm; or that he may be cured by the proper remedies of ague, *before a second* paroxysm has time to show itself. But all this is captious trifling. The paroxysms, if the disease be left to itself, will recur for a certain length of time; and, unlike the paroxysms in many of the spasmodic diseases which we have lately been speaking of, they recur at regular periods, and often with singular punctuality. This is a circumstance which we should waste our time in attempting to account for. Dr. Cullen has tried to explain it on the principle of some diurnal *habit* of the body; but the truth is, that no *satisfactory* explanation of it has ever been given, and we must be content, for the present at least, to receive it as an ultimate fact; and doubtless a very strange and interesting fact.

For distinguishing some equally curious varieties of these successions and alternations of disorder and health, certain terms have, by common consent, been adopted by pathologists; which terms it is necessary that I should explain. The period that elapses between the *termination* of one paroxysm and the commencement of the next is called an *intermission*; while the period that intervenes between the *beginning* of one paroxysm and the beginning of the next, is called an *interval*. As the paroxysms are liable to vary in length, the intermissions may be very unequal, even when the intervals are the same. When the intermissions are perfect and complete, the

patient resuming the appearance and sensations of health, the disorder is an *intermittent* fever. When the intermissions are imperfect, the patient remaining ill and feverish and uncomfortable in a less degree than during the paroxysm, then the complaint is said to be a *remittent* fever.

But, confining ourselves for the present to intermittents, it is another curious property of this complaint that, although the intervals are commonly constant in each case, and quite regular, they differ in duration in different cases. Upon this circumstance is founded a division of agues into species. When the paroxysm occurs at the same hour *every day*, the patient is said to have *quotidian* ague. When it comes on at the same hour *every other day*, appearing and remaining absent day by day alternately, he is said to labour under *tertian* ague. The paroxysm, strictly speaking, repeats itself every *second* day: and if the species I first mentioned be fitly termed *quotidian*, that in which the fits occur on alternate days ought to be styled *secundan*. But nosologists have chosen to reckon the day on which the preceding fit happens as the first: and then the day on which the fit next to it will happen, in the species now under consideration, is the third. In the same way, when a paroxysm absents itself for two whole days, and then recurs, the complaint is called a *quartan* ague. These are the three principal species or types of intermittent fever. It follows, of course, from what I have been stating, that in the *quotidian* type the interval is twenty-four hours; in the *tertian*, forty-eight; and in the *quartan*, seventy-two.

Each of these types has some other characters peculiar to itself. Thus, the paroxysms of the *quotidian* ague begin in the morning; those of the *tertian*, at noon; those of the *quartan*, in the afternoon. These are the *rules*. You are not to expect to find them always or rigidly observed; for the most part you will find that they are observed. It is probable that *quotidian* paroxysms, occurring at noon or at night, have sometimes been ascribed to ague, when they were merely symptoms of some local disease or inflammation; or perhaps accessions of *hectic* fever. It is observed also of the paroxysms, that when the disease is about to yield, they often occur later day after day, before they take their final departure. This is called *postponing*, and when they occur earlier than their stated hour, the paroxysms are said to *anticipate*. Now a *postponing* *quotidian* may be deferred till noon. But when the disease is pursuing its regular undisturbed course, the rule is such as I have mentioned.

The three principal types differ from each other, not only in their respective intervals, and in the periods of the day at which the paroxysms severally commence, but also in the *duration* of the paroxysms; and in the proportions which the stages of these paroxysms bear to each other. The average duration of the paroxysm in the *quotidian* is ten or twelve hours; and of course the average duration of the intermission is nearly the same. The *tertian* paroxysm commonly begins at noon, and is finished the same evening; its average duration may be estimated at six or eight hours. And that of the *quartan* does not exceed four or six hours.

You must observe also that while the *quartan* has the longest interval and the shortest paroxysm, it has the longest cold stage; whereas the *quotidian* has the shortest interval and the shortest cold stage, but the longest paroxysm. To express these facts in mathematical language, the length of the paroxysm varies inversely as the length of the cold stage; inversely also as the length of the interval.

Of these three principal types or species, the *tertian* is by much the most common: but the *quotidian* and *quartan* are neither of them unfrequent wherever ague is rife.

I should tell you that there are other types also spoken of, as *quintans* and *sextans*: but they are scarcely worth our attention. It is probable that when they are noticed (and that is very rarely) they are merely irregular *quartans*, *postponing* perhaps for a day or two. They never prevail epidemically. Galen describes one of these; so does Van Swieten. Boerhaave talks of a *septiman*, and even *octavans* are mentioned; or if you desire still more of the marvellous, Pliny, the naturalist, informs us that a certain *Improvisatore* was in the habit of having a paroxysm once a year. It came exactly on his birth-day; yet he died at a good old age.

There are, however, some curious modifications of the three principal types; or rather of two of them, the *tertian* and the *quartan*. For instance, a paroxysm may occur daily, and yet the ague not be of the *quotidian* type, but of the *tertian*. The paroxysm of one day will differ from the paroxysm of the next, but exactly resemble that of the third day; while the paroxysm of the second day will be like that of the

fourth; and so on alternately. And these differences will be decidedly marked: the paroxysms of two consecutive days will come on at different hours, and will differ in duration and severity. This form of ague is called the *double tertian*. One case of this kind, very distinctly characterized, was some time ago under my care in the hospital.

There is another form of double tertian. Two fits shall occur on the same day—Monday, for example—one in the morning, the other in the evening; on Tuesday there shall be no fit; on Wednesday again two; on Thursday none; and so on. The Latin nomenclature is more precise than the English in denoting these variations. The form I have last mentioned, in which two dissimilar paroxysms occur every other day, is called *tertiana duplicata*, while the other form, in which there is a fit every day, but those on the alternate days resemble each other, is called *tertiana duplex*.

In the same way you may have a double quartan. In that case, a paroxysm occurs on two days in succession, and leaves the third day free; then it returns on the fourth day as it was on the first, and on the fifth as it was on the second, and leaves the sixth day free like the third, and so on. This is the *quartana duplex*. But two fits may happen on one day—say on Monday; none on Tuesday or Wednesday; and two again on Thursday. This is the *quartana duplicata*. Nay, the paroxysm of quartan ague may recur every day, and so far resemble a quotidian; but the fit of the first day will differ from those of the second and third, and resemble that of the fourth; the fit of the second day will be dissimilar from that of the first and that of the third, and like that of the fifth; and the fit of the third will be unlike that of either of the two preceding days, and find its counterpart in that of the sixth. This is a triple quartan: and where three paroxysms occur on the first day, which we will again suppose to be Monday, and none on Tuesday or Wednesday, but three again on Thursday, corresponding respectively to the first three, we have the *quartana triplicata*. And there are other complications still, with which I need not trouble you. In Dr. Cleghorn's book on the diseases of Minorca, you may find a very good and authentic account, evidently drawn from nature, of the irregular types and varieties of ague. They are well worthy of the attention of any among you who may be likely to practise abroad.

Some physicians have used the words *double tertian*, and so on, in the literal sense, and have supposed that two or more distinct agues coexisted. This savours of the error that I formerly warned you against, of looking upon diseases as separate entities, and not as merely modes of being and of acting, different from those which are proper to the state of health. The vulgar always regard disorders in this light. A coachman by whose side I sat while travelling from Broadstairs to Margate, was speaking of the rarity of ague in that part of the Isle of Thanet. His father, he said, once had the complaint, and a fit came on while he was on a visit to him, the coachman, at Rams-gate. The son administered to his suffering parent a glass of brandy: whereupon "he threw the agy off his stomach; and it looked for all the world like a lump of jelly." That was the only occasion on which he had ever "*seen the agy*."

Besides these varieties in *type*, some other deviations from the regular form of the paroxysm require to be noticed.

Sometimes the paroxysm is *incomplete*: it is shorn of one or more of its stages: the heat and sweating occur without any previous rigors; or the patient shakes, but has no subsequent heat; or the sweating stage is the only one of the three that manifests itself. These fragments of a fit are often noticeable when the complaint is about to take its departure; but they may also occur at other periods of the disease. Sometimes there is no distinct stage at all: but the patient experiences frequent and irregular chills, is languid and uneasy, and depressed. This state is commonly known among the inhabitants of our fenny and aguish districts as the *dumb ague*, or the *dead ague*; the patient is said not to *shake out*.

Again, there is often remarked a tendency to a change of type in the course of the same disease affecting the same person. Thus a quotidian may be transformed into a tertian; a tertian into a quartan; or, on the other hand, a quartan into either of these. I have already noticed the fact that the paroxysms may also alter their time of invasion, sometimes coming later and later in the day at each recurrence, sometimes earlier and earlier. When the paroxysm so *postpones*, the disease is getting milder: when

it *anticipates* its usual period of attack, the disease is increasing in severity. The postponement or anticipation, therefore, of the fit has a close relation to prognosis.

The most singular instance of variety that I have heard or read of, is referred to by Dr. Ranking (in his *Retrospective Address*, 1846), as recorded by M Maugenet. In this case the usual order of the stages was reversed. "The patient, upon each accession of the fit, was first attacked with profuse sweating, which lasted for an hour. Then the skin became dry and hot, and the face flushed, with headache, &c. This stage lasted ordinarily for five hours, when the patient began to feel cold, and eventually had distinct rigors." Quina was as effectual in this remarkable variety, as I shall presently show you that it is in the more regular form of ague.

There are yet other cases, in which from first to last no determinate type or order of succession is observed by the paroxysms; and these cases authors speak of as *erratic* forms of ague.

There are also many modifications or complications observable in the symptoms which constitute the fits. Occasionally each paroxysm is attended by violent delirium: this is most common, I believe, in the hot stage. This symptom has been known to be almost constant throughout an epidemic. Sometimes the patient is convulsed in the paroxysm; or syncope comes on; or tetanic rigidity; or petechiæ take place on the skin, and disappear with the paroxysm. These deviations from the common and regular kind and order of the symptoms may sometimes depend upon the constitutional predispositions of the person affected; but there is another way also in which they may be explained. I shall presently have a good deal to say upon the one grand—I may say *sole*—exciting cause of intermittents. Now exposure to that cause, a residence in aguish districts, will sometimes impart a periodic character to *other diseases*; and I apprehend that this explanation will apply to many of the instances which have been observed of hysterical, tetanic, or other paroxysmal complaints occurring at perfectly regular intervals.

The duration of ague—of the whole disease, and not merely of a separate paroxysm—it is not easy to estimate. If persons who laboured under it were always removed at once from the influence of the exciting cause, and were always suffered to remain without treatment calculated to check the malady, we might then find materials for determining its average natural duration. But we have not these data. In point of fact, ague sometimes consists of a very few paroxysms only, half a dozen, or four, or three, or even of one fit; and on the other hand they may be protracted over a space of several weeks, or months; nay, of many years.

An ague may attack a person at any time; but they are much more common in spring, and in autumn, than in the other seasons of the year; so that you will hear and read a good deal of *vernal* intermittents, and of *autumnal* intermittents. The autumnal agues are, *cæteris paribus*, the more severe and dangerous. The quotidian is most common in the spring; the quartan in the autumn; and the tertian is frequently met with both as a vernal and as an autumnal ague. You will bear in mind that in all this I am stating the prevailing *rules*; which are liable to numerous exceptions.

Ague is one of those disorders of which (as of common inflammation) all persons, at all periods of their existence, seem to be susceptible, when submitted to the influence of the specific exciting cause. Individuals of all ages, from sucking infants to persons of fourscore, are *liable* to it, but they are not *equally subject* to it. It is less likely (*cæteris paribus*) to affect the very young, and the aged, than those of middle life. However, the very old are by no means exempt from the operation of the cause of ague: and with respect to the very young, some extremely curious statements have been made. It is said that persons have had ague before they were born. We know that the period of intra-uterine life is obnoxious to many forms of disease; for we trace the consequences of such disease, in visible changes of structure, immediately after birth. Pulmonary tubercles constitute one malady to which the fœtus in utero is liable: hydrocephalus is another: acute inflammation of the peritoneum a third. And there can be no doubt that various specific poisons influence, occasionally, the included being, even although they may have no sensible effect upon the parent. The fœtus may thus contract small-pox, which sometimes proves fatal to it, sometimes not. The daughter of my bed-maker at Cambridge had a child ill of whooping-cough in the house with her while she was in the last months of pregnancy; and the infant in the

womb must have caught the disease, for I was assured that he hooped the very day he came into the world. The sins of the parent are thus visited often upon the child, when, before its first breath is drawn, its frame is contaminated by the virus of syphilis. And in like manner unborn infants are capable of being affected by the poison that produces ague. One case in proof of this recorded by Dr. Russell, in his *History of Aleppo*. The woman had tertian ague, which attacked her, of course, every other day: but on the alternate days, when she was well and free, she felt the child shake; so that they both had tertian ague, only their paroxysms happened on alternate days. Bark was prescribed for her; and it cured the little one first, and afterwards it cured the mother.

One probable reason why ague more commonly affects persons about the middle period of life, than those near its extremes, is, that the former are much more likely to be exposed to the primary exciting cause. And the same reason may be given, I presume, for another fact; viz., that the complaint is much more frequently seen in men than in women.

Among the circumstances which predispose to ague, debility has a powerful influence. It is important to be aware of this, as it concerns the prophylaxis, and the management of the patient after the disease has been subdued. Soldiers have been exposed to the exciting cause, without becoming affected by it, while strong and in good health; and have fallen ill of intermittent fever upon being weakened by exertion and fatigue. When I have told you that debility, any how produced, constitutes a predisposition to intermittent fever, I need scarcely add that all the multiform causes of debility may also be regarded as predisposing causes of this same disease; as they are of so many others.

But the strongest predisposing cause of all is an actual occurrence of the disease itself. The effect of former intermittents upon the system is such, that the complaint may be reproduced by agencies which under any other circumstances would be quite inoperative in exciting ague. I have stated already my persuasion that, strictly speaking, there is but one exciting cause of intermittent fever; but in making that statement I refer to its *first production*. The disease leaves the body in a condition in which other injurious influences may, of themselves, be sufficient to renew it. It brings into play a new order of exciting, or rather of re-exciting, causes. If a person were never exposed to the malaria, he would never, as I believe, have ague: but having once had ague, he may many times have it again, although he should never again be subjected to the direct influence of the malaria. The late Dr. James Gregory, of Edinburgh, had a brother-in-law who illustrated well in his own person the effects of predisposing circumstances in respect to ague. This gentleman was a strong active man, and commanded a battalion in the West Indies: and he escaped for a long time, while others were falling down around him in remittent fever. At last he was wounded by a musket-ball which passed through his shoulder. He insisted, much against the will of the surgeon of the regiment, on resuming his duties before his strength was completely restored; and the consequence was that he was immediately attacked by a remittent fever of such violence, that his life was for some time despaired of. But this was not all. The remittent disease assumed by degrees a distinctly intermittent form, and became a tertian: and at last he got well, and strong, and came over to this country. Yet for a long while, though to all appearance his health was re-established, ague-fits would from time to time occur; and they came precisely at the day and hour on which they would have happened if the tertian had continued with its original type; and slight causes were sufficient to reproduce them. He had marked, in an almanack, the days of the expected accession; and on those days it recurred, for some time, whenever the *east wind blew*. This very circumstance, the east wind, is a *common* re-exciting cause in such cases; exposure to cold in any way is another.

The curious fact which this instance exemplifies of the extension of the periodic law, in cases of relapse, throughout long intervals of apparent health, has since been noticed by Dr. Graves; who doubtless was not aware that Dr. Gregory had already observed the tendency thus lastingly impressed upon the system by the cause, or by the once occurrence, of an ague-fit. "The periodic rate is carried on, although (to quote Dr. Graves' happy illustration) as in a clock from which the striking weight

has been removed, the usual signal does not mark the termination of each certain definite portion of time."

The *exciting cause* of intermittent and remittent fevers—the primary exciting cause I mean, that without which ague would never occur at all—deserves a somewhat particular consideration. I need scarcely say that it consists in certain invisible effluvia or emanations from the surface of the earth, which were formerly called marsh miasmata, but to which it has, of late years, become fashionable to apply the foreign term *malaria*. In some respects the latter designation is the more convenient of the two.

The malaria is a specific poison, producing specific effects upon the human body. In its medical sense, it is not simply bad air, or impure air, although the word is loosely employed by many to express any mixed kind of contamination of the atmosphere. Thus we hear of the malaria of London: but ague, even when it occurs in London, is very seldom indeed, now-a-days, of London growth. The impure air incident to large and populous cities is prejudicial enough to health, as I formerly took occasion to show you, but it does not *generate* fever: neither continued fever, nor intermittent.

[If the author means to assert, as his language would seem to imply, that no form of fever whatever is generated by "the impure air incident to large and populous cities," but that all fevers are produced by its own "*specific poison*," resulting from other causes than those by which the atmosphere is rendered impure in the crowded, unventilated, and filthy lanes, courts, and alleys, which abound in suburbs of most large cities, he is certainly in error. That typhus, typhoid, and bilious fevers are generated in large and populous cities, no one, we presume, will pretend to deny; and evidence, as we think conclusive, has been adduced to show that the same is true of yellow fever; but of the dependence of either of those fevers upon a specific aerial poison we have not as yet been furnished with any satisfactory proof.—C.]

The emanations which cause ague have been called marsh miasmata, because they are notoriously common in marshy places. But they are not peculiar to marshy places. For this reason, and for brevity's sake, I prefer using the single word *malaria*. In this country, thank God, we witness its milder evils only, and those not very often; but it is the bane and scourge of large portions of the world. Whether you practise here or abroad, it is very fit that you should know the qualities, habitats, and habits, of this wide-spread poison. The mildest form of fever to which it gives birth is the intermittent fever, or ague; but in climates and places where it exists in greater abundance and intensity, the fever becomes remittent, or even assumes the continued form. This has led to strange errors, and proved a fertile source of difference and controversy amongst medical men: not a few of whom confound the severe continued fevers which spring from the malaria, and which are never contagious, with the severe continued fevers usually called typhous, which are unquestionably communicable from person to person.

The effluvia which thus form the sole exciting cause of intermittent and remittent fevers proceed from the surface of the earth, and are probably gaseous, or aëriform: at any rate they are involved in the atmosphere. But they are imperceptible by any of our senses. Of their physical or chemical qualities we really know nothing. We are made aware of their existence only by their noxious effects; and the inference that they exist was not made till within the last century and a half. Time out of mind, indeed, it had been matter of common observation that the inhabitants of wet and marshy situations were especially subject to these definite and unequivocal forms of disease. But the Italian physician, Lancisi, was the first, so far as I know, to put forth distinct ideas concerning malaria, in his book, published about 1695, *De noxiis paludum effluviis*. This is the great original work upon the subject.

To the production of this deleterious agent, a certain degree of temperature seems necessary. It does not appear to exist within the Arctic circle: nor does it manifest itself during the colder seasons of more temperate climates. It is very seldom traceable beyond the 56th degree of north latitude; and it is supposed to require for its development a continuous temperature higher than 60° of Fahrenheit's thermometer. The nearer we approach the equator, the more abundant, virulent, and pernicious

does the poison become, wherever it is evolved at all. In this climate it gives rise to intermittents, and principally to tertians. As we go south, in Spain, and along the shores of the Mediterranean, the remittent becomes the predominant form; and (what is very instructive) remittents there contracted often improve into intermittents upon the removal of the patient to a colder climate. Under the tropical heats, in the West Indies, for example, the fevers very frequently assume the continued form.

And another condition of the development of the poison soon becomes apparent. It requires a certain degree of moisture. Of all these regions, malaria, showing itself always by its effects alone, infests certain parts only; which parts are, most generally, remarkable for their humid and swampy character. Thus, in this island, intermittents are produced chiefly, I may say almost exclusively, along the eastern coast; in parts of Kent, Essex, Cambridgeshire, Norfolk, Lincolnshire, and the East Riding of Yorkshire: and in each of these counties there are marshes, or fens, or low grounds and lands that are occasionally overflowed with water. Many of these spots have, within the last fifty years, been drained, and brought under cultivation; and agues are consequently much more rare in England than they formerly were. In Sydenham's time they were very frequent, and very fatal indeed, in this metropolis. James I., and Oliver Cromwell, both died of ague contracted in London. At present (as I said before) we seldom meet with them. Except in the year 1827, I have never, since I have been in practice, known ague to be at all prevalent here. This comparative freedom from malaria is mainly owing, no doubt, to the improved character of the draining and sewerage.

Agues, or aguish fevers, are endemic along every part of the low and level coast of Holland. In Italy, the Pontine marshes, near Rome, have possessed for ages an infamous celebrity of the same kind. The whole of the district called the Maremma, —stretching for about thirty leagues along the shores of the Mediterranean, and in some places ten or twelve leagues broad—is rendered dangerous, and almost uninhabitable, by the vast quantity of malaria annually evolved from its soil. In America large districts are, for the same reason, prolific of disease. The late Bishop Heber, in his *Narrative of a Journey through the Upper Provinces of India*, gives the following striking picture of the influence of the malaria in that part of the world. It seems to be alike pestiferous to man and beast.

"I asked Mr. Boulderson if it were true that the monkeys forsook these woods during the unwholesome months. He answered that not the monkeys only, but everything which has the breath of life, instinctively deserts them from the beginning of April to October. The tigers go up to the hills; the antelopes and wild hogs make incursions into the cultivated plain; and those persons, such as dâk-bearers, or military officers, who are obliged to traverse the forest in the intervening months, agree that not so much as a bird can be heard or seen in the frightful solitude. Yet during the time of the heaviest rains, while the water falls in torrents, and the cloudy sky tends to prevent evaporation from the ground, the forest may be passed with tolerable safety. *It is in the extreme heat, and immediately after the rains have ceased*, in May, the latter end of August, and the early part of September, *that it is most deadly*. In October the animals return. By the latter end of that month the wood-cutters, and the cow-men, again venture, though cautiously. From the middle of November to March troops pass and repass, and with common precaution no risk is usually apprehended."

Persons who live in England might perhaps be disposed to think lightly of the malaria, had not such fearful evidence of its appalling power been brought home to the experience of our countrymen, in the early part of the present century, by the results of the unfortunate expedition to Walcheren. Sir Gilbert Blane has given an account of the ravages it there committed among our troops. You may see his paper, to which I shall presently again refer, in the third volume of the *Medico-Chirurgical Transactions*.

Not only a certain degree of heat, and a certain quantity of moisture, but the presence of all the four elements of the ancients, would appear to be requisite for the production of this poison. Air of course there must be; and earth also is essential. If heat and moisture were alone adequate, we should find the fever prevailing among sailors when out at sea; but it is not so, whatever may be the temperature under which they cruise. It is when they approach the coast, and land upon it, that they

are attacked. The water of marshes has been examined under the microscope, and analysed again and again, with a view to the discovery of the nature of this pestilential agent; but in vain. A more likely way to detect the noxious material would seem to be by examining the *air* of malarious districts; and this has been done carefully and repeatedly by expert chemists; and with the same want of success. The poisonous principle eludes the test of the most delicate chemical agents.

Where there is much heat, and much moisture, there we usually find also much and rank vegetation, and much vegetable dissolution and decay. The belief was as natural, therefore, as it has been general, that the putrefaction of vegetable matters was somehow or other requisite to the formation of the poison that exists so commonly in swampy situations. This belief has descended, almost unquestioned, from the time of Lancisi; and it obtains almost universal acceptance, I fancy, among physicians of the present day. Yet very strong facts have been adduced to show that the decomposition of vegetable substances is only an accidental, though a frequent, *accompaniment* of the miasm; and not by any means an essential condition of its evolution.

In the first place, the decomposition of vegetable matter goes on abundantly without the production of malaria. The rotting cabbage-leaves of Covent Garden, and those which taint the air of the streets from the neglected dust-holes of London, during the hot weather of summer, give rise to no ague. The same may be said of the putrefying and offensive sea-weed, which is deposited in large quantities upon some very healthy parts of our sea-coast. But the converse facts are the most remarkable and conclusive. I have stated that *marshes* are not necessary to produce malaria; but Dr. William Ferguson—a physician who had, and who well used, very sufficient opportunities of investigating the question—shows that *vegetation* is not necessary: that the peculiar poison may abound where there is no decaying vegetable matter, and no vegetable matter to decay. As the prevailing belief is, in my opinion, an erroneous one, and as it is really of great importance that correct views of this subject should be taken and disseminated by medical men, I will mention a few of the most striking of the facts detailed by Dr. Ferguson. They are contained in a very interesting paper “*On the Nature and History of the Marsh Poison*,” published in the *Edinburgh Philosophical Transactions*.

In August, 1794, after a very hot and dry summer, our army in Holland encamped at Rosendaal and Oosterhout. The soil, in both places, was a level plain of sand, with a perfectly dry surface, where no vegetation existed, or *could* exist, but stunted heath plants. It was universally percolated to within a few inches of the surface, with water which, so far from being putrid, was perfectly potable. Here fevers of the intermittent and remittent type appeared among the troops in great abundance. It is interesting to observe that the soil in Walcheren is precisely similar. Sir Gilbert Blane describes it as consisting “of a fine white sand, known in the eastern counties of England by the name of silt, and about a third part of clay.” It was after a dry and hot summer also that the British army suffered in that island from the endemic fever, to a degree which Dr. Ferguson speaks of as “being almost unprecedented in the annals of warfare.”

In the year 1809, several regiments of our army in Spain took up an encampment in a hilly ravine which had lately been a water-course. Pools of water still remained here and there among the rocks, so pure that the soldiers were anxious to bivouack near them for the sake of using the water. Several of the men were seized with violent remittent fever before they could move from the bivouack the next morning. “Till then (says Dr. Ferguson) it had always been believed amongst us that vegetable putrefaction (the humid decay of vegetables) was essential to the production of pestiferous miasmata; but in the instance of the half-dried ravine before us, from the stony bed of which (as soil never could lie for the torrents) the very existence even of vegetation was impossible, it proved as pestiferous as the bed of a fen.”

After the battle of Talavera, the army retreated along the course of the Guadiana river, into the plains of Estremadura. The country was so arid and dry for want of rain, that the Guadiana itself, and all the smaller streams, had in fact *ceased to be streams*, and were no more than lines of detached pools in the courses that had formerly been rivers. The troops there “suffered from remittent fevers of such destructive malignity, that the enemy, and all Europe, believed that the British host was extirpated.”

Cividid Rodrigo is situated on a rocky bank of the river Agueda, a remarkably clear stream : but the approach to it on the side of Portugal is through a bare open hollow country, that has been likened to the dried-up bed of an extensive lake ; and upon more than one occasion, when this low land, after having been flooded in the rainy season, had become as dry as a brick-ground, with the vegetation utterly burned up, there arose among our troops, fevers which, for malignity of type, could only be matched by those before mentioned on the Guadiana.

Many more facts to the same purpose are related in Dr. Ferguson's paper, which is in every way well worth your perusal. He tells us "that in the most unhealthy parts of Spain, we may in vain, towards the close of the summer, look for lakes, marshes, ditches, pools, or even vegetation. Spain, generally speaking, is then, though as prolific of endemic fever as Walcheren, beyond all doubt one of the driest countries of Europe ; and it is not till it has again been made one of the wettest, by the periodical rains, with its vegetation and aquatic weeds restored, that it can be called healthy, or even habitable with any degree of safety."

Our time will not allow of my extracting any further evidence on this point ; one circumstance of contrast, however, I am unwilling to omit.

The river Tagus is, at Lisbon, about two miles broad ; and it separates a healthy from a very unhealthy region. On the one side is a bare hilly country ; the foundation of the soil, and of the beds of the streams being rock, with free open water-courses among the hills. This is the healthy side. But the Alentejo land, on the other side, though as dry superficially, being perfectly flat and sandy, is most pestiferous. Moreover, in and near Lisbon there are numerous gardens, where they keep water, during the three months' absolute drought of the summer season, in stone reservoirs. These reservoirs, containing water in the most concentrated state of foulness and putridity, are placed close to the houses and sleeping rooms : the inhabitants literally live and breathe in their atmosphere. "Yet no one ever heard or dreamt of fever being generated amongst them from such a source ; though the most ignorant native is well aware that were he only to cross the river, and sleep on the sandy shores of the Alentejo, where a *particle* of water at that season had not been seen for *months*, and where water, being absorbed into the sand as soon as it fell, was *never* known to be *putrid*, he would run the greatest risk of being seized with remittent fever."

Now these facts, and facts like these, seem to prove that the malaria, and the product of vegetable decomposition, are two distinct things. They are often in company with each other, but they have no necessary connexion. Whoever, in a malarious country, waits for the evidence of putrefaction, will wait, says Dr. Ferguson, too long. For producing malaria it appears to be requisite that there should be a surface capable of absorbing moisture, and that this surface should be flooded and soaked with water, and then dried : and the higher the temperature, and the quicker the drying process, the more plentiful and the more virulent (more virulent probably because more plentiful) is the poison that is evolved.

The putrefaction of *animal* matter is sometimes spoken of as an element in the formation of the malarious poison. But the evidence I have just set before you refutes this supposition as completely as it excludes the alleged necessity of vegetable decay. I hope to prove to you, in a future part of the course, that neither animal nor vegetable decomposition is sufficient to generate fever of any kind.

Dr. Ferguson's facts are generally in accordance with the observations which others have made upon the same subject ; and his views will be found to account for some phenomena which the ordinary theory of vegetable putrefaction did not cleverly explain.

There is good reason for believing that in all cases the poisonous emanations proceed from parts of the surface that have been flooded and then dried, rather than from parts that are still wet or putrid. And this elucidates a circumstance very often noticed, viz., that neighboring places — especially high and low lands lying near each other — change their character in respect of salubrity upon the occurrence of rains. The low grounds, which had previously been very dangerous, become healthy when they are flooded over ; and the higher lands, which are made wet, and which rapidly dry again, produce the malaria abundantly. For the same reason, the edges or borders of swamps, which of course expand or contract according to the wetness or

dryness of the season, are more unsafe than their centres. The drying and half-dried margins of the purest streams may be prolific of the evil, when, from the want of confining banks, those margins have been flooded by the rising of the waters.

There is no observation more general than that, in malarious places, agues and remittent fevers abound more in hot and dry years than in those which are cold and moist. And this influence of temperature it is which mainly determines the differences observable in regard to these fevers at *different elevations*, and in *different seasons* of the year. In the higher grounds of the West Indies *agues* occur, as in this country: as you descend, and the mean atmospheric temperature increases, *remittents* are met with: and in the lowest and hottest parts the fever becomes *continued*. The following instructive facts are stated by Dr. Ferguson. In 1816, the British garrison of English Harbour, in Antigua, was disposed in three separate barracks, on fortified hills surrounding the dock-yard. One of the barracks was on an eminence named Monk's Hill, six hundred feet above the level of the marshes. The other two were situate on an eminence called the Ridge, one at the height of five hundred, and the other at the height of three hundred feet. So pestiferous were the marshes among which the dock-yard was placed, that it often happened to a well-seasoned soldier, coming down from Monk's Hill, and mounting the night-guard in perfect health, to be seized with furious delirium while standing sentry, and to expire within less than thirty hours after being carried up to his barracks, with a yellow skin, and having had black vomiting. Those in the barracks on Monk's Hill, *who did not come down*, the superior officers, the women, children, and drummers, had no fever of any kind. Seventeen artillerymen, in the barrack at the height of three hundred feet, did not come down to the night-guards. (We shall see hereafter that malarious places are always most dangerous at *night*.) Every one of these men was attacked with remittent fever, of which one of them died. At the barrack on the top of the Ridge, at the height of five hundred feet, there scarcely occurred any fever worthy of notice. Thus, *in the same place*, the malaria, in the level plain, caused continued fever, resembling, and I believe identical with, yellow fever: at the elevation of three hundred feet it gave rise to remittent fever: and at the height of five hundred or six hundred feet its influence was scarcely felt at all. In the neighbourhood of the Pontine marshes you see the villages perched curiously on the intervening hills; the Italians having been taught by experience that these elevated spots afford comparative security against the effects of the miasmata.

Wherever the malaria prevails, it produces its peculiar consequences chiefly in certain seasons: and it is in the autumn especially that agues and aguish fevers occur; that is to say, after the heats of summer: and the hotter and drier the preceding summer, the more frequent and fatal are the autumnal fevers. The Pontine marshes lie to the southward of Rome; and Horace, you know, says or sings,

*Frustra per autumnos nocentem
Corporibus metuemus austrum.*

The effects of these morbid effluvia upon the human body vary much under different circumstances. Where they are most concentrated and deadly, their operation may be almost immediate. Witness their speedy influence upon the soldiers who descended at night from Monk's Hill. So also sailors who have gone on shore for a single night only, have been attacked by the fever before they could return to the ship. On the other hand, when the emanations are less copious, or less virulent, there is sometimes a long and uncertain period of incubation. The disease remains latent, or the poison lies dormant, for a considerable space of time. Many of the soldiers who were exposed to the malaria at Walcheren did not experience its bad effects until after they had returned, and had even resided several months in England. In the same way, labourers, especially the itinerant Irish, will go down in the autumn for harvest work into Lincolnshire, and bring back the seeds of the disorder within them, and yet may not be attacked with ague for weeks or months; upon the occurrence of an east wind perhaps, or after unusual exposure to cold and wet. We trace, in all this, some analogy with the animal contagions; but the period of incubation is more irregular and accidental; and it probable that in many instances the ague would not happen at all, but for the concurrent operation of some other malign influence.

Another fact worthy of notice in respect to the agency of the malaria upon the

human frame, is that it affects strangers much more readily and decidedly than the natives of the place. In other words, habit mitigates the injurious effects of the poison. Persons become *seasoned* to it. At Walcheren, though almost every adult among the lower classes had laboured, in the course of his life, under the endemic intermittent, yet they were infinitely less subject to it than strangers: and they will not believe that their beloved birth-place is unhealthy. Sir Gilbert Blane says that persons of education, and even medical men, denied indignantly that their country was less healthy than any other; and attributed the sickness which raged among our troops to some trivial circumstance of diet or habits, and not to any insalubrity of the air. This is a curious moral feature; but a very general one. In the pestilential plains of Estremadura the superstitious natives, unable or unwilling to account for disease of a type so uncommon among the soldiers, from any unwholesomeness of the air, declared that they had all been poisoned by eating mushrooms.

It was found also, at Walcheren, that the strangers who survived the first attacks became thereafter much less liable to the endemic fevers. The French general, Monnet, who had held the command at Flushing for seven years, had acquired a knowledge of this fact, and endeavoured to turn it to practical account. He recommended that troops should not be frequently changed; for when it was the custom to send battalions from Bergen op Zoom every fourth night in succession, to work on the lines of Flushing, these men never failed, upon their return, to be taken ill in great numbers. General Monnet therefore advised, however displeasing it might be to the officers, that a stationary garrison should be retained at Walcheren, in order that the men might be habituated or seasoned to the air (*acclimatés*), and he adduced the instance of a French regiment which suffered in the second year of its being stationed there only one-half the sickness and mortality which it suffered during the first year; and hardly suffered at all in the third year.

But although the natives and residents in malarious places are not so liable as new comers to the violent and distinct forms of fever, they are chronically affected by the insalubrity of the atmosphere. They are spoken of by travellers as being puny, sallow, and sickly; feeble in body and spiritless in mind; as having yellow faces, swelled bellies, and wasted limbs; as being subject to dropsies and fluxes; phlegmatic, melancholy, and short-lived.

One remarkable exception is mentioned by Dr. Ferguson. From some peculiarity or idiosyncrasy (which he conjectures may be somehow connected with the texture of the skin) the negro appears to be proof against endemic fevers. "To him marsh miasmata are in fact no poison; and hence his incalculable value as a soldier, for field service, in the West Indies. The warm, moist, low, and leeward situations where these pernicious exhalations are generated and concentrated, prove to *him* congenial. He delights in them, for there he enjoys life and health, as much as his feelings are abhorrent to the currents of wind that sweep the mountain tops, where alone the whites find security against endemic fevers."

No very certain or extensive observations have yet been made in respect to the *kind of soil* from which the miasmata are most apt to be extricated. Such as is loose, penetrable, porous, and sandy, appears highly favourable to their formation. So are soils which, containing much clay, are very retentive of moisture. One curious fact, however, bearing upon this question, seems to have been made out: viz., that what is termed peat-bog, or peat-moss, is not productive of malaria. Many parts of Scotland and of Ireland, that are occupied by large tracts of marsh, in which the peat-moss abounds, are completely free from these fevers. Dr. Bisset affirms that the exhalations from black peat-moss do not occasion intermittents, "at least in high moors under a clear sharp air." Now in the climate of Virginia, this counteracting influence of a sharp air can scarcely be looked for: yet it is a remarkable fact, that though the provinces of North America, especially North and South Carolina and Virginia, are full of ague, that disease is never seen among the inhabitants near the country of the *Dismal Swamp*, a moist tract of 150,000 acres on the frontiers of Virginia and North Carolina. Weld, the traveller, informs us, that this immense tract is covered with trees, and abounds with water, which appears the moment the shallowest trench is dug. The water is brown, like brandy, but quite clear, and not unpalatable. Its colour is ascribed by the inhabitants to the roots of juniper; and it is said to be diuretic. (CRAIGIE.)

LECTURE XLI.

Ague, continued. Speculations respecting its periodicity. Habits and properties of the malaria: most noxious at night: lies near the ground: is carried along by winds: cannot pass across water: attaches itself to trees: is diminished by the increase of cultivation and of population. Ultimate effects of the poison on the body. Ague formerly thought salutary. Prognosis. Propriety of stopping the disease.

You will remember the progress we made, at our last meeting, in the subject of intermittent fever. I described the ordinary phenomena of a paroxysm of ague; and afterwards mentioned certain unusual symptoms with which it is sometimes complicated. The three principal types of ague were also delineated; the quotidian, the tertian, and the quartan: as well as their respective characters, and intervals, and varieties, and changes of type. I spoke too of the predisposing causes of intermittent fever, which may all be briefly included under the head of circumstances that tend to debilitate the body: the strongest predisposing cause of all being a former attack of the disease. And I began to consider the great exciting cause of agues and aguish fevers; the malaria. I first directed your attention to the circumstances under which the malaria appears to be evolved. Since the time of Lancisi it had been very generally supposed that the humid putrefaction of vegetable substances was necessary to the production of this peculiar and wide-spread poison; and that heat accelerated the putrefactive process. That was Dr. Bancroft's opinion. That also is (I believe) the opinion held, and stated in lectures, by many pathologists at the present time. I showed you, from facts which rest upon Dr. Ferguson's authority, that this notion is founded in mistake: that the products of vegetable decay and decomposition may and do often coexist with malaria, but are distinct and separable from it, and by no means essential to its formation. There is reason to believe that the flooding of a porous earthy surface with water, and a subsequent drying of that surface under a certain degree of heat, constitute the sole or main conditions of the generation of the poison. We found that the effects of the malaria are modified by the temperature of the place: that in low and hot situations it may give rise to an affection not distinguishable in its symptoms from yellow fever; and that in proportion as the locality is higher and cooler, the fever tends to assume first the remittent, and then the intermittent type: that the period of incubation—the period which intervenes between exposure to the malaria and the invasion of the fever—is extremely variable in duration: that the poisonous effluvia affect strangers more certainly and more severely than natives of the place: that persons may become in some sort seasoned to the malarious districts: but that, with the exception of the negroes in the West Indies, the inhabitants of places much infested with the peculiar miasmata, are feeble, and sickly, and short-lived.

There was one point which I briefly adverted to, and dismissed perhaps too unceremoniously: I mean the very curious fact of the *regular periodic recurrence* of the paroxysms of intermittent fever. I ought, I think, to have informed you of the views which pathologists have entertained respecting the explanation of that singular circumstance; although it must be confessed that the solution of the phenomenon is still to be sought for. A great number of persons have tried their hands, however, upon this question. Many of the earlier attempts at explanation are either quite hypothetical, or totally insufficient and illogical. Willis ascribed the intermission to a periodic development of the *fermentable matter* in the blood. But if any such development took place (of which we have no evidence) we should be no nearer the mark: the question would still recur, why the development of this matter should happen *periodically*: and the same remarks apply to various other so-called explanations brought forward by different writers of considerable reputation. Reil referred the intermit-
tence of fevers to some *general law of the universe*; by which he meant, I believe, some vague generalization of such facts as the alternation of light and darkness, the periodic recurrence of the seasons, the ebbing and flowing of the tides, the succession

of appetite and satiety, of the states of sleeping and waking, and so on: but this evidently is no explanation at all. M. Bailly offers a very singular conjecture upon the subject: he attributes the periodic phenomena to the modification necessarily induced in the human system, and particularly in the function of circulation, by the alternate change of position from the upright to the recumbent, and from the recumbent to the upright, every twenty-four hours; and he adduces in corroboration of this notion the alleged fact that animals, which undergo no such oscillation of posture, are not subject to intermittent fevers. But this is said not to be a fact. Rodet and Charpentier affirm that horses are liable to such complaints. Dr. Macculloch refers to the case of a dog which laboured under a regular tertian ague for some years: the cold paroxysms taking place always at three o'clock in the afternoon. Even if this were not so, M. Bailly's theory fails to account for the occurrence of *continued* fevers. If his views were correct, then we might avoid having ague by refraining from these changes of position from the vertical to the horizontal during sleep, and back again upon waking. More recently M. Roche has put forth the opinion that the attacks of ague are periodic, because the *causes* of them are periodic. And if this could be made out, the conjecture would carry with it some show of reason. He observes that the spring and the autumn are the seasons in which intermittent fevers chiefly break out, especially the autumn: and that during those periods there is a very sensible difference in the temperature and humidity of the atmosphere by day and by night, and even within the space of three or four hours; that a consequent alternation of action and reaction is thus produced in the human body, and soon becomes an established habit. Throughout a part of the twenty-four hours, the operation of the miasmata is slight, or not manifest at all; while during another part of that period it is in full energy, and at about the same time daily. The emanations (which he conceives to proceed from putrefying vegetable matter) are most abundantly disengaged during the hottest part of the day; these watery effluvia are dissolved by the warm air to a certain amount; but after sunset, they are again deposited, and deposited the more copiously in proportion to the coldness of the atmosphere at that time; and coming in contact with the surface of the body, with the mucous membrane of the air passage, and perhaps also with that of the digestive organs, and being absorbed by those surfaces, they occasion the phenomena which constitute an ague fit. The influence of the miasmata being intermittent, we need not wonder, he says, that their effects should be intermittent too: and then he goes on to ascribe the repetition of the paroxysms, after the cause has ceased to be applied, to that tendency observable in the animal system to reproduce certain actions, simply because they have been produced before: in one word, to the effect of *habit*. At length the habit wears out; which accounts for the spontaneous recovery of those who are removed from the malarious district.

It seems to be a very serious objection to M. Roche's theory, that the disease does not show itself, sometimes, for weeks or months after the patient has been exposed to the miasmata. His theory fails altogether also to account for the different *types* of intermittent fever. The differences of type are indeed *opposed* to the theory.

After all it is probable that Cullen had recognised a *part* though not the whole of the truth respecting the periodicity of intermittent fevers, when he ascribed it to some law of the animal economy whereby it is subjected, in many respects, to a diurnal revolution. "Whether it depends," he says, "upon the original conformation of the body, or upon certain powers constantly applied to it, and inducing a *habit*, I cannot positively determine; but the returns of sleep and watching, of appetites and excretions, and the changes which regularly occur in the state of the pulse, show sufficiently that in the human body a diurnal revolution takes place." But he also is much perplexed with the differences of type; and all that he can say on that point amounts to this — that as the three principal types observed, severally, a particular time of day for their accession, and as quartans and tertians are apt to become quotidian, these to pass into the state of remittents, and these last to become continued; and that as even in the continued form daily exacerbations and remissions are generally to be observed — all this attests the agency of a diurnal revolution. Suggestions as to some of the influences whereby this diurnal habit or variation may possibly be created and perpetuated, have been thrown out by Professor Laycock, of Edinburgh. "In the first place (I am quoting from a lecture of his published in the 38th volume of the *Medical Gazette*), we find that the atmospheric tides attain their maximum and

minimum at certain hours of the day: for there are tides in the circumambient atmosphere, as well as in the circumambient ocean; and therewith there are also changes in the electricity of the air, and the magnetism of the earth. From 8 to 10, A.M. and P.M., the barometer is at its maximum height; the electric tension is at its maximum too; and there is also the greatest maximum variation east of the magnetic needle at the same hours. From 4 to 5 o'clock, A.M. and P.M., the barometer is at its minimum, and so is also the electric tension. The respiratory movements, and of course the activity of the circulation, are likewise in connexion with these hours. About 4 or 5 o'clock in the morning, with a minimum temperature, a minimum electric tension, and a minimum height of the barometer, there is also a minimum consumption of oxygen. Further, I have ascertained by frequent inquiry, that sleep generally comes on about that hour after a feverish and restless night; and what is more remarkable, the statistics of deaths in York show that the chances are in the proportion of 3 to 2, that the last sleep — the sleep of death — will occur at that hour."

A most interesting experiment, as it appears to me, performed by M. Brachet upon himself, shows in a strong light the influence of acquired habit in continuing certain unnatural states of the system when once they have been originated: the experiment connects itself also with the peculiar phenomena of intermittent fever. Towards the end of the month of October, in the year 1822, M. Brachet took a cold bath, at midnight, for seven nights in succession, in the river Saone. On the first occasion he remained a quarter of an hour in the river; on the second half an hour; till at length he was able to stay in the water a full hour at a time. After each bath he betook himself to a warm bed, and in a short time became affected with considerable heat, followed by copious perspiration, in the midst of which he fell asleep. At the end of the seven days M. Brachet ceased to repeat this experiment; but what was his surprise at finding, on the following nights, between twelve and one o'clock, that all the phenomena of a true ague fit appeared in due order and succession! As, however, this artificial paroxysm was not very severe, and as he felt quite well during the day, M. Brachet determined not to interfere with it; but to observe the result. Six times it recurred with great regularity. On the seventh night after he omitted the baths, he was summoned, towards midnight, to a woman in labour: the ride to her house heated him, and on his arrival he kept up the heat by placing himself before a large fire, and from that time the febrile phenomena ceased to recur.

The facts and theories which I have thus brought roughly together, in respect to the periodicity of agues, are not without interest, but they show that we have yet much to learn on this subject. Granting that habit may have its share in continuing the regular recurrences, we want some explanation of the return of the second and third fit, after certain determinate intervals, to give a *beginning* to the habit. In respect to the quotidian, Dr. Cullen's *diurnal revolution* might come to the rescue; but this principle evidently will not apply to the tertian type. I know of no two-day, or bidual habit. And the objection holds still more strongly in regard to quartans. Indeed, in quotidians themselves there is much difficulty in applying the explanation, for though by anticipating, or postponing, they *may* come on at different hours of the day, yet their usual and natural paroxysms occur, not in the evening, but in the morning, when, on the principle of diurnal habit, there should be the *least* tendency to exacerbation of febrile action.¹

In yesterday's lecture, I pointed out the favourite *habitats*, if I may so speak, of the malarious poison. I have still a few observations to make respecting its ascertained habits and properties. Some of the laws to which it is subject are of great practical importance, and ought to be popularly known; much more ought every medical man to be familiar with them.

In the first place, all malarious districts are (as I have already hinted) much more dangerous *at night* than in the day time. Whether the poison be then more copiously evolved, or whether it be merely condensed and concentrated by the diminished temperature, or whether the body be at that time more susceptible of its influence, it certainly is most active and pernicious during the hours of darkness. To *sleep* at night in the open air in such places is almost to ensure an attack of the fever.

¹ I would beg to refer the reader to Dr. Holland's interesting chapter (in his *Medical Notes and Reflections*, published since these lectures were delivered) "On Morbid Actions of Intermittent Wind."

Lancisi was quite aware of this, and devotes a chapter to the question. "Cur juxta paludes noctu præsertim indormientes magis quam vigilantes lædantur." It has repeatedly been observed among the crews of ships, when off a malarious coast, that the sailors could go on shore in the day to cut wood, or for other purposes, with impunity, while the men who remained on shore through the night guarding the water casks, were many or all of them seized with the fever. Take one instance as a sample of many. It is recorded by Dr. Lind. In 1766 the Phoenix ship of war was returning from the coast of Guinea. The officers and ship's company were perfectly healthy till they touched at the island of St. Thomas. Here nearly all of them went on shore. Sixteen of the number remained for several *nights* on the island. Every one of these contracted the disorder, and thirteen of the sixteen died. The rest of the crew, consisting of 280 men, went in parties of twenty or thirty on shore in the day, and rambled about the island, hunting, shooting, and so on; but they returned to the ships at night; and not one of those who so returned suffered the slightest indisposition. Exactly similar events occurred the following year, with the same ship, at the same place, where "she lost eight men out of ten, who had imprudently remained *all the night on shore*;" while the rest of the ship's company, "who, after spending the greatest part of the day on shore, always returned to their vessel before night, continued in perfect health." Many more examples of the same kind are stated or referred to by Dr. Bancroft in his book on the *Yellow Fever*: a book which is rich in information respecting the malaria.

The reapers in the "Campo Morto"—a well-named part of the Maremma which I yesterday mentioned—are permitted to sleep for two hours about noon. They did so at that time without danger; but when the dews of evening have fallen down upon the earth, which serves them for their bed, it is then that the poison puts forth its most deadly power. Upon this principle Lancisi admonishes those who in summer travel through the Pontine marshes, not to do so *by night*, as many had been accustomed to do, in order to avoid the greater heat of the day: and similar advice is still given at Rome to all strangers. Though the passage requires but six or eight hours, there are numerous instances of travellers who, in consequence of their having crossed these fens during the night, have been attacked with violent and mortal fevers.

The practical lesson to be derived from a knowledge of this fact is too obvious to dwell upon. In malarious countries the open air at night must be avoided. "Early to bed" is always a good and wholesome rule; but the other half of the proverb "early to rise" becomes in such countries an unsafe precept. At least it is hazardous to leave the house early.

Secondly, the malaria *loves the ground*. It tends downwards. Whether this results from its specific gravity; or from its adhering to the moisture suspended in the lower strata of the atmosphere; or from some peculiar attraction for the earth's surface, I cannot tell you. There is reason to suppose that the poison combines somehow, or becomes entangled with fog: and fogs usually brood and settle, at night especially, upon the surface. This may be one reason why *lying down* to sleep in the open air at night is so very perilous; and why the *indormientes* suffer more than the *vigilantes*. The lower rooms of the same house may contain the noxious effluvia, while the upper are free. "In all malarious seasons and countries," says Dr. Ferguson, "the inhabitants of *ground floors* are uniformly affected in a greater proportion than those of the upper stories. According to official returns during the last sickly season at Barbadoes, the proportion of those taken ill with fever in the lower apartments of the barracks exceeded that of the upper by one-third, throughout the whole course of the epidemic. At the same time it was observed that the deep ditches of the forts, even though they contained no water—and still more the deep ravines of rivers and water-courses—abounded with the malarious poison." Dr. Hunter, in his work on the diseases of the army in Jamaica, says, "The barracks of Spanish Town consists of two floors, the first upon the ground, the second on the first. The difference in the health of the men on the two floors was so striking as to engage the attention of the Assembly of the island: and upon investigation it appeared that *three* were taken ill on the ground floor, for *one* on the other. The ground floor was not therefore used as a barrack afterwards." Mr. Ralph, in a table printed as an appendix to a paper of Dr. Ferguson's, in the eighth volume of the *Medico-Chirurgical*

Transactions, states the results of an inquiry into the comparative healthfulness of the upper and lower apartments of barracks in Barbadoes, to have been that the individuals residing in the lower apartments were attacked in the proportion of two to one of those living in the upper: and with certain apparent exceptions, which I shall notice presently, experience is uniformly in favour of the proposition that the poison is most prevalent and destructive near the surface of the earth, and does not rise high into the atmosphere.

To specify the sanitary precautions dictated by an acquaintance with this property of the malaria, must be quite superfluous.

Thirdly, the malaria is *moveable by the wind*. It is capable, therefore, of being carried from the spot where it was generated; and to other places which might else be free from it, and healthy. In this respect it is analogous to a heavy fog or vapour; and, in some cases, it is accompanied by a palpable mist; to which, perhaps, it may cling. The following passage relative to this subject occurs in Bishop Heber's *Journal*. "From Cheeta Talao our road lay through a deep and close forest, in the lower parts of which, even in the present season, the same thick milky vapour was hovering as that which I saw in the Terrai, and which is called *essence of owl*." This Terrai is the region which I mentioned in the last lecture as being so pestiferous, that it is deserted, during certain parts of the year, by every living creature.

This conveyance of the poison, like a cloud or fog, from one part of the surface of the ground to another, it is very important to attend to in all places; and especially so in tropical climates, where the wind blows for a long time together from the same quarter. We are thus enabled to account for the apparent exceptions to the last-mentioned property of the malaria, viz., its preference of low to elevated situations. You will readily understand how the miasmata may roll up, and hang accumulated upon, the side of a hill towards which a current of air sets steadily from or across a neighbouring marsh. Nay, the poison may be thus blown *over* a hill, and deposited on the other side of it. In this way, I presume, are to be explained the following curious facts, related in Dr. Ferguson's paper.

"The beautiful port of Prince Rupert's, in the island of Dominica, is a peninsula which comprehends two hills of a remarkable form, joined to the main land by a flat and very marshy square isthmus *to windward*, of about three-quarters of a mile in extent. The two hills jut right out on the same line into the sea, by which they are on three sides encompassed. The inner hill, of a slender pyramidal form, rises from a narrow base nearly perpendicular, above and across the marsh from sea to sea, so as completely to shut it out from the port. The outer hill is a round-backed bluff promontory, which breaks off abruptly in the manner of a precipice above the sea. Between the hills runs a very narrow clean valley, where all the establishments of the garrison were originally placed; the whole space within the peninsula being the driest, the cleanest, and the healthiest surface conceivable. It was speedily found that the barracks in the valley were very unhealthy; and to remedy this fault, advantage was taken of a recess or platform near the top of the inner hill, to construct a barrack which was completely concealed by the crest of the hill from the view of the marsh on the outside, and at least three hundred feet above it: but it proved to be pestiferous beyond belief. In fact no white man could possibly live there, and it was obliged to be abandoned. At the time this was going on, it was discovered that a quarter which had been built on the outer hill, on nearly the same line of elevation, and exactly five hundred yards further removed from the swamp, was perfectly healthy; not a single case of fever having occurred in it from the time it was built."

There is a striking anecdote given by Lancisi, showing, on a small scale, the effect of the wind in carrying the malaria with it. Thirty ladies and gentlemen had sailed to the mouth of the Tiber on an excursion of pleasure. Suddenly the breeze shifted to the south, and began to blow over a marshy tract of land situated to the windward of them. Twenty-nine of the thirty were immediately after attacked with tertian ague. So also Humboldt informs us that the town of Cariaco is afflicted with intermittents by the north-west wind conveying across it the miasmatic emanations of the Laguna of Campona.

And, as the wind may thus transport the malaria to a distance, and thereby render a spot unhealthy which naturally might be salubrious, so also it is often of service in clearing the poison from other places, and preventing its concentration.

A knowledge of these facts ought to be valuable in determining the choice of encampments, and of sites for dwelling-houses in aguish districts. Settlers in hot climates, especially where trade-winds prevail, would do well to avoid founding towns on the lee side of any swampy or suspicious ground. The outlets of rivers are commonly selected, for the convenience of commerce: and there is often a right and a wrong bank. I believe that most of the principal towns in the West Indies are built, for the advantage of the outward bound vessels, upon the western, or lee side of the islands.

Fourthly, it is a singular, but well-ascertained fact, that the miasmata lose their noxious properties *by passing over even a small surface of water*. Probably they are absorbed by it. And this is another mark of their tendency downwards. Many instances have already been referred to, where some of the crew of a ship have landed on a malarious coast, and have all been attacked by the fever; while the rest of the sailors, who remained on board, continued all healthy and well, though the ship was close to the shore. You could not have a better or more striking example of this than what took place at Walcheren. "Not only the crews of the ships in the road of Flushing were entirely free from the endemic, but also the guard-ships which were stationed in the narrow channel between this island (Walcheren) and Beveland. The width of this channel is about six thousand feet, yet, though some of the ships lay much nearer to one shore than to the other, there was no instance of any of the men or officers being taken ill with the same disorder as that with which the troops on shore were affected." This Sir Gilbert Blane has told us; and it is curious that Sir John Pringle made the very same remark in the very same place in 1747. He is speaking of the diseases of the campaign in Dutch Brabant: especially in reference to four battalions which had remained for some time in Zealand: and he says, "But Commodore Mitchell's squadron, which lay all this time at anchor in the channel between South Beveland and the island of Walcheren, in both which places the distemper raged, was neither afflicted with the fever nor the flux, but amidst all that sickness enjoyed perfect health; a proof that the moist and putrid air of the marshes was dissipated, or corrected, before it could reach them."

It is probable that this peculiarity has led to an erroneous and contracted estimate of the space through which the poisonous effluvia may be wafted, upon land, by the wind. Although the distance to which they are capable of being so conveyed, without losing their morbid power, has never been precisely defined, there can be no doubt that it is considerable. In Italy, according to Dr. Macculloch, the poisonous exhalations of the lake Agnano have been ascertained to reach as far as the convent of Camaldoli, situate on a high hill three miles distant.

I say the miasmata may probably be absorbed by the water over which they pass, or hang; and this reminds me of an opinion entertained by some writers, that the fever may be caused by drinking such water. The following interesting facts, suggestive of this notion, are recorded by Mr. Grainger in his Report on the Cholera of 1848-49.

Dr. and Mrs. Evans, of Bedford, were both attacked with ague while staying at Versailles in the year 1845. The water used there for domestic purposes is brought from the Seine at Marli. A large tank in which it was collected for distribution to a particular quarter happened at that time to be damaged; and the mayor of the place provided a new supply of water, consisting of the surface drainage of the surrounding country, which is marshy. This water the inhabitants of Versailles would not drink; but Dr. and Mrs. Evans, living at an hotel, drank of it unwittingly. It was made use of by a regiment of cavalry also. The result was, that they who drank the water suffered intermittent fever of so severe a type that seven or eight of the soldiers died of it in one day. Upon careful investigation it was ascertained that those only of the troops were attacked who had drunk the marsh water; all the rest, as well as the townspeople, having escaped, though all of them breathed the same atmosphere.

He quotes another instance from M. Boudin. "In July, 1834, 300 soldiers, all in good health, embarked on the same day in three transports at Bona, and arrived together at Marseilles. They were all exposed to the same atmospheric influences, and, with one essential difference, supplied with the same food, and subjected to the same discipline. On board one of the vessels were 120 soldiers. Of these, 13 died of a destructive fever during the voyage, and 88 more were taken to the Military

Hospital of the lazaretto at Marseilles, presenting all the pathological characters proper to marshy situations. It appeared upon inquiry that the water furnished to the soldiers on board the affected ship had been taken, in the hurry of embarkation, from a marshy place near Bona, while the crew, not one of whom was attacked, were provided with wholesome water. It was further ascertained that the nine soldiers who escaped the disease had purchased water from the crew, and had not partaken of the marsh water. Not a single soldier or sailor suffered in the other transports, which were supplied with pure water."

Dr. Snow, who refers to these statements in his interesting volume on cholera, cites this additional evidence to the same purpose from the *Report of the Poor Law Commissioners on the Sanitary Condition of Great Britain*. "Mr. William Blower, surgeon, of Bedford, states that typhus and ague, which had long infested the village of Wooton, near Bedford, had been much diminished by digging a few wells, and obtaining good water. He states, also, that in the neighbouring parish of Houghton, almost the only family which escaped ague at one time, was that of a respectable farmer who used well water, while all the other families had ditch-water only."

This theory, even if its truth be conceded, of the occasional conveyance of the ague poison into the stomach by means of water as a drink, is by no means inconsistent with the belief that it most commonly operates through the medium of the atmosphere, and enters the body with the breath.

Fifthly, another remarkable property of the marsh poison, is its attraction towards, and its adherence to, the foliage of lofty umbrageous trees; so that it is very dangerous, in malarious places, to go under large thick trees, and still more dangerous to sleep under them. But this property, thus a source of peril to those who are ignorant of it, affords, when known and rightly made use of, a mode of protection and remedy against the influence of the miasmata. In the territory of Guiana, where large trees abound, the settlers live fearlessly, and unhurt, close to the most pestiferous marshes, and to leeward of them, provided that a screen or belt of trees be interposed. New Amsterdam, in Berbice, lies on the lee side of an immense swampy forest, in the direct track of a strong trade-wind that blows night and day, and pollutes even the sleeping apartments of the town with the stench of the marshes; yet it brings no fevers. The inhabitants are well aware that it would be almost certain death for a European to sleep, or even to remain after nightfall, within the verge of the forest. To cut down the trees would not only be a perilous operation in itself; but would let in pestilence upon the town.

This property also of the malaria, as well as the use to which it may be turned, was known to Lancisi. He describes the vast increase of agues and remittent fevers in Rome during the summer of 1695, after a great overflowing of the Tiber, by which the lower part of the city, and the fields adjacent, had been inundated in the preceding winter. The bad effects of this flood were felt throughout the whole of Rome, with the exception of one particular quarter, which was protected by a belt of trees around it. Lancisi even addressed a remonstrance to the Pope against a project which was entertained of felling some wood near the Pontine marshes, between them and the city. He endeavours to show that woods and groves were first made sacred, on account of their conservative influence in this way, to prevent their ever being cut down.

It would appear, from the facts I have just been detailing, that dwellings unfortunately built in the vicinity of marshes, might sometimes be rendered salubrious and safe by encircling them at a little distance by a hedge of trees—or (perhaps) even by drawing round them a broad moat of water. Such expedients, deserve, at least, a fair trial.

Sixthly, the generation and consequently the effects, of the malaria are prevented, or lessened, by culture of the soil. It is to this, that the diminution of agues in this country is mainly attributable. The fenny lands have been drained; and much of them brought under the plough. Dr. Craigie states that East Lothian, in Scotland, was at one time so productive of malaria, that for the reapers in harvest to be attacked with ague was quite a thing expected; but that now, in consequence of the perfect tillage, and the numerous tracts of wood with which the country is covered, the disorder is quite unknown there. Conversely, in regions which have been suffered to fall out of cultivation, intermittent and remittent fevers multiply. The more thoroughly

any country is cultivated, the more fully, in general, is it *peopled* also: and in many places the prevalence of these fevers has been observed to diminish and increase with the increase and diminution of the population. *Cæteris paribus*, agues are much less common in large towns than in country villages. This has been oddly enough accounted for by saying that populous cities are so full that there is no room for the malaria. A much more rational and probable explanation is that which attributes the freedom of crowded towns, and thickly inhabited districts, to the number of fires burned in them.

Many instances might be adduced to show that the more any place, naturally producing malaria, is depopulated, the more evident does the power of the poison become. The Italians date the introduction of the malaria into the Maremma, from the great plague in the sixteenth century, since which period the inhabitants of that district have never been sufficiently numerous to counteract the bad air, which increases as population and agriculture diminish.

Bishop Heber, in the narrative I quoted before, bears testimony to facts of the same kind with those I have now been stating. He says, "At the foot of the lowest hills, a long black level line extends, so black and level, that it might seem to have been drawn with ink and a ruler. This is the forest, from which we are still removed several coss, though the country already begins to partake of its insalubrity. It is remarkable that this insalubrity is said to have greatly increased in the last fifteen years. Before that time, Ruderpoor, where now the soldiers and servants of the Police Thanna die off so fast that they can scarcely keep up the establishment, was a large and wealthy place, inhabited all the year through, without danger or disease. The unfavourable change is imputed by the natives themselves to depopulation. The depopulation of these countries arose from the invasion of Meer Khân, in 1805. He then laid waste all these Pergunnahs, and the population, once so checked, has never recovered itself."

When persons having intermittent fever are unable to leave the unhealthy situation in which they have been exposed to the influence of the malaria—and especially when they are placed under unfavourable circumstances in respect to food, clothing, and shelter—the disease is apt to become exceedingly serious, leading to disorder of the sensorium, and great disturbance of the abdominal viscera, even in the intermissions; sickness, diarrhœa, dysentery, diseases of the liver.

[Dr. S. A. Cartwright, of Natchez, asserts, that the *jussiaea grandiflora*, or floating plant of the bayous and lakes of lower Louisiana, has the power of preventing the development of malaria in regions particularly adapted to its generation. He affirms, also, that "it purifies all stagnant water in which it grows—that of the lakes and bayous inhabited by it, being as pure to the sight, taste, and smell, as if it had just fallen from the clouds." He ascribes to the presence of and the peculiar "hygienic or health-preserving properties of this plant," the remarkable exemption of the inhabitants of lower Louisiana from "malarious or miasmatic diseases." "The fact," he remarks, "that the region of country in which this aquatic plant abounds, is exceedingly healthy, can be established beyond cavil or dispute; it nevertheless contains more stagnant water and swamps than any other inhabited district, of the same extent, in the United States."—C.]

In Zealand, the biliary functions suffer so much during the complaint, that it is commonly known among the inhabitants of that country under the name of the *gall fever*. The frequent unnatural concentration of the blood in the internal parts may afford a reasonable explanation of these phenomena. When death takes place, morbid appearances present themselves such as might be expected; hepatic alterations; inflammation and ulceration of the mucous membrane of the alimentary canal: but the most characteristic morbid condition produced by repeated attacks of intermittent fever consists in enlargement of the *spleen*; with or without induration of its substance. That viscus is sometimes enormously increased in bulk, so as to be *felt*, and even its outline *seen* through the integuments of the abdomen. It has been known to weigh nearly eleven pounds. So common is this state of the spleen that it is familiar to the observation of the vulgar, who have even given it a name; it is called among

the inhabitants of the fenny parts of this country, the *ague cake*. I believe that whenever the abdominal circulation is much embarrassed, and the abdominal veins gorged, as they must be during the cold stage of an intermittent, the spleen in particular becomes distended with blood. Constantly we see this happen when the passage of the blood through the portal vessels is impeded by disease of the liver. Now this distension may not thoroughly subside perhaps at once. If the paroxysms of ague be frequently repeated, we may understand how the spleen may become fuller of blood on each successive occasion. It may be that a portion of the blood coagulates; or that inflammation of a slow kind is set up in the stretched covering of that organ. At all events, this is a very common sequel of ague: and it can scarcely be doubted that the repeated congestions of the internal vessels and viscera are the determining causes of the ague cake.

Independently of the paroxysm of ague, there is ample evidence to show the injurious influence of the malarious districts upon the general health. In this country such effects are not much seen; but in places where the malaria is more constantly and abundantly present, the race of inhabitants deteriorates. Their stature is small; their complexion sallow and yellowish; they are prematurely old and wrinkled; even the children early acquire an aged aspect; and the spirits and intellects of those who dwell in these unhealthy spots are low and feeble, and partake of the degeneration of their bodily qualities.

It is therefore strange that a notion should ever have prevailed, of the *salutiferous* effects of an attack of ague. But such a notion may be traced from very nearly our own times up to the earliest records of physic. The late Dr. James Sims, who was a physician of some note in this town, felt convinced, at the commencement of the illness which terminated his life, that he should recover if he could catch an ague: and he went down into one of the marshy districts expressly for that purpose; but returned to London without having succeeded, complaining that the country had been spoiled by draining, and that there were no agues to catch. The superstitious Louis XI. entertained a similar opinion, and prayed to the Lady of Selles that she would confer upon him a quartan ague. Our monarch, James the First, had more sensible notions on that score. There is an old English proverb which says, "An ague in the spring, is physic for a king;" and when this was repeated to him by his courtiers, he, being then ill of that disease, answered that the adage might be applicable to a young man, but that it would not do for an old one like him. In fact, as I mentioned before, he died of his ague. The same doctrine has, however, been handed down to us by the father of physic himself. Hippocrates says, in the fifty-seventh Aphorism of his fourth Section, *υπο σπασμον, η τετανου ενοχλονομενο, πυρετος επιγενομενος λειπει το νοσημα*. And Celsus, in his capital digest of the medical knowledge of his time, preserves the same opinion, with some apparent astonishment that it should be true. "Denique ipsa febris, quod maxime mirum videri potest, sæpe præsidio est." I recollect hearing Dr. Graham, the professor of botany in Edinburgh, relate the following anecdote in one of his clinical lectures:—His brother was intimate with the professor of natural history at Cremona; and this gentleman was resolved to put the truth of the aphorism that I have quoted from Hippocrates to the test. Accordingly he sent a patient afflicted with *epilepsy*, to pass a night or two in a marshy place, where the malaria was known to be so abundant, and so powerful that few escaped ague, who were there exposed to its influence; and the two-fold design succeeded admirably. The patient got an ague, and lost his epilepsy. The worthy professor contented himself with moderating and keeping in check the new complaint, thus intentionally produced, for a period of six months, when he administered its *coup de grace* in a few doses of Peruvian bark; and the epilepsy never returned. If I had believed that this could have been anything more than a mere coincidence, I should have told you of it before, when I was speaking of the treatment of epilepsy. I should rather imagine the notion thus prevalent for so long a time, that ague had a salutary tendency, and that it was wrong to stop it too soon, to have originated in the difficulty which physicians found in stopping it, before its cause was so well understood, and the specific for it was discovered. They found it obstinate under the feeble and inert methods then employed, and therefore they endeavoured to persuade their patients, and perhaps themselves also, that the disease had better proceed a certain length.

I have very little to say, in addition to what you must have inferred from what I have already said, as to the *prognosis* in intermittent fevers. In cold countries, such as ours, it is almost always favourable. Of course it will be modified by the previous condition of the patient: if he were beforehand the subject of serious organic visceral disease; or if he be very old or infirm; the supervention of ague may destroy him. But to persons of tolerable health and strength prior to the setting in of the ague, we may confidently promise a *cure*. In warm countries intermittent fevers are much more dangerous: and are sometimes very rapidly fatal. They are often accompanied by most severe affections of the head, stupor, delirium, convulsions; and of the alimentary canal, diarrhœa, sickness, and not unfrequently the black vomit. They are prone also, in those climates, to run into the remittent or continued form; and this tendency is shown by long protracted paroxysms, or by the anticipation or doubling of the paroxysms. In all countries quartans are cured with more difficulty than either tertians or quotidianas. And quartans are most common in the autumn: and accordingly autumnal intermittents are more pernicious and intractable than the vernal. This fact has passed into a proverb in Italy; which proverb has been thus translated into Latin, "*Febris autumnalis — vel est longa, vel lethalis.*" The longer intermittents have lasted, the more difficult also are they to cure; and certainly there is much more danger of *visceral disease* in those that are of long standing.

It is probable that agues, such as we see in this country, would generally, under favourable circumstances, terminate in spontaneous recovery, provided that the patient could be put beyond the further operation of the malaria, protected from exposure to wet and cold, and suitably nourished. But possessing as we do a specific cure for ague, there would be no sense in our allowing the experiment of a spontaneous recovery to be made: or rather we should be inexcusable, knowing as we do that the complaint is the more obstinate the longer it has lasted, and that it tends to the establishment of organic visceral disease, we should be without excuse if we did not stop it as quickly as we can. The disease is always distressing to the patient, and always debilitating. It may be dangerous, even in these climates, to weak or old persons: and it is dangerous to all persons in hot climates. "If the first fit (says the wise and observant Heberden) has been marked so clearly as to leave no doubt of its being a genuine intermittent, the remedy should be immediately given in such a manner as to prevent, if possible, a second." There needs very little preparation of the patient before administering the specific substance which is to cure him; and which every one here knows, before he hears me say so, to be the celebrated Peruvian bark, or its active principle as presented by the salts of quina. The old practice was to wait a few returns of the fits, either till some hypothetical ferment had taken place, or until supposed morbid matter had been expelled by vomiting or purging. There is, however, one very simple and short preparative which I am in the habit of using, and which I learned at Cambridge. You are aware that Cambridge is situated on the very edge of the fenny country which extends along that part of the east coast of the island. Numerous patients afflicted with ague come in from the surrounding villages; and Dr. Haviland found that many of these had taken quina before they applied for assistance as out-patients at the hospital: but with very poor success. Now these cases readily gave way—the patient remaining in all other circumstances as before—after the operation of a calomel purge. I have adopted this practice, therefore, upon his recommendation; but it does not delay the specific treatment. I generally prescribe three grains of calomel with six or eight grains of rhubarb at bed-time, and commence with the quina the next morning. Very lately, in perusing the late Dr. Baillie's posthumous volume, I met with the following passage:—"I have known a good many cases in which bark alone would not cure an ague. In all these cases, as far as I now recollect, when a grain of calomel was given every night for eight or ten nights, bark cured the ague in the course of a few days. This practice I learnt from my friend Dr. David Pitcairn."

I believe that calomel given once in a purgative dose is enough.

But first of all, what is to be done for the patient while he is *in* the fit? I confess to you that I seldom give myself much concern on that head. In ague, as we see it in this country, nature generally prompts the patient what to do: to cover himself up in bed, and apply warmth to his feet, and to take some hot drink, during the rigors; to adopt a cooler regimen during the hot stage; to wipe his skin dry, if the

sweating should be very profuse or protracted. But in hot countries, and in severer forms of intermittent, the patient really requires some help; and therefore I must consider shortly in the next lecture the management of the paroxysm; and I am the more bound to do so, because certain measures which I do not think necessary or judicious, at any rate for the complaint as we see it here, have lately been strongly recommended during the ague fit.

LECTURE XLII.

Treatment of Intermittent Fever; during the paroxysm; during the intermissions. Prophylaxis.

I WAS about, when we last separated, to consider the treatment of ague: first, during the paroxysm; secondly, during the intermissions.

In this climate we need not, I say, encumber a patient in an ague-fit with too much help. But in hot countries, where the disorder is apt to run into the remittent, or even the continued form, and where, during its violent and rapid course, internal organs are liable to sustain serious damage, the best, and indeed almost the only time for the effectual interference of the physician is in the first assault or paroxysm of the disease.

The objects of treatment during the paroxysm are, to alleviate the uneasy sensations of the patient; to abridge, if possible, their duration by shortening the fit; and to avert the danger which, under certain circumstances, may arise from intense internal congestion long continued, or from the severity of particular symptoms.

Now in the cold stage of ague, *diluent drinks* have been recommended, and *cordials*, and *external warmth*, and *opium*, and *emetics*, and *blood-letting*. One would suppose that if some of these expedients were useful, others could scarcely be so too. The *diluent drinks* are very proper: and I should allow the patient to use his own discretion in the choice of them. It was customary, formerly, to prescribe medicated drinks of this kind; and one pleasant, but neglected ptisan still lingers in our Pharmacopœia, the *decoctum hordei compositum*. Now-a-days we are contented with the simple barley-water, toast and water, weak tea, gruel, and the like. These diluents should be taken warm, and for persons who are very feeble or exhausted, they may be made gently *cordial*: weak negus, for example, or white wine whey, may be given.

External warmth, being what nature and common sense would suggest, is certainly advisable and beneficial in the cold fit; even the warm bath, if it can be procured. In some places it is the custom to await an expected fit in the warmth bath. When this cannot so conveniently be obtained, the pediluvium may be employed; or the patient may be put into a warm bed, and have bags of hot salt or brain applied to his epigastrium; and a hot bottle, or a hot brick, wrapped up in flannel, to his feet. Or, what perhaps, is best of all, he may have a hot-air bath *applied to him*, as he lies in bed. This may be very easily done by means of a semicylinder or cradle of wicker work, closed at one extremity by a board. This is laid over the patient, and then covered with blankets. Through a hole in the centre of the board, one end of a curved iron tube is passed; the other end, expanded into a bell, looks downwards; and a spirit lamp being placed beneath it, the air between the wicker work and the sick person is soon made very hot. This apparatus was constructed many years ago, by Dr. Gower, when he was physician to the Middlesex Hospital; where its utility has been fully proved. External warmth applied in some one of these ways, affords singular comfort oftentimes, and contributes to shorten the cold stage. And the same may be said of friction, with stimulating liniments, along the course of the spine. Lind found that, in children, rubbing the spine with an embrocation composed of equal parts of soap liniment, and laudanum, at the approach of the cold stage, often prevented the paroxysm.

Opium has often been exhibited in the cold stage, with the view of cutting short the fit; and not without some success. The strongest evidence of its usefulness in that stage of the paroxysm is furnished by Dr. Trotter, in his *Medicina Nautica*. Agues being very frequent among the crew of the *Vengeance*, he resolved to try the full effect of opium in preventing the fit. At its first approach a dose of laudanum (never less than thirty drops) was given: if this did not bring on some warmth within ten or fifteen minutes, from twelve to twenty drops more were administered. In most cases, "in a few minutes an exhilaration of spirits was perceived: the pulse from being weak, quick, and sometimes irregular, became full, less frequent, and equal; an agreeable warmth was diffused over the whole frame, and every unpleasant feeling vanished, sometimes in a quarter of an hour." The patients were themselves surprised at the sudden change in their sensations." Dr. Trotter speaks of these as being the completest cures that ever came under his observation. If, at the next period, the paroxysm threatened to recur, the opiate was repeated always with the same success. "Few instances were met with where any indisposition indicated a third attack, at the expected period of accession." Notwithstanding this testimony, it appears that opium is still better adapted to another stage of the paroxysm.

Emetics were formerly much prescribed in the cold stage, at its earliest approach. Cullen recommends them; and they may sometimes be useful, in spite of Chomel's assertion that they are always hurtful. That they have gone so much out of fashion is, however, a proof that they cannot be depended upon for cutting short the paroxysm. Vomiting is itself no small distress to many persons; and for my own part, I should not think of giving an emetic unless some indications of a loaded and oppressed state of the stomach were present; such as nausea, an ill taste in the mouth, a coated tongue, and foul breath. A scruple of ipecacuanha will, even then, be sufficient. The object is to empty the stomach effectually but mildly. I would not give antimony. Irritability of the stomach, in the more violent of these fevers, is too apt to arise spontaneously. Sir Gilbert Blane tells us that the greatest impediment to the cure of the severer intermittents at Walcheren, in their early stages, proceeded from the extreme irritability of stomach, which made it difficult to administer the requisite medicines. In hotter climates nausea and vomiting are still more common and more urgent; and we have to guard against the risk of inducing or aggravating these symptoms. "Emetics (says Dr. Mackintosh, in his *Practice of Physic*) have been often extolled, but I believe every experienced tropical physician will agree with me in cautioning young practitioners against their indiscriminate employment. Irritability of the stomach is one of the most frequent and troublesome symptoms; and once excited, it is always difficult, and in many cases impossible, to restrain it. I have seen emetics exhibited, and the vomiting has continued till death, in spite of every remedy."

[We have repeatedly prevented the accession of the chill by administering to the patient, just before the period when it was expected, an emetic of ipecacuanha, and after this had ceased operating, immersing his feet in hot water, and, as soon as he is placed in bed, giving him from 30 to 40 drops of laudanum, or a teaspoonful of the camphorated tincture of opium. By this treatment not only is the chill prevented or shortened, but the whole paroxysm is often rendered milder. — C.]

Lately, the practice of *blood-letting* in the cold stage has been revived (for it is not a new practice), and strongly recommended, by the physician whose name I have just mentioned; and whose opinion carries with it the more weight from its having been founded on much personal experience in the treatment of these fevers. Dr. Mackintosh affirms that bleeding, performed in the cold stage, will often stop at once the paroxysm, and with it the disease: that even when its curative effects are less decisive, it will generally stop the cold stage, and shorten the paroxysm, and mitigate its severity, and afford speedy and great ease to the distressful sensations of the patient; and that any subsequent paroxysms which may occur will be mild and few. One bleeding, he says, is commonly sufficient: sometimes two are required; seldom more than two. The blood is to be suffered to flow till the patient feels relief: which usually consists in liberation from pain of the head and loins; freedom of respiration; the departure of the painful sensation of cold; and the cessation of the tremors and

of the debility. Most of the patients fall asleep after the operation. These effects have been produced by the abstraction of an ounce and a half of blood; they have sometimes (but rarely) required for their production twenty ounces.

Now this is the piece of practice to which I adverted at the close of yesterday's lecture, as being, in my humble opinion, inexpedient, and not to be recommended; at least in the agues of this country. I have seen a good many cases, first and last, and certainly I have never seen one in which I could have thought such a heroic remedy necessary, in the cold stage; if indeed it be, in that stage, a remedy at all. But I do not desire to oppose my experience alone, or my judgment, to that of Dr. Mackintosh. His method has been tried, since he first made it public, by various practitioners in this country. Drs. Townsend and Law, of Dublin, found it fail in the majority of cases. In Dr. Stokes's hands, the most usual effect of blood-letting in the cold stage was, to check the shivering; and, next to this, to mitigate its severity, without abridging its duration. In most instances, no modification was produced of the hot and of the sweating stages. In Dr. Kelly's experience, the general effect was, to shorten the cold stage, and to render the hot one milder; but in some cases it seemed to aggravate the symptoms. Mr. Gill found that, although the blood-letting might cut short the cold stage, it appeared to lengthen the period of febrile disturbance.

Confining myself, then, to intermittents as they show themselves in this climate, I cannot advise you to adopt the practice introduced by Dr. Mackintosh — of bleeding in the cold stage. I object to it because it appears to me quite unnecessary; because it is not such as the nature of the symptoms would suggest; because it tends to produce subsequent debility, which we should not needlessly inflict; and because the experience of other sober-minded men, who have given the method a fair trial, does not bear out the statements made by Dr. Mackintosh in respect to its usefulness.

At the same time, after a careful perusal of nearly a hundred cases adduced by Dr. Mackintosh to illustrate the efficacy of this measure, I think it highly probable that blood-letting may constitute the most important part of the treatment, in the very outset of the severer malarious fevers of hot climates; attended as they are with a degree of internal congestion and disturbance which is dangerous to the integrity of vital organs.

[Mr. Twining, in his work on the Diseases of Bengal, bears strong testimony in favour of bleeding in the cold stage of intermittents. In the greater number of cases he has found it to arrest the paroxysm; that is, the occurrence of the hot and sweating stages is prevented. In the majority of patients, when the bleeding has been preceded by a course of mild purgatives, there will be no return of the disease, provided attention is paid to keep the body properly clad and to guard against exposure to atmospherical vicissitudes. In this manner, he remarks, we cut short the fever, and guard against those ulterior visceral engorgements and indurations, by which it is too often prolonged, until the constitution is completely ruined.]

The only period of the cold stage at which bleeding is proper, Mr. Twining states to be, at the very commencement of the rigor, or just when the coldness and shivering are completely established. He has found that, in general, it is sufficient to take from an adult twelve or fifteen ounces of blood, and in the most robust European he would limit the quantity to be taken at one bleeding to twenty ounces. After the arm is tied up the patient should be permitted to lie quiet in bed for an hour or two — but not heated by being covered with too many bed-clothes; he should be immediately supplied with a cup of warm tea, gruel or thin sago.

According to Mr. Twining, "the requisites to ensure success from bleeding in the rigor, are, 1st, the preliminary course of moderate purging; 2d, that the blood be taken, from a large orifice, quite as soon as the coldness and rigor are fairly established; and 3d, that the patient be bled in a recumbent posture, and no more blood be taken than is sufficient to arrest the paroxysm."

In robust plethoric patients, who, during the intervals of the paroxysm, complain of headache, and morbid tenderness on pressure over the abdomen, and pain or uneasiness in the chest, the disease will seldom be arrested by the first bleeding; in many cases, they will have repeated paroxysms, in each of which the use of the lancet will be required. Patients, also, in whom the paroxysm, more especially the cold stage,

is attended with vomiting, Mr. Twining has found to require the repeated abstractions of small quantities of blood during the rigors. — C.]

If in this country, bleeding be requisite at all, it is in the *hot* stage. But it is not requisite at all, except when there appears to be danger of some internal inflammation. The best remedy of the hot stage is undoubtedly opium. Dr. Lind, who wrote after large experience, says that he never saw a person die in the cold fit, but had known several carried off in the hot one, with strong convulsions and delirium. He happened to notice the beneficial effect of an opiate given while the patient was very hot and feverish. He determined therefore to make further trial of opium in the paroxysm. "Having at that time (says he) twenty-five patients labouring under intermittent fevers, I prescribed an opiate for each of them, to be taken *immediately after* the hot fit, provided the patient had then any inquietude, headache, or any such symptom usually subsequent to the fever. The consequence was, that nineteen in twenty-two received immediately relief; the other three had no occasion to take it."

"Encouraged by this surprising success, I next day ordered the opium to be given *during* the hot fit. In eleven patients out of twelve to whom it was thus administered, it removed the headache, abated the fever, and produced a profuse sweat; which was soon followed by a perfect intermission. Since that time I have prescribed an opiate to upwards of three hundred patients labouring under this disease: and I observed, that if taken during the intermission, it had not the least effect, either in preventing or mitigating the succeeding fit; when given in the cold fit, it once or twice seemed to remove it; but when given half an hour after the commencement of the hot fit, it generally gave immediate relief."

Dr. Lind goes on to state that he found the influence of opium more uniform and constant in intermittent fever than in any other disease; and more quick and sensible than that of any other medicine.

Very little need be said in regard to the sweating stage. Up to a certain point the perspiration is to be promoted and encouraged. When the uneasy feelings of the patient have abated, it should be restrained; not suddenly, but with caution. Now the sweating may be promoted by diluents; by keeping the patient in bed, and covered with moderately warm clothes; by sippings of hot gruel, or of hot chicken broth. On the other hand, when the sweating has continued long enough, it may be stopped by drying the patient carefully with towels, changing his linen, and getting him up, out of bed.

It is well to bear all this in mind; but I repeat once more that in agues, such as you are likely to meet with in this country, it is unnecessary, and therefore objectionable, to be *over-busy* during the paroxysm. Whenever the disorder assumes a distinctly intermitting form, the most important part of the practice is that to be employed during the intermissions. Now there are certain general remedies advised for adoption in this period; and there are certain specific remedies. The general remedies are bleeding, emetics, and purgatives. They need not detain us a moment. Blood-letting may be used if there be any apparent tendency to local inflammation, or any marks of severe topical congestion; especially in young and robust subjects. Barring such circumstances, there can be no occasion to bleed your patient in the intermissions.

An emetic given a short time before the expected paroxysm, has been known to prevent its accession; and even has sometimes cured the disease. But we can stop the paroxysms by gentler and better means; so that I should not prescribe an emetic unless I saw symptoms of a foul and loaded stomach.

Purgatives should always be given at the outset. They clear the stomach and intestines of hurtful accumulations, which are apt to impede the beneficial operation of the quina, or of other drugs given to check the disorder. I mentioned in the last lecture my own custom in this matter; viz., to give a couple or three grains of calomel with eight or ten of rhubarb at bed-time; and to commence with the specific remedies the next day.

Of these specific remedies, *bark* and *arsenic* are by far the most certain and important; but a multitude of others have been highly praised for possessing similar virtues. I shall by and by say a word or two about *some* of these, because bark is dear, and arsenic is scarcely a safe drug to be entrusted to the hands of unprofes-

sional persons; and yet it is often expedient, in country places, where agues are rife, to provide the poor with remedies which they may have at hand; and which should both be reasonably cheap, and perfectly safe.

I shall not detain you with any account of the difficulties and objections which were thrown in the way of the *Peruvian bark*, upon its introduction into the *materia medica* about the middle of the seventeenth century. Its use met with the most violent opposition, even from physicians of the highest authority. It was resisted by Stahl and Hoffman; and Boerhaave was never quite reconciled to it. Sydenham, by his example and recommendation, greatly promoted its adoption in this country. All this history is sufficiently curious and interesting, but I have no time for it: and you will doubtless hear it from one of my colleagues. I will merely say that in the *Peruvian bark* we have one of the very few *specifics* that we can boast of possessing: and that, unlike most other highly vaunted substances, so far from falling off from the accounts first given of its virtues, it has acquired in the lapse of time an increase and stability of reputation.

Neither shall I enter at all into the consideration of the qualities of the several species of cinchona; nor of the several principles that may be educed from them; nor of the modes in which the quina even may be best procured. This would not belong legitimately to my province. I must suppose that the professors of chemistry and of *materia medica* have furnished you with the sulphate of quina, which is the only preparation of the bark I intend particularly to notice: and *my* business is to tell you what I know in respect to its employment as a *remedy for ague*.

I may observe, however, that this is a remedy to which we could never have been led by any process of reasoning. It is a matter of pure empiricism. We know nothing of the seat or of the essential nature of the disease; we are equally in the dark as to the *modus operandi* of the quina in curing it; yet our knowledge of ague, upon the whole, estimated in reference to its precision and practical bearing, is more satisfactory than of many other complaints, with the seat and nature of which we are much better acquainted. The group of symptoms is so distinct, that we have no trouble or doubt as to the diagnosis; and experience has taught us a remedy which is all but infallible.

The discovery of quina and its salts formed a great era in the history of the *materia medica*. As far as my own experience goes, the sulphate of quina has quite superseded the necessity for exhibiting any other form of cinchona for the cure of ague. Before quina was unshrouded by the chemist, the bark in substance was the only form in which the remedy could be confidently relied upon; and I am old enough to be aware of the infinite superiority of the salt, over the actual bark. To obtain the desired effect, it was often necessary to give it in such quantities as almost justified Mr. Abernethy's sarcastic way of speaking of it and of physicians. He said the doctors talked of throwing in the bark, as if it were to be pitched into the stomach with a shovel. The sulphate of quina lies in a much smaller compass, and a more commodious form; and it does not cause that insupportable nausea which the woody mass of the powdered bark was so apt to occasion.

I am in the habit of giving two, and sometimes three, grains of the sulphate of quina every four or six hours during the intermissions, to those patients whom I have occasion to treat for ague. This plan has succeeded so well, that I have never been tempted to try any other. I may, indeed, say that I have never known it fail to stop an ague; and to stop it speedily: so that very few paroxysms have occurred after the patient has begun to take the medicine. You may give it in the infusion of roses, which contains a convenient quantity of sulphuric acid to ensure the solution of the sulphate of quina. It changes the colour of the infusion, however, and renders it pinker and opaque. Whether the draught be more or less elegant on that account, I will not take upon me to say; I know that the virtue of the quina is not much interfered with by the change. In private practice, I commonly prescribe twice as many drops of dilute sulphuric acid as there are grains of the quina, with a drachm of the tincture of orange peel, and a drachm of the syrup of the same; completing the draught with water. This I find my patients commonly approve of, except in its bitterness, which, in a solution, nothing can disguise. Or that salt *may* be administered in the shape of a pill: it is best, however, and surest in solution.

A question has been raised, whether this remedy should be given in repeated doses

during the intermissions, or whether one very large dose should be given a short time before the paroxysm is expected. Dr. Home made some experiments on that point in the clinical wards of the Edinburgh Infirmary, some time ago; and he thought that the result was in favour of the plan of giving the bark regularly at short intervals. I have told you the amount of my own experience, which, however, is not very great; nor have I had any severe cases to deal with. I think it not improbable that my patients would have been cured quite as soon if I had given the remedy in half the strength. Dr. Barker, of Dublin, has found small doses equally effectual with large ones; and this is very likely to be the case with *specific* remedies. It would appear, however, that in some quartans it is better to give large doses before the return of the paroxysm. Dr. Elliotson gives large doses just *after* the paroxysm; and then smaller doses during the remainder of the intermission, at regular periods. A great majority of those who suffer ague are poor persons. Of course the first object is to make the cure as *speedy* as possible; the next to make it as *cheap* as possible. So that it is not a matter of indifference, or mere speculative curiosity, to ascertain with how little quina you may cure an ague. I repeat that it has not happened to me to be disappointed, when I have given the medicine in small doses, as already described: which amount to about twelve grains in twenty-four hours; but, then, I suppose my cases have been well behaved and submissive. Dr. Elliotson states that he is continually obliged to give twenty or thirty grains in the twenty-four hours, before he can cure the complaint: sometimes in obstinate quartans, forty-five grains; and he mentions one case in which a scruple of the sulphate of quina, with ten minims of the liquor arsenicalis was given every eight hours in vain, but succeeded perfectly when given every six hours.

It appears, also, upon the testimony of careful observers, that in warm climates larger doses *are* required; and that it takes a *larger quantity*, upon the whole, to repel the complaint. In the aguish tracts of Italy, in the Maremma, small doses are said to be inadequate; and the physicians there are in the habit of giving twelve, twenty-four, or even thirty grains at a time: and in one recorded instance, the dose, in seven days, was got up to 108 grains, before the ague was arrested. The medium dose, in many parts of America, seems to be eight grains.

Respecting any drug, it is desirable to possess some easy test that the amount of it administered has reached the limits of sufficiency and safety;—the limits beyond which it is needless, and might be hazardous, to carry it. Now it has been ascertained that when the quina is given in repeated and gradually increasing doses, it comes at length to affect most persons with peculiar sensations (generally spoken of as sensations of fulness) about the head, and with a sort of buzzing noise in the ears. Sometimes, when thus administered, it reduces, in a remarkable degree, the force and the frequency of the pulse. Very different quantities are requisite to produce these symptoms in different individuals; but whenever the buzzing is experienced, you may conclude that the system is conscious of the full force of the remedy, and that to push it further would be inexpedient.

In this country it has been the custom to exhibit the bark, or its equivalent substitute, in the intermissions only, and to suspend it during the fits. But our American brethren have taught us that this forbearance is unnecessary; that the quina may be given during the paroxysm with perfect safety, and with much advantage; and that, in the severer remittent fevers, the real hazard lies in abstaining from its use, until a comparatively apyretic period may arrive.

The irritability of the stomach is sometimes so great as to make it difficult to introduce a sufficient quantity of the remedy into the system. This difficulty was very much felt at Walcheren: it is in a great measure removed since the discovery of quina. But even the quina sometimes sits ill on the stomach; and often it is scarcely possible to get children to swallow any preparation of bark, on account of its bitter taste. It is an important thing to know, therefore, that this drug has been found scarcely less effectual in curing the disease, when thrown into the rectum. The menstruum in which it is dissolved should not exceed two or three ounces, lest the bowel should reject it. Its expulsion may sometimes be prevented by adding a few drops of laudanum to the enema.

[The quina may also be administered endermically; from 4 to 10 grains being

sprinkled upon a blistered surface, once, twice or oftener, according to circumstances, in the course of the day. That the remedy will produce its specific effects when thus exhibited we know from experience. — C.]

It is said that bark in substance will sometimes cure the disease when quina fails. I have never witnessed this: but in obstinate cases I would give the quina in the *decoction* of bark.

[It is very certain that the quinia, even when given freely and in large doses, will occasionally fail in arresting the disease, and that the majority of such cases will be promptly cured by the bark in substance. This we have seen repeatedly to occur. — C.]

You must not be satisfied with merely stopping the paroxysms. Patients are often too ready to give up their medicine, as soon as the paroxysm has once missed. But the disease is very apt to recur; and it will always be right and prudent to go on with the quina for ten days or a fortnight after the patient *seems* cured, gradually diminishing, after the first week, the amount and the frequency of the doses.

There have been some curious facts observed in regard to the relapse that are apt to take place after the bark or the quina has been omitted. Clark of Dominica, states that if no more of the remedy be taken, in the West Indian ague, than is barely sufficient to stop a fit, and then the bark be suspended, a relapse may take place on the eighth day, in the case of a quotidian; on the fourteenth or fifteenth in the case of a tertian or double tertian; and on the twenty-first or twenty-second in the case of a quartan: thus making (you see) in each type, seven periodical revolutions from the time the fit was suppressed, to the next attack; and the fit was found to return on the proper day, at the same hour at which it would have returned if its course had not been interrupted by the administration of the remedy. Here we have a still earlier glimpse of the abiding periodic tendency, noticed by Dr. Gregory and by Dr. Graves, during the long protracted absence of actual paroxysms. All this is very curious, and inexplicable; but it points clearly to the propriety of continuing the remedy for some time after the disease appears to have vanished.

[The very high price to which quinine has attained of late years, rendering its free use, in cases of fever occurring among those in moderate circumstances, a matter of serious inconvenience, extensive experiments have been made with other of the proximate principles obtained from different varieties of the cinchona, with the view of discovering among them some substitute of less cost and equal efficacy. Of these, the cinchonine, quinoidine, and quinidine present the strongest claims to our notice. The sulphate of cinchonina has been found, from ample experience, to be little inferior, even in the same dose, to the sulphate of quinia as an antiperiodic. Both the quinoidine and quinidine are possessed of very decided efficacy, but require to be given in much larger doses than the quinine. — C.]

Arsenic is another substance which has unquestionable and great power over ague. It carries with it these marked advantages: it is efficacious; it is cheap; and it is tasteless. It is well adapted by these qualities for the poor, and for children, and for patients of every age and rank in whom there is much irritability of stomach present; but then it has also the serious disadvantage of being an active poison. One over-dose may be fatal: and even its long-continued use in minute doses leads sometimes to evident and lasting disorder of the health. Arsenic, therefore, is an unsafe remedy to be trusted in the hands of the ignorant. It should never be administered except under the immediate supervision of a medical eye; and even then it requires to be given with much caution. Its bad effects may be very certainly prevented, however, by care and attention; and it becomes a valuable instrument of cure, and should be adopted without scruple, in cases where its operation can be watched, and where the quina does not agree with the stomach, or fails to stop the disease. I often prescribe arsenic for other complaints; but, as I said before, I do not recollect ever having been foiled in removing ague by the sulphate of quina. Some persons are of opinion that relapses are less frequent after the cure by arsenic than after the cure by bark. It would require a large induction of particular facts to make that point clearly out.

When substances, which even in small quantities prove active poisons, are used as remedial agents, it is convenient to have some definite form in which they may be administered at all times, and in all places. The *liquor potassæ arsenitis* of the London Pharmacopœia supplies such a form. This is the form in which arsenic was recommended to the public by Dr. Fowler: and it is therefore sometimes called *Fowler's solution*. It was founded upon an analysis of the *tasteless ague drop*, which had been in considerable repute in some parts of England. The pharmacopœial preparation is an arsenite of potass in solution. There are eighty grains of arsenic in the new or imperial pint, and therefore four grains in an ounce of the solution. Ten minims two or three times a day are a full dose for an adult; and you had better commence with not more than five minims. Ten minims contain one-twelfth of a grain. Twice that quantity has been administered at once; but this ought never to be done except when the system has been gradually inured to the arsenic, and thereby enabled to bear such a dose. It is a good precaution not to give this corrosive substance on an empty stomach.

The poisonous or hurtful effects that we have to look out for, when arsenic has been prescribed, are a peculiar silvery whiteness of the tongue, loss of appetite, nausea, and sometimes vomiting; griping pain of the stomach and bowels, and diarrhoea; and if the medicine be continued, *fainting* is often added. Other symptoms, less constant perhaps, and less important, are painful and hot tumefaction, suffness, and itching of the face and eyelids, with redness of the conjunctiva, or even a tingling eruption something like nettle rash. These effects may, I believe, be controlled by adding a few drops of laudanum to each dose; but I would rather advise you to suspend the use of the arsenic; or to leave it off altogether. When this is done, the unpleasant symptoms will readily yield to mild laxatives, followed by opiates.

[The arsenical solution is a remedy admirably adapted for the cure of intermittent fever when it occurs in children, to induce whom to take the quina in any form will be found often impossible. When cautiously administered, and its effects are carefully watched, we have never known any disagreeable effects to result from the arsenic, and we have employed it somewhat extensively. — C.]

When the paroxysms continue to recur in spite of the bark, it has been recommended (and I think the plan a good one) to try to stop them by arsenic; and then, the periodic recurrence having been broken, to employ sulphate of quina to prevent a relapse.

These, then, quina and arsenic, are the two sheet anchors to which we trust, in the cure of ague. A host of other remedies, I say, have had their praises sung. I do not intend to enumerate them. But there are a few which I think it right to mention, for reasons already assigned. There is strong evidence of the efficacy of some of them; they are cheap, and easily accessible, and above all, safe; and therefore, in aguish districts, they may with much propriety and benefit be recommended to the poorer classes, or distributed by Lady Bountifuls.

[The ferrocyanuret of iron is certainly a very valuable remedy in cases of intermittent fever. Stokes places it in efficacy after quina and the arsenical solution, and perhaps he is right in so doing; it will, however, often succeed in preventing the recurrence of the paroxysms, given in the dose of six grains every three hours. — C.]

One of these is *willow-bark*; in substance, or in decoction. If this dose cure agues, as it is affirmed to do, it would seem as if Providence had placed the antidote alongside of the poison; for these trees, as you know, abound and flourish in marshy places. The bark of the willow furnishes an alkaloid substance called *salicine*, in which the febrifuge property is believed to reside. Holly leaves, and *ilicine* derived from them, stand in much the same repute in France, as willow-bark and salicine here.

Another curious remedy, said to be very successful, is the *web of the black spider*, which inhabits barns, stables, and cellars. This substance has been tried on a tolerably large scale, and the testimony to its influence in curing agues is very strong. Dr. Craigie has given this account of it. In the year 1760, a number of prisoners from the vanquished squadron of Thurot having been landed in the Isle of Man, Dr. Gillespie, who was practising there, found that many of the agues which came to prevail

both among the prisoners and the inhabitants of the island, obstinately resisted bark and such other remedies as he had recourse to. He was informed, by an old French physician belonging to the squadron, of the alleged efficacy of cobweb, in certain forms of the disease. He therefore made trial of cobweb, and found it to answer admirably. He was successful with it in more than sixty cases of different types, in the Isle of Man, and he had further experience of its utility subsequently in Ayrshire.

After this, the same remedy was tested in the West Indies, by Dr. Jackson, to whom Dr. Gillespie had recommended it. Dr. Jackson's observations were made in the hospital of the army dépôt, in the West Indies, in 1801. Several cases of ague, on which bark, arsenic, or mercury, singly or alternately, had made either a very temporary impression or none at all, were selected for experiment. In four of these cases, two pills, containing each five grains of cobweb, were given at intervals of two hours, commencing six hours before the expected time of recurrence of the paroxysm. The fit did not return. On subsequent trials it was found not only to arrest the course of agues, but to remove various symptoms, such as pain, delirium, vomiting, griping, in ague, and in continued fever, when these symptoms were unconnected with inflammation.

[We have employed the spider's web in this manner in a number of cases, and in many of them found it very promptly to suspend the paroxysms — as effectually, certainly, as the quina; in a few cases, however, it failed. — C.]

Charcoal is another substance which has been found effectual for the cure of intermittent fevers. You may find an account of it in the tenth volume of the *Edinburgh Medical and Surgical Journal*. It would seem to be especially useful in those cases in which there is a marked disturbance of the digestive organs; nausea, flatulence, hiccup, diarrhoea, or dysentery. It is said generally to cure the complaint by the time two drachms of it have been taken. It may be given in doses of ten or twenty grains, in arrow-root; or with a few grains of rhubarb. If the power of this substance should be confirmed by future observations, a cheap remedy would thus be open to the poor. A clergyman of my acquaintance assures me that he seldom fails to cure agues among his parishioners by administering to them the snuffs of candles, which he takes care to have collected. He does not inform them of what his black powder consists. I presume that its virtue may proceed from the charcoal it contains; unless it is derived from the confidence his flock is accustomed to place in his specific. The very same remedy, the snuff of a candle, is mentioned by Lind.

Piperine, the crystalline salt of pepper, has obtained considerable reputation of late years, as a remedy for intermittent fever. It was largely tried by an Italian physician, Meli; and Dr. Gordini has repeated Meli's experiments at the hospital at Leghorn. The following are the general conclusions at which these physicians have arrived:— 1. Piperine, in doses of six or eight grains, cures intermittents. 2. It is more efficient in powder than in pills. 3. It succeeds in certain cases in which the sulphate of quina fails. And 4. It is more effectual in preventing relapses. I have seen letters from some practitioners in this country, bearing testimony to the power of the piperine. That pepper will cure ague has long been the vulgar belief; and a very popular remedy for the disease is a teaspoonful of pepper in a glass of gin.

I presume that the efficacy of *chamomile* flowers in the removal of intermittent fever is to be attributed to the piperine which they have been ascertained to contain. These flowers had been long in use for the treatment of ague, before the Peruvian bark was discovered; and they are said to have accomplished a cure, since that time, after the bark had failed; but this was before the quina had been educed from it. Heberden advises us to have recourse to chamomile flowers, if the bark should disappoint us. I am always willing to embrace an opportunity of referring to his commentaries, for the exact observations they contain, but above all for the beautiful Latinity of which the whole book is an example. I recommend it strongly to you as being, next to Celsus, the best model you can study for good medical Latin. In reference to the point before us he says, "*Cortex, quanquam rite sumtus, interdum parum efficax est; quo in casu suspicio erit ventriculū sordibus onustum vim remedii impedit. Itaque vomere oportet; quo facto, febris raro non cedit. Quod si redire perseveret, confugiendum est ad flores chamæmeli, quorum contritorum scrupulus*

dandus est loco drachmæ cinchonæ, et ad idem præscriptum repetendus. Hos flores, sic sumtos, semel atque iterum profecisse expertus sum."

Several mineral substitutes for the bark, or for arsenic, have been tried and found useful: preparations of *iron* and of *zinc*. From 5 to 10 grains of the sulphate of zinc have been given several times a day; or 3 grains of the oxide of zinc every three hours. Sir Gilbert Blane says that both in the West Indies and in London, intermittents have been cured by the use of this oxide, when they had previously resisted the Peruvian bark. Sir James M'Grigor also speaks of it in terms of praise; from what he saw of its effects in the agues of the Peninsula during the war.

Some of the remedies of this mysterious disorder operate upon the mind, or rather upon the nervous system, through the mind. Hence it becomes probable that the *drugs* which have such power over the disease, act also on the nervous system, through the body. And hence also we derive a confirmation of the opinion, that the disease itself is essentially a disease of the same nervous system. Ague has often been cured by the agency of strong mental emotion, such as sudden and great joy, anger, terror, or eager expectation. Thus we read that Quintus Fabius Maximus was cured of an old quartan on the day of a great battle. Strong impressions upon the imagination, producing feelings of disgust and horror, have had the same effect: such as those caused by drinking blood; swallowing a spider gently bruised, and wrapped up in a raisin, or spread upon bread and butter; keeping a spider suspended from the patient's neck in a nutshell, till it dies; and the like. The undoubted success, in many cases, of charms, must be referred to the principle of *faith*. The patient recovers, because he firmly believes in your power to cure him. Dr. Gregory used to relate the case of a patient in the clinical wards in Edinburgh, who, with sundry ceremonies, swallowed some word, written on a slip of paper: the result was, that he had not another paroxysm. And I perfectly recollect having a great awe, when I was quite a child, of my maternal grandmother, because she was reputed to have the power of curing agues by means of some charm. I believe all that she did was to assure the poor people who came to be relieved from their ague, that they should have no more of it *after such a day*; and their implicit reliance upon this prophecy brought about its fulfilment. There seems to be this general principle observable in respect to agues, and to most other diseases which occur in paroxysms, viz., that after they have continued for some time, their further continuance depends more upon the effect of *habit* than anything else: and this habit may be broken by strong impressions made upon the nervous system; and the cure of one paroxysm is often thus the cure of the disease. We have seen examples of the existence of this morbid habit in hysteria, and in some cases of epilepsy. *Cæteris paribus*, that physician will be most successful in these disorders, who is best able to acquire the confidence of his patient, and to gain a powerful influence over his mind.

There is no disease in which the *prophylaxis* is of more importance; but this you will have gathered from the facts which were stated in the two preceding lectures. The disposition to relapse is strongest soon after the disease has been removed; but it generally continues long, perhaps even for life. The late Dr. Macmichael caught an ague many years before his death, by sleeping on a rock somewhere in Greece; and he was ever after subject to occasional attacks of periodic headache, and other aguish symptoms, for which he was obliged to have recourse to bark or arsenic. Of course one essential point in the prophylaxis is the withdrawal of the patient from the influence of the exciting cause; taking him away from the malarious locality. But this cannot always be done; and when it cannot, we must impress upon him those cautions which arise out of the facts ascertained respecting the operation of the malaria upon the human body. Persons who have been exposed to the exciting cause, or who have once had the fever, should in whatever place they may happen to be, avoid over-fatigue, and exhaustion of all kinds; sudden exposure to cold or heat; and the neglect of changing wet clothes; wet shoes and stockings for instance. In a malarious district persons should bear in mind the facts, that the miasmata are much more virulent in the night-time than in the day; and close to the surface of the earth, than in a higher part of the atmosphere. They should refrain, therefore, from going out late in the evening, or early in the morning; and they should rather select the attic than any other floor for their bed-chamber. They who are obliged to go out in the morning in countries where agues are rife, should take care not to go out fasting: a

good hot breakfast should be first taken, or at any rate some moderate stimulus. A crust of bread and a glass of wine, or a small quantity of ardent spirits, will fortify the system against the pestilential miasma. Measures of this kind have been found extremely beneficial in the navy: the giving, for instance, the men a warm breakfast before going out in the morning on malarious shores in boats, whatever the hour of starting might be. Generous diet, and a fair allowance of fermented liquor, are proper also for all persons in aguish countries. The late Dr. James Gregory used to mention in his lectures an anecdote in point told him by his father. The elder Dr. Gregory studied at Leyden, under Boerhaave; and twenty-four other English students were living there at the same time: that is, they were called English, on account of their common language, but they were in fact composed of English, Irish, Scotch, West Indians, and Americans. The celebrated John Wilkes and Charles Townsend were among the number. These twenty-five students lived a good deal together; in truth they were *cut*, as the phrase is, by the Dutch, for some raffish behaviour on their parts. However, of the twenty-five, one only was a water-drinker. The other twenty-four each drank a bottle of claret daily; and the water-drinker, and he alone, fell ill of ague there.

Persons who have recently become residents in aguish districts, or who even happen to be travelling through them, would do well to take one or two moderate doses of quina daily, by way of safeguard. Strong evidence has been brought forward of late, by Dr. Bryson and others, to show that its prophylactic power is no less marked and sure, and therefore no less valuable, nay even more valuable, than its curative. Under these circumstances of exposure, warm stomachic laxatives are to be preferred to cold aperients, such as the neutral salts, for due regulation of the bowels.

There is just one more expedient which I would suggest as not unlikely to afford complete protection to those who are of necessity exposed to the malaria; and it is, that they should wear an *orinasal respirator*. It is possible that as a breeze is filtered of the poison which was mingled with it, by passing through a dense mass of foliage, so, on a smaller scale, the air inspired in breathing may be strained and purified, and rendered harmless, in its transit through the sieve-like structure of Mr. Jeffreys' ingenious instrument. The principle of the suggestion is not new; but this mode of applying it has not, so far as I know, hitherto been tried. It is said that by surrounding the head with a gauze veil, or conopeum, the action of malaria is prevented; and that thus it is possible even to sleep in the most pernicious parts of Italy without hazard of fever. Dr. Macculloch states that in Malta, and elsewhere, this belief is universal: and hence the popular practice of covering the mouth and nose with a handkerchief in the morning on going out, or in other suspicious circumstances: a practice (he observes) the efficacy of which is attested, as far as popular belief can attest anything. Can it be the moisture which accumulates upon the handkerchief from the breath that confers the protection? Under inevitable exposure to the malaria might not Dr. Stenhouse's *charcoal apparatus*, used during the acts of inspiration only, effectually arrest, and perhaps destroy, the entering poison?

[BILIOUS REMITTENT FEVER. — With the exception of the intermittent, the most common form of fever prevalent in the middle, southern, and south-western sections of the United States, is the remittent, or, as it is generally denominated, from the evidences of more or less derangement of the hepatic function by which it is attended, the bilious remittent, or, simply, bilious fever. It constitutes the summer and autumnal endemic of many of our states, and even in those which are exempted from its annual occurrence, it occasionally prevails, during seasons of unusual heat and dryness, sporadically, or as a severe and widely-spread epidemic. It is this form of disease to the attack of which individuals, from the cold and temperate sections of our Union, are so peculiarly liable on removing to those localities where it is endemic, or on visiting them during the summer and autumn. The bilious remittent fever is not peculiar to the United States; it prevails extensively in the southern portions of France and Italy, and other parts of the south of Europe, in Africa, and in the East and West Indies: it is in fact the endemic of the paludial districts of all hot climates. A high degree of atmospherical temperature has a very marked influence in the production of this form of fever. It may act either by eliminating an aëriform morbid matter, or by predisposing the system to the action of those morbid causes by which

it is constantly surrounded. The prevalence, as well as the violence of the disease, is almost invariably in direct proportion to the heat of the season, while, in many of the more northern states, where the disease is not one of ordinary occurrence, it will nevertheless prevail, during a summer of extreme heat, often to a considerable extent—and marked, occasionally, by symptoms of extreme violence; being limited, however almost exclusively to those situations, noted in other seasons for their insalubrity, or in which the causes of disease are ready to be developed upon the occurrence of any unusual degree of atmospherical heat.

Notwithstanding the bilious remittent fever can scarcely ever be mistaken by the least experienced practitioner, it still exhibits considerable diversity in many of its phenomena in different seasons and localities, and often in different individuals. While, in many cases, the remissions are well-marked, approaching in some to almost a complete intermission, in others the remissions are so slight that the fever has nearly a continued form. There are always, however, certain prominent symptoms, invariably present, an attention to which will lead at once to an accurate diagnosis:—These are, gastric irritability, a sense of oppression and distress at the epigastrium, pain of the head, back, and limbs, and the great prostration of strength early exhibited by the patient.

Bilious remittent fever is closely allied in its nature, the localities in which it chiefly prevails, and in many of its phenomena, to intermittent fever, of which by many it is considered as a mere modification. It is presumed, and with much plausibility, that “a more intense operation of the same morbid cause required for the production of intermittent fever engenders remittent,” and that “the more violent the latter, the more remote is its character from that of intermittent; or, in other words, the less perceptible the remissions.” That a more powerful action of the morbid cause is demanded for the production of remittent fever, is supposed to be indicated “by the circumstance, that, when periodic fevers are prevailing in certain countries, the permanent residents are often observed to have the disease in the form of ague only, and the mortality among them is small; while strangers, unhabituated to the climate and its diseases, suffer from remittents, with a proportionably greater loss of life. In more sickly seasons remittents will be the prevailing form among both classes of persons, but strangers are more violently affected, and the mortality among them is greater. Its affinity to intermittent is shown, too, by the tendency which it has to pass into that form, and, inversely, by the proclivity of ague to assume the remittent type.” (BROWN; *Cyclopædia of Pract. Med.*)

There is a very striking analogy between the milder and more ordinary form of the bilious remittent fever, in its course and progress, and the periods of a double tertian ague. Although the exacerbations occur daily, yet there is almost always a very manifest aggravation of all the symptoms on the odd or alternate days. When the disease assumes, as it does occasionally, a quotidian type, the exacerbations generally occur several hours earlier than those of the double tertian form—the former happening usually about nine or ten o’clock, and the latter not until towards noon, or an hour or two later.

The milder and more simple form of bilious remittent fever is generally preceded, for some days, by listlessness, languor, a bitter taste in the mouth, nausea, aversion from food, an indescribable uneasiness and sense of fulness about the epigastrium, sometimes costiveness, and, very generally, more or less pain and heaviness over the eyes. The attack is usually ushered in by a slight chill, or merely by a sense of coldness, particularly about the back, which, after one or more hours, and often sooner, is followed by increased heat of the whole surface—the skin becoming, at the same time, dry and constricted, the face flushed and turgid, the eyes red and suffused, the respiration hurried and uneven, the pulse quick and frequent, but rarely tense; there is great prostration of strength, with considerable restlessness and watchfulness. The patient complains of pain and a sense of fulness, weight, and tension of the head, pain of the back and of the extremities, particularly of the calves of the legs;—and of a sense of weight or oppression, and often of pain at the epigastrium, which part generally exhibits more or less tenderness upon pressure, even when no pain is complained of. The stomach is more or less irritable—some cases being attended with distressing nausea, while in others, everything swallowed is instantly rejected. Spontaneous vomiting is a not unfrequent symptom—it is often present from the com-

mencement of the attack, but more generally it does not set in until the second or third day, or even later; the matter vomited being of a bitter taste, and of a yellow, greenish, or bright-green colour. The tongue is usually moist, red at the sides and edges, and coated on its upper surface with a whitish, light-brown, or yellowish fur, which often acquires considerable thickness. There are usually considerable thirst, a costive state of the bowels, and a diminished amount and increased coloration of the urine; and, after the disease has continued for some days, the skin acquires a yellow tinge, which is sometimes very decided, and extends to the adnata of the eyes. The exacerbation lasts from eight to twelve hours, when a gradual abatement of all the prominent symptoms takes place, and often a slight moisture breaks out upon the surface, and the patient falls into a refreshing sleep; more generally, however, the skin continues dry after the heat has declined, and the patient during the remission continues restless, uneasy, and disinclined to sleep. The duration of the remission varies with the violence of the attack; gradually, however, the heat of the surface increases, and an exacerbation of the fever follows, marked by the same degree of intensity as the former one, or even by increased violence.

All the preceding symptoms, in an aggravated degree, with long-continued exacerbations and less distinct remissions, mark what has been termed the highly inflammatory form of the disease. During the exacerbation the skin is intensely hot, the eyes are suffused, of a muddy, yellowish hue, and often dull and languid; there are intense pain, and a sense of insupportable weight and tension of the head; aversion from light and sound is ordinarily present, and occasionally delirium; there are great thirst, and a feeling, often almost insupportable, of oppression at the chest; the respiration being quick and laborious, frequently irregular; the pains in the back and extremities are often of great severity; the pulse is quick, frequent, and more or less tense; occasionally it exhibits some degree of irregularity; the nausea and vomiting are generally peculiarly distressing — the matter discharged being a thick ropy fluid, of a yellow, darkish-brown, or green colour; the bowels are costive, or if open, discharge, with tenesmus and griping, a thin watery fluid: when evacuations are procured by appropriate means, they are large in quantity, dark, slimy, tenacious, and offensive. There is always an intolerable sense of oppression or constriction at the epigastrium, accompanied by a degree of tenderness which renders the slightest amount of pressure insupportable — or a severe pain and burning, attended in many cases with great nausea, and frequent ineffectual efforts to vomit, are experienced. There is great restlessness, with jactitation, and continued watchfulness. In the course of the disease, the skin acquires, most generally, a brownish, bronzed, or more frequently, a deep yellow tinge, which is particularly marked upon the face and breast. A symptom mentioned by Rush, as generally present in cases of bilious remittent — namely, a screatus, or constant hawking and spitting of a small quantity of tough, glairy matter — we have repeatedly noticed.

The succeeding exacerbations are marked by an increased violence of all the predominant symptoms of the disease, and unless the intensity of the fever is relieved by an appropriate course of treatment, the powers of life gradually sink; the surface, at length, becomes cool, and covered, generally or partially, with a cold clammy sweat; the pulse small and weak; the tongue covered with a dark, thick coating, and occasionally dry and chapped; the respiration short, quick, and difficult; the abdomen sometimes swollen and tympanitic; stupor or coma often ensues; not unfrequently, repeated, and at length involuntary, discharges take place from the bowels, of a dark, offensive matter, and finally death ensues.

In the milder forms of the disease, or when it has been from the commencement of the attack subjected to an appropriate treatment, instead of an increased violence in the phenomena of each succeeding paroxysm, there is less decided heat of the surface, the pulse becomes slower and less frequent, there is less gastric distress, less pain and tenderness of the epigastrium; less pain of the head and back; the exacerbation is of shorter duration, and as it declines, the skin becomes softer and more moist, the tongue less coated, the countenance more cheerful, and the patient obtains often several hours of quiet and refreshing sleep; the remissions are more distinct, and of longer continuance, until finally a complete intermission occurs, followed by a state of convalescence — which is usually protracted.

There generally takes place in the milder and uncomplicated forms of bilious

remittent, a distinct exacerbation, followed by an evident remission, once in every twenty-four hours: but in the more violent and decidedly inflammatory form, the remissions, as we have already remarked, are so short and slight as almost to pass unnoticed, and hence the inflammatory bilious fever is described by many writers as a continued fever.

The duration of the disease varies with its degree of violence and its simple or complicated character—it may terminate in a few days or run on for several weeks—but its usual duration is from nine to fifteen days. It is not uncommon, Dr. Dickson, of South Carolina, remarks, especially among the most perfectly acclimated adult natives resident in malarious localities, and strangers long familiarized to these, to find bilious remittent fever of a very protracted duration; “the patient sinking, after the tenth or twelfth day, into a low form, resembling the less severe grades of typhus, and hence obtaining among us the designation of the typhoid stage of bilious fever. Here the well-marked lines which separate the period of exacerbation and remission, are almost effaced; the characteristic periodicity almost obliterated; the fever degenerates nearly into the continued type, and the patient, in the language of the older practitioners, ‘wades through’ the attack with no definitely regular changes observable from time to time, until, by the success or failure of our efforts in his behalf, he recovers or is lost. The pulse is small and chorded, the tongue throws off its fur, and is smooth, red, and dry, or smeared over, like the teeth and lips, with foul sordes; the stomach loses its irritability, and the vomitings cease; the stools are dark or even black; meteorism occasionally shows itself; there is muttering delirium, or disposition to heavy stupor and coma; the countenance is dull and inexpressive; muscular languor and great debility ensue, with nervous tremors on motion, and perpetual subsultus tendinum.” Dr. Dickson has known cases of this kind protracted, in three instances, to thirty, thirty-five, and fifty days, though the average, he observes, would scarcely reach beyond fifteen or twenty.

A careful analysis of the phenomena of bilious remittent fever shows, very conclusively, that the organs chiefly affected are the liver and stomach, and the results of post-mortem examinations confirm the accuracy of this deduction. In many cases, it is true, the symptoms during life, and the lesions detected after death, indicate more or less affection of the lungs or brain; but this latter is to be viewed as an accidental complication, and not essential to the disease. Basing our conclusions upon the investigations of Dr. Stewardson, (*American Journ. Med. Scienc.*, April, 1841, and April, 1842,) which are confirmed by the subsequent observations of Dr. Swett, (*ibid.*, Jan., 1845,) and correspond in many particulars with those of Mr. Twining (*Diseases of Bengal*, chap. v.), a morbid condition of the liver would appear to be invariably present in remittent fever—to be indeed diagnostic of the disease. It is nevertheless very certain that few cases of billous fever, of any intensity, occur without being attended also by indubitable lesions of the gastro-enteric mucous membrane—the evidences of which are very generally detected in those who die of the disease. In many cases a morbid condition of the liver is the principal lesion detected after death, while in others lesions of the gastro-enteric mucous membrane are the most prominent; in general, however, indications of disease in both these organs are sufficiently well marked in the same case. It has been proposed to distinguish the fever, according as the phenomena of hepatic or gastro-enteric disorder predominate, by the terms *hepatic* and *gastric*. *Hepatic remittents* being characterized by intense febrile heat; violent pain of the head; fulness and tension of the right hypochondrium, with pain and pulsation of this part and of the epigastrium; excessive irritability of the stomach; frequent and forcible vomiting, the ejections being without any trace of bile; a clean state of the tongue in the early stages; a decidedly yellow colour of the skin and adnata of the eye; great torpor of the bowels, until towards the decline of the disease, when a copious discharge takes place from the bowels of a dark or pitch-like tenacious matter, which may be considered as in some degree critical.

Gastric remittents are distinguished by a bitter taste in the mouth; great thirst, with a craving for cool and acidulated drinks; frequent vomiting of a green or dark-brown fluid; total loss of appetite, and generally disgust for every kind of food; a feeling of great weight and anxiety at the præcordia; pain and tenderness, chiefly at the epigastrium; intense pain of the loins and knees, and soreness of the calves of

the legs; severe and constant pain of the forehead; the tongue coated in the centre with a thick layer of yellowish mucus, and red at the sides and tip; the remissions distinctly marked; bowels costive, or when discharges from them take place, these are thin and watery, and often attended with griping and tenesmus: in the course of protracted cases the stools, not unfrequently, consist of a reddish fluid resembling the washings of meat. In the progress of the disease, the tongue becomes dry, cracked, and covered with a dark-brown or blackish crust; upon the separation of which the tongue presents a smooth, shining, and red surface. In the advanced stage, there is often retention of urine, difficulty of swallowing fluids, meteorism of the abdomen, &c.

Although we frequently meet with cases in which the predominance of hepatic or of gastric disorder, but especially of the first, is thus strongly marked; nevertheless, in the general run of cases, there is a complete blending of the two sets of phenomena.

There is one form of bilious intermittent still to be described, with which the experience of the last ten years has made the practitioner of the southern and south-western portions of the United States, unhappily, too well acquainted, under the denomination of *congestive fever*. "In certain localities, indeed," remarks Dr. Dickson, "the ancient inflammatory features of malarious endemics seem to have disappeared, and to have become supplanted by the more hideous and pestilential modification thus entitled."

The form of the disease to which we allude is marked by a diminished temperature and decreased sensibility of the whole surface of the body—the skin being, at the same time, soft, contracted, and often clammy, or wet with a copious perspiration. When a partial reaction takes place, the heat is never considerable, and it is often confined to particular parts of the surface. There are considerable and universal lassitude and debility; the head is confused and affected with vertigo, and sometimes with a deep-seated pain, or a sensation of oppressive weight or tension; the eyes are heavy, suffused and dull; the countenance is haggard, and the face pale, and of a dingy, muddy appearance; the pulse is small, frequent, and indolent, or struggling, compressible and variable; the tone of the voice is often changed, the articulation being slow and drawling, or imperfect and stammering. The respiration is anxious and laborious, with frequent sighing. The tongue exhibits, at first, but little change, but soon becomes dark-brown or black, especially in those cases in which the earlier stages of the disease have been marked by some degree of excitement. The stomach is occasionally irritable; the epigastrium and right hypochondrium are tender upon pressure, and more or less tumid; the bowels are torpid, and when stools are procured, they are dark-coloured and offensive, and often attended with termina and tenesmus. The mind is generally dull, indifferent, or confused, from the commencement of the attack, and, in the progress of the disease, sinks into a state of more or less complete stupor, or of low muttering delirium. The remissions of the fever in the congestive form are not well marked, or rather, there is an entire absence of the febrile exacerbations and remissions, the phenomena of the disease presenting but little other change than a rapid or gradual augmentation in intensity. In fatal cases, death, which may take place between the fifth and fifteenth days, or even later, is often preceded by hiccup, subsultus tendinum, involuntary stools, hemorrhage from the stomach or bowels, petechiæ, &c.

The congestive form of bilious remittent fever may attack suddenly, when it is apt to assume a peculiarly malignant form, and to terminate rapidly in death; in many cases, however, it is preceded by the same symptoms as the milder and more open forms of the disease. In the more violent attacks of congestive fever, "the system seems," to use the words of Dr. Dickson, "to sink at once prostrate before the invasion or exacerbation, which can scarcely, at times, be called febrile. Reaction, to use our technical phrase, does not take place, or very feebly, if at all. The skin is cold, and covered with a clammy sweat, as in the collapse of cholera; the pulse is weak and fluttering; the stomach is very irritable, with frequent and painful, but usually ineffectual, efforts to vomit; the countenance is shrunk, and pale or livid; there is often low muttering delirium, with shivering and fainting. In some cases, no complaint is made, a lethargic insensibility seeming to oppress the patient; in others, the most extreme anguish is endured by the miserable sufferer, who in his agony often

utters groans or loud cries. The vital powers are speedily and irrecoverably exhausted by the recurrence of a few such exacerbations, although the remissions in this class of cases are usually well defined, and full of transient relief and hope. The third, fourth, or fifth return of the train of symptoms delineated, for the most part, puts an end to the distressing scene."

The phenomena of congestive fever are evidently the result of defective innervation, and an impeded action of the heart and lungs; the blood, imperfectly decarbonized, accumulates in the interior organs, and thus prevents the full and regular exercise of their functions. The impression of the morbid causes by which the disease is produced is, in all probability, made primarily upon the nervous centre, and by depressing the energy of its action, gives rise to all the other phenomena which characterize this form of fever.

The propriety of denominating these cases of congestive disease bilious remittent fever, when they frequently run their course without exhibiting the slightest indication of febrile reaction, has been doubted by some. They are, however, produced by the same causes as the bilious fever, and, when the congestion is early removed, the case often assumes all the characteristics, and runs the same course as in the ordinary form of bilious fever, showing that the phenomena of the latter were merely masked or suspended by the congestive state of the principal vital organ.

The convalescence from an attack of all the forms of bilious remittent fever is always protracted. Relapses, from slight irregularities of diet or too early exposure, are not unfrequent. After severe attacks, the hair often falls entirely off, and is only slowly and imperfectly renewed, or the patient remains permanently bald. The functions of the stomach and alimentary canal, generally, are imperfectly performed for a long period after recovery, and require the utmost circumspection on the part of the patient to insure their complete restoration. Jaundice is a very frequent consequence of bilious remittents. This, Dr. Dickson states, "was noticed generally in the summer and autumn of 1824; very few patients recovering from the fever in that year without becoming icteric. Nor is it rare to meet with hepatic enlargement and induration, attended by a long succession of sufferings from visceral obstructions and mechanical impediments to the performance of the abdominal functions, and to the transmission of the abdominal circulation. Hence ascites and anasarca, and hence diarrhoea and dysenteries called hepatic, and occasionally melæna and true intestinal hemorrhage. The spleen suffers from remittents of malarious origin, as from their kindred intermittents, though not by any means so uniformly, sometimes undergoing a permanent and inconvenient increase of size or hypertrophy."

With the pathological anatomy of remittent fever we are but imperfectly acquainted; very few facts in relation to it have been recorded, and of these few many are loosely and imperfectly detailed, while those upon the accuracy of which reliance can be placed, are derived from too limited a series of observations to allow of any positive general conclusions being based upon them. That, however, the stomach and liver are the organs in which indications of disease are the most uniformly detected, all the facts in our possession very clearly prove, although the exact nature of the morbid lesion is not very clearly made out. "The closest attention to clinical observations," remarks Mr. Twining (*Diseases of Bengal*, chap. v.), "as well as the result of post-mortem examinations, convince me, that remittent fevers in Bengal, are invariably connected with local congestions, which often run rapidly into inflammations, attended with much interstitial effusion. The seat of these local affections is found principally in the stomach, intestines, cellular structure about the duodenum, and at the root of the mesocolon, more especially where it passes across the spine. The principal disease is also often found in the spleen, liver, brain, or lungs." Dr. Gerhard, of Philadelphia, maintains, as the result of his observations, that the anatomical character of the more severe forms of intermittent fever is to be detected in the spleen, liver, and stomach; the bilious and remittent fevers being "probably referable to the same class as the malignant remittents." These conclusions are confirmed by the observations made by Dr. Stewardson, in the Pennsylvania Hospital; by those of Dr. Swett, made in the New York Hospital; and by the results of dissections in single cases, as reported by Dr. Howard and others. Dr. Boyd describes the indications of disease, after death, in the bilious fever of Minorca, as occurring in the brain, lungs, liver, and stomach; the liver, he states, was in most instances enlarged,

often inflamed, with its inferior margin livid; the gall-bladder distended with viscid bile. The stomach and intestines were often inflamed; the villous coat being of a dark colour.

The appearances in the stomach are variously described; but in general, they are stated to be injection, with increased redness of the mucous membrane—sometimes thickening and occasionally softening of that membrane. In many instances, the same appearances are described as being present in the duodenum. The glands of Brunner, in this intestine, were, according to Dr. Stewardson, “developed in a remarkable manner,” in the cases examined by him; a similar development did not, however, occur in the fatal cases which fell under the notice of Dr. Swett.

In all the dissections made by Drs. Stewardson and Swett, and the one reported by Dr. Howard, the liver was found to be flabby, of a bronze colour, the two substances composing the organ being blended together in such a manner as to be scarcely distinguishable. The spleen was much enlarged and softened. The peculiar condition of the liver, just described, Dr. Stewardson believes to constitute “the essential anatomical character” of remittent fever: he thinks it highly probable that the same alteration of the liver will be found to exist in intermittents which prove fatal in their early stage; these and intermittents being, in his opinion, essentially the same disease.

Drs. Gerhard, Stewardson, Swett, and Howard, never detected in any case, after death from remittent fever, the glands of Peyer, and the other intestinal follicles, in a diseased condition. Although we believe that these parts will generally be found in a perfectly healthy state, nevertheless, it is certain that cases do occur, particularly of the more protracted forms of the disease, in which the evidences of follicular inflammation of some portion of the small intestines, terminating occasionally in ulceration, especially of the glands of Peyer at the lower end of the ileum, are present after death. Such cases are referred to by Dr. Geddings, of Charleston, Drs. Stevens and Vache, of New York, Dr. Harrison, of Cincinnati, as quoted by Dr. Dunglison (*Practice of Med.*, 2d ed., vol. ii. p. 450). A diseased condition of the glands of Peyer is stated to have been present also, in all the cases examined by Dr. Richardson in the New York Hospital, in 1840, (*Dr. Swett, American Journal Medical Sciences*, January, 1845;) and the same lesion was noticed by Dr. McWilliam in the African remittent fever.

The indications of congestion and of inflammation so frequently met with in the brain and lungs, point out the tendency there exists in remittent fever to disease of those organs, but they are not invariably present, nor are they essential to the disease.

In cases of congestive intermittents, the anatomical lesions are the same as those described above, excepting, that when death takes place previously to the occurrence of complete reaction, there is very generally extensive hyperæmia of the brain, lungs, liver, and spleen.

The diagnosis in bilious remittent fever will depend, in a great measure, upon the character which the disease presents in different localities and in different years, as well as upon the constitution, and other circumstances connected with the individuals attacked by it. In the milder forms of the disease, with distinct exacerbations and remissions, and no marked tendency to disease of the more important organs, particularly when these occur in persons of temperate habits, and of constitutions neither broken down on the one hand by previous disease, fatigue, or the depressing passions, &c., nor, on the other, marked by undue plethora, or a tendency to congestive or inflammatory affections of the central organs; by a judicious treatment, early commenced with, a favourable result may very generally be obtained. Even the more strongly marked inflammatory form of the fever seldom terminates fatally when vigorously treated from its onset. The congestive or malignant form of the disease is one in which the prognosis is much more unfavourable. In epidemics of congestive bilious fever the mortality is often truly appalling. Remittent fever, generally speaking, attacks with less severity, and is more readily controlled in those acclimatized, as it is termed, to the districts of country in which it occurs as an epidemic—that is, persons who, having passed through the disease, continue to reside in those districts. A stranger, recently arrived, particularly from a northern climate, and if a high liver, or of intemperate habits, or whose occupation subjects him to exposure and fatigue, is not only more liable to be attacked than the former class of individuals, but in such a one the disease will usually be marked by the greater severity, and be more liable to ter-

minate fatally The favourable symptoms, in individual cases, are, the mildness and comparative shortness of the exacerbations, the completeness and long duration of the remissions, particularly when the latter are accompanied by a free and diffused perspiration, and the patient falls into a quiet and refreshing sleep; the postponement of the succeeding exacerbations beyond their anticipated periods; the diminution or removal of the tendency to disease of particular organs; the occurrence of more regular and healthy discharges from the bowels; the tongue becoming cleaner and the pulse slower and less frequent, &c.

The bilious remittent fever, as we have already remarked, is a disease of hot climates and of the season of greatest heat; thus, while it constitutes the summer endemic of the southern portion of our Union, it seldom occurs in the middle and northern states, excepting during those summers that are marked by an unusual elevation of temperature; and here, as well as in the places where it prevails endemically, it ceases as the winter approaches. Whether the fever can be produced simply by exposure to a high degree of atmospherical temperature, is a question that has been frequently raised; and while many maintain the affirmative, others as positively assume the negative. It is very certain, however, that the long-continued action of excessive heat upon the body, by predisposing it to the action of other morbid agents, will, in this manner, often bring on an attack of bilious fever, independently of malarious influences. Thus, nothing is more common than for the disease to immediately occur in those who, after exposure to the midday sun, or after many hours of severe labour in intensely hot weather, allow their bodies to become chilled by sleeping on the ground in the open air after night; by neglecting to change their clothing when they become wet by a shower of rain or other cause; or, while the perspiration is running in streams from every pore, by throwing off a portion of their clothing, and sitting or lying in a draught of air, particularly after sundown. But while many of the sporadic cases are produced in this manner, and the same species of imprudence is a common exciting cause of the fever when it prevails as an endemic or epidemic, a large number of incontestable facts would seem to prove that its more common cause is a poison produced by the action of high degrees of solar heat upon such portions of the earth's surface as are ordinarily covered or soaked with water, when these are accidentally laid bare, or rendered dry by long-continued heat and drought. The presence of substances, whether vegetable or animal, liable to decomposition, is to be included among the causes capable of generating bilious fever in hot climates, and in the more temperate climates during summers of unusual heat. In our larger cities the disease always prevails most extensively, and in its more malignant forms, in the most confined and filthy districts, to which it is often entirely confined; or, when it makes its appearance in other parts of them, it can generally be traced to the presence of some local cause of infection. In Philadelphia, where bilious remittent fever was formerly a common disease of the summer and early autumnal months, it is now of rare occurrence, excepting in its mildest form; and this disappearance of the disease from our city is to be ascribed more to the better construction and cleanliness of our docks, and the greater attention which is paid to the paving, draining, and sewerage of our streets; to the filling up of the numerous ponds which formerly existed in and about the city, and to the preventing any mass of putrefiable materials from being stored in the city proper and surrounding districts, than to any very decided diminution in the temperature of our summers.

In the treatment of bilious remittent fever, the first and most important question that presents itself is, the propriety of direct depletion by the lancet. This must be determined solely by the character of the symptoms and condition of the patient in each case. In the more decidedly inflammatory cases, when the exacerbation is well-marked and prolonged — when it is attended by a hot skin, flushed and turgid face, severe pain of the head and back, great oppression about the præcordia, and a firm or chorded pulse — and when the intermissions are short and imperfect — more particularly in cases occurring in young, robust, and plethoric habits, and who have but lately arrived from a colder climate, there can be no doubt of the importance of blood-letting. In such cases a vein in the arm should be opened at the early period of the attack, and a sufficient amount of blood drawn off at once to reduce the violence of the exacerbation: one sufficient bleeding at the onset of the disease is far more efficacious and better adapted to prevent local determination than the repeated detraction

of small quantities of blood during its course. A repetition of the bleeding will seldom be demanded when the first operation has been carried to a sufficient extent; should, however, the succeeding exacerbation be marked by any degree of violence, we should not hesitate again to resort to the lancet; but the loss of a less amount of blood will be required to reduce the excitement than in the first instance. If direct depletion by the lancet has been neglected in the early stage of the disease, it will seldom be admissible subsequently—or, should symptoms seem to demand it, it must be employed with the utmost caution, and only to a moderate extent.

That numerous cases of bilious fever occur in which venesection is altogether unnecessary, is unquestionably true; in those, however, that are marked in their early stage by a high degree of arterial excitement, and a tendency to local hyperæmia or inflammation, the remedy cannot be dispensed with without danger to the patient. When timely employed, we have repeatedly been surprised at the very great and prompt relief produced by it—the violence of the exacerbations becoming reduced, the intermissions more complete, the skin cooler and more relaxed—the pain of the head relieved—the bowels readily acted upon; and a state of calmness, even of refreshing sleep, will often succeed to the previous restlessness and distress.

Local blood-letting is a remedy which is, perhaps, more generally demanded in bilious remittent fever than bleeding from the arm. In all cases attended with intense pain of the head, throbbing of the temples or delirium—or with pain or decided tenderness of the epigastrium and right hypochondrium, or with pain, and a sense of tightness or oppression about the thorax—in conjunction with general bleeding, or where this has not been judged necessary, the application of leeches or cups in the neighbourhood of the local suffering, in numbers proportionate to its intensity and to the age and condition of the patient, will invariably be followed by decided and, generally, permanent relief. The very great suffering which many patients experience from pain in the back and loins, we have, in repeated instances, known to be completely relieved by cups applied along the spine.

In regard to the propriety of cathartics in the treatment of bilious remittent fever, there is a much greater unanimity of opinion than in relation to blood-letting. There are few, if any cases, in which their operation will not be found beneficial, by removing from the intestines the vitiated secretions with which they are often filled—procuring regular stools, and by assisting to reduce the congestion of the portal system, and thus restoring the healthy functions of the liver and digestive organs generally.

It is not, however, every purgative from which good effects are to be anticipated in bilious fever. Under the free employment of certain cathartics the patient will often be found gradually to become more and more debilitated, and finally to sink into a state of stupor, with suffused eyes, weak, fluttering pulse, and other symptoms of extreme exhaustion. In such cases it will be found that the discharges from the bowels consist of a thin, serous fluid. Under the employment, however, of such purgatives as produce consistent, viscid, and dark-coloured stools, the indications of improvement in the symptoms of the disease are often prompt and striking. Calomel is probably one of our best cathartics in this disease. It may be given either alone, in a full dose, and followed by occasional doses of castor oil—senna tea—calined magnesia—or laxative enemata; or the calomel may be combined with rhubarb, or jalap; or, what is frequently a more effectual plan, a full dose of the calomel administered at first, and then, every three or four hours, according to the effects produced, moderate doses of the blue mass and rhubarb, with the addition of a small portion of ipecacuanha. By these means we shall generally succeed in freeing the bowels of the dark-coloured, viscid matter with which they are often loaded, and procuring more regular and healthy stools—after which, an occasional dose of the milder laxatives will alone be required.

In all cases of bilious remittent fever attended with a hot and dry skin, the application of cold to the surface, either by sponging it with cold water or cold water and vinegar, or by the application of cold water in the form of affusion, will have the effect, while it reduces the morbidly increased temperature, of relaxing the skin, and promoting perspiration, and in this manner shortening the exacerbations and inducing a complete intermission; even the exposure of the body to a current of cool air—by throwing off the clothing and opening the doors and windows of the patient's chamber, will in most cases be productive of the most beneficial effects. The mere

immersion of the hands and arms in cold water, by carrying off a portion of the morbid heat and allaying thirst and restlessness, will be found grateful to the patient, who not unfrequently will fall immediately afterwards into a refreshing sleep. Whenever the exacerbation is attended by intense heat of the surface and other symptoms of violent excitement, it is from the application of cold water to the surface by affusion or sponging that the most prompt and permanent advantage is to be obtained. Dr. Dickson ranks it among the most efficient of our febrifuge measures — far above the lancet, both in the extent of its adaptation, and in its degree of specific utility. All that we can hope or anticipate from blood-letting may be obtained, in the majority of cases, from the use of the cold bath, while the latter possesses this striking and obvious advantage, that we can repeat it as often as the symptoms are renewed that require its employment. The local determination which it controls most promptly, he very correctly remarks, is that to the brain, indicated by the headache, flushed face, red eye, delirium, &c., with a full, hard, bounding pulse. In such cases, the patient being seated in a convenient receptacle, a large stream of cold water should be poured over his head and naked body from some elevation, and continued until he becomes pale, or his pulse loses its fulness, or his skin becomes corrugated and he begins to shiver; he should then be dried and placed in bed, with just so much covering as he feels to be comfortable, the chamber being, at the same time, fully and freely ventilated; or without removing the patient from the bed, he may be supported in a leaning posture over its edge, and the cold water poured from a pitcher over the vertex.

The application of cold water to the surface is proper only when the temperature of the latter is considerably and permanently increased over the whole body, and at the same time the surface is perfectly dry. When the skin is cool or covered with moisture, the use of the cold water is inadmissible; it is also of doubtful propriety when there exists a decided tendency to hyperæmia or inflammation of the lungs, or in cases attended with diarrhœa. Its repetition is forbidden when it has occasioned a protracted chill or rigor; or the patient continues to feel cold or uncomfortable after its use.

The testimony of Dr. Dickson in relation to the curative powers of cold water applied to the surface in the bilious remittent fevers of the south is strong and pointed, and is fully borne out by the experience of physicians of other portions of the United States and of Europe, who have extensively employed it in the disease under consideration. Equally important with the external employment of cold water is its use internally; — allowing the patient to drink freely of cold, or even iced water or iced lemonade, is not only highly grateful to him, but it tends to diminish the morbid excitement, relax the skin, and promote a free and uniform diaphoresis.

Diaphoretics, particularly a combination of tartar emetic, nitre, and calomel, have been recommended and highly extolled by many practitioners, in the inflammatory forms of bilious fever. We have occasionally employed the combination just mentioned, but have never seen any decidedly beneficial effects produced by it, while the emetic tartar, in however minute doses administered, we have found, very generally, to augment the irritability of the stomach, and increase in this manner the distress of the patient. The neutral mixture, spirits of mindererus with the sweet spirits of nitre, or a weak solution of nitre in water, we have occasionally employed, but we must frankly confess, that we are not aware of any benefit resulting in a single case from the administration of either.

Upon the propriety of blisters in bilious remittent fever, there exists not a little dispute, their use being entirely condemned by some practitioners, while others rank them among our most valuable remedies. In the more inflammatory forms of the disease, until the violence of the exacerbation has been reduced by the remedies already detailed, from the application of blisters more injury than good will unquestionably result. So soon, however, as the exacerbations have become less intense, and the remissions more marked, in all cases where considerable gastric distress remains, or any considerable affection of the head or lungs, very considerable and prompt relief will always be obtained from blisters applied over the epigastrium and right hypochondrium, to the nape of the neck, along the spine, or to the chest.

In many cases of bilious fever there is so great a degree of irritability of stomach, that not only is everything taken into it rejected as soon as it is swallowed, but the patient is tormented with almost constant vomiting, or frequent ineffectual efforts to

vomit. This distressing symptom will, very often, be completely removed by the abstraction of blood from the arm, whenever this is admissible, or by cups or leeches over the epigastrium, or by a large blister applied to the latter part. Relief will also be obtained from small portions of ice held in the mouth, from the effervescing draught, the artificial mineral water of the shops, taken perfectly cold, or from iced lemonade or toast water. Minute doses of calomel, say a third to half of a grain, suspended in some simple mucilage, and repeated at short intervals, will, very generally, promptly relieve the gastric irritability and suspend the vomiting. In cases where every other remedy has failed in relieving the irritable state of the stomach, we have repeatedly seen it promptly removed by a grain of the acetate of lead, given every hour or two, dissolved in a small quantity of water.

By a few practitioners, a mercurial course is recommended in all the more violent forms of bilious remittent fever. Calomel is directed to be given first, in large doses, as a purgative, and subsequently, in small doses, continued until its specific effects are obtained; our own experience is not, however, in favour of this practice. In the inflammatory form of the disease, a mercurial impression is calculated rather to protract than to diminish the suffering and danger of the patient, while the milder forms of the fever may be readily controlled without it. Cases, however, occasionally occur in which a strictly alterative course of mercury will be found, we are convinced, highly advantageous. We have repeatedly observed, that after the violence of the exacerbations has been reduced, the patients will continue to be affected by a low form of fever, marked by a dry skin, a small irritated pulse, a dry dark-brown tongue, frequent watery discharges from the bowels, restless nights, and a total want of appetite, and have found this condition of things to be speedily removed by a cautious mercurial course, carried even to the extent of producing a very slight tenderness of the gums. In these cases small doses of calomel, or, what is preferable, the blue mass, combined with a portion of opium and ipecacuhana, may be given every three hours until the desired effect results.

A form of the disease, somewhat similar to the one just referred to, is described by Eberle as occurring "when, either from the imprudent employment of *irritating* purges, or from other causes, the mucous membrane of the intestinal canal is brought into a state of high irritation or subacute inflammation. The disease then generally loses its remittent form, and often assumes a low typhoid character, with almost constant delirium, a tender and tympanitic state of the abdomen; a dry, dark-brown, or black crust on the tongue, with clean red edges; watery and reddish stools; great prostration; and a very dry and hot skin. Cases of this kind frequently run on for several weeks, and the convalescence is always very gradual and tedious." The treatment directed by Dr. Eberle is leeches to the abdomen, followed by a large emollient poultice; small doses of calomel and opium—one-sixth of a grain of the former to a quarter of a grain of the latter—every two or three hours. The bowels to be kept open by laxative enemata, and the patient directed to partake freely of some bland mucilaginous fluid—such as barley water, very thin oatmeal gruel, or a solution of gum acacia in water; no other article of food being allowed. "In such cases," he remarks, "I have thought that considerable benefit was derived from epispastics on the legs just above the ankles; this measure is particularly useful when the extremities are cool, while the skin of the body is hot—a circumstance which is not uncommon in instances of this kind. An emulsion of balsam copaiba may also be frequently employed with unequivocal advantage. I have so often seen the most decided benefit derived from this emulsion, in cases attended with great irritation or subacute inflammation of the bowels, that I should consider myself neglecting an important curative means, were I to omit prescribing it in diseases of this character." "Although very considerable prostration often occurs in such cases, stimulants or tonics are by no means admissible."

Throughout the entire course of bilious remittent fever, little desire for food will be expressed by the patient, nor should any be allowed until after the disease is fully subdued; and then only barley or rice water, thin gruel or panada, in very moderate quantities. To allay the thirst, which is always present and frequently very urgent, the cold drinks, already pointed out, will be the most suitable. Perfectly cold toast water, slightly acidulated with lemon or orange juice, tamarinds, or currant or plum jelly, forms generally a very palatable beverage, of which the patient may partake at

short intervals. Small quantities of cold acidulated fluids, slowly swallowed, it is to be recollected, are far more efficacious in allaying thirst than large draughts hastily swallowed. Frequently sponging the patient's mouth with cold water, cleaning his tongue with a slice of orange, or allowing him to suck the juice of the latter, will aid very materially in abating his sense of thirst, and is always refreshing to him.

The chamber of the patient should be kept perfectly clean and cool, and, while a free ventilation is kept up, it should be guarded from any glare of light. His body should be slightly covered with bed-clothes, which, together with his linen, should be frequently changed. At all times the patient should be kept perfectly quiet, and the most perfect calmness and quietness should be maintained within and around his apartment.

In our enumeration of the symptoms of bilious fever, it was stated that great restlessness and wakefulness were very commonly present. So long as the period of increased excitement lasts, or there is any tendency to disease of the brain, the only means by which the restlessness of the patient is to be subdued and sleep procured, are the general remedies that have been already enumerated. After the morbid excitement, and whatever cerebral affection that may exist, have been reduced, a state of restlessness and an inability to sleep will often still continue from mere nervous irritability. Under such circumstances, the exhibition of an opiate in the form of Dover's powder, about bed-time, will very generally be found advantageous. By some practitioners, the extract of hyosciamus, combined with ipecacuanha, is preferred to any preparation of opium; while others, again, recommend the tincture of opium, combined with a vegetable acid, as tinct. opii ʒss, acid. citric. gr. v. to viij, aq. puræ ʒij, M.; of which a tablespoonful, or half an ounce, is a dose, to be repeated, if necessary, after a short interval.

We have now presented a general outline of the treatment of bilious remittent fever, in its mild and inflammatory forms. It is hardly necessary for us to repeat, that the extent to which each remedy is to be carried, and the propriety of its repetition and continuance, as well as the period and particular circumstances of the disease to which it is adapted, must be determined by the application of the general principles of pathology and therapeutics to each case we are called upon to treat. Although this will demand a degree of judgment in the practitioner, which he can only acquire by habits of close attention and nice discrimination, and a somewhat extended field for observation, it is the only course from which success can be anticipated in the treatment of a disease, which, like the one under consideration, varies so much in its character in different seasons and localities, and even in different individuals attacked at the same place and during the same period.

When the disease, notwithstanding our best directed efforts, runs on to that stage when its symptoms resemble those of the latter stages of typhoid fever, nothing is left us but to endeavour to sustain the patient's strength, and to remove or relieve any urgent cause of distress or danger. He should be allowed such nourishing food as is easy of digestion, as beef tea, sago, arrow-root, and the like, with wine-whey, wine, pure or diluted, porter, milk-punch, or brandy, in quantities adapted to the emergency of the symptoms and the effects produced. Sinapisms or blisters may at the same time be applied to the inside of the thighs or calves of the legs, and to the epigastrium. If diarrhoea be present, this we should attempt to abate or suspend by opiate and astringent injections. The singultus, which is often present to a very distressing degree, may be frequently allayed by the camphor julep; and the meteorism of the abdomen may often be considerably relieved by the spirits of turpentine, thirty drops of which may be given every two or three hours; this latter is said indeed to be among the safest and most useful remedies at this stage of the disease; it will often be found to produce a beneficial impression upon the diseased mucous membrane of the alimentary canal, to suspend the frequent griping stools, and to correct the morbid character of the discharges. In the sinking stage of bilious remittent fever, the effects of ammonia, with a strong decoction of serpentaria or valerian, or combined with the aromatic confection, have been represented as peculiarly beneficial; but, unfortunately, when the disease has arrived at this stage, every remedy will too often be found unavailing, and our best directed efforts to save the life of the patient ineffectual.

A question has been agitated as to the propriety of and the proper period for the

administration of quinine in remittent fever. Without attempting to detail the various and opposite opinions that have been advanced upon this subject, we shall merely remark, that in the simple and inflammatory forms of bilious remittent fever, it would be reasonably inferred from the character of the disease, that the quinine is not a remedy adapted to control the symptoms of its earlier stages, and the correctness of this inference has been very fully established by the result of experience. So soon, however, as a complete intermission has been obtained, and no indications of visceral hyperæmia or inflammation are present, a cautious administration of the quinine in solution, with the addition of a few drops of the elixir of vitriol, and to the extent of from three to five grains, repeated at such intervals as the circumstances of the case may demand, in conjunction with a cold infusion of serpentaria or some agreeable aromatic, will be found, in general, to cut short the remaining symptoms of the disease, and accelerate the period of convalescence. The more violent the form of disease has been, the more important is the administration of the quinine, so soon as a complete remission has been obtained, and the larger should be the dose of the remedy.

During the period of convalescence, the patient should be guarded from the effects of any sudden transitions of temperature, and from incurring the least degree of fatigue by sitting up too soon or for too long a period; and while his mind should be occupied and amused, at proper times of the day, by the conversation of his friends, or some light occupation, fatigue from this source must also be avoided. His bowels are to be kept regularly open, which may generally be done by allowing him to partake, in moderation, of any of the perfectly ripe subacid fruits of the season; if they, however, should become closed, some mild laxative should be occasionally administered. In regard to diet much caution is to be observed. The patient should be confined at first to moderate quantities of the more agreeable farinaceous articles; and if his strength has been greatly prostrated, a glass or two of sound wine will not be improper. As his digestive powers improve, he may be allowed plain beef or chicken broth, oysters slightly cooked, parboiled eggs, and finally, the lighter and more readily digested meats, plainly but nicely cooked. The appetite often remains, for a long time, weak and capricious—and frequently, some agreeable bitter infusion, as calombo, quassia, or the wild cherry tree bark, will be proper, to augment the tone of the stomach. In all cases, the food of the patient should be presented to him in small quantities, and served with great neatness; large masses of food, slovenly prepared and served, will in general disgust, and thus destroy what little appetite may exist. In regard to exercise and exposure to the open air, the convalescent should undertake them only so soon as he has acquired some degree of strength, when a trip to some agreeably situated watering-place will often restore him to perfect health, more quickly than can be effected during his continuance at home, where his mind is too early directed, with anxious care, to the concerns of his family, his business, or profession.

In the congestive form of bilious fever, the indications of cure are, to rouse the nervous energies of the system, to relieve as quickly as possible the central organs from their state of hyperæmia, and to restore to the capillaries of the surface their healthy action. Among the most important of the remedies calculated to effect these objects, is the external application of heat to the surface of the body in the form of the warm or hot bath, the vapour bath, or in a dry form, by bags of heated bran, salt or sand, bottles of hot water, &c. We shall often succeed in rousing the patient by immersing the whole of his body, as early as possible after he is attacked, in water, of a temperature proportioned to the urgency of his symptoms; that is, of a greater or less degree of heat, within proper limits, according as the temperature of the skin, the state of the pulse, and the general strength is more or less depressed, and the greater or less degree of torpor of the nervous system; at the same time applying brisk friction with a soft brush or a flannel cloth, to the whole surface, and particularly to the epigastric and abdominal regions. On coming out of the bath the patient is to be rubbed dry with heated flannels, put into a well-aired bed between blankets, and supplied with some mild diaphoretic drink, as the infusion of serpentaria, of tepid warmth. A full dose of the Dover's powder, or a combination of opium, camphor and ipecacuanha, one grain each, with ten of nitre, given at this period, will often be found particularly beneficial.

Next in efficacy to the warm bath, we may rank external stimulants; sinapisms

should, therefore, be early applied to the extremities and over the epigastrium, while frictions, with some stimulating liniment, are made along the spine. By some practitioners internal stimulants have been strongly recommended, as a means of rousing the central organs from their state of torpor, and bringing on a general and moderate reaction. It is probable that in certain cases, where the stage of congestion is very considerable, and it is not promptly and permanently relieved by the remedies already recited, some mild stimulant, as ammonia, turpentine, or wine-whey, may prove advantageous, if cautiously administered and its effects carefully watched; as a general rule, however, the free use of stimulants, particularly of the more active class, is calculated to produce injury rather than good.

An active cathartic, as the combination of calomel or blue mass with aloes and extract of colocynth, followed in a few hours by the compound infusion of senna, will very generally cause a copious discharge from the bowels, of a dark-coloured, tenacious matter, and thus by assisting to unload the portal system, will often produce a marked improvement in the general symptoms of the disease. In the treatment of congestive bilious fever, the abstraction of blood from the arm, in the first period of the attack, or subsequent to the use of the warm bath and frictions to the surface, has been recommended by many practitioners as an important remedy—one indeed from which more prompt and certain relief is to be obtained than from any other that we can employ. The evidence in its favour is certainly very strong; and we believe that there are many cases in which prompt reaction may be produced, and the danger of permanent local disease prevented by its employment. The greatest judgment and caution, however, are demanded in the use of the lancet in all cases of congestive disease; the effect produced upon the pulse must be carefully watched, and the flow of blood instantly stopped if any disposition to sinking is detected. To determine beforehand the amount of blood that it will be proper to take away is impossible—the effects produced—the relief obtained, are the only guide. Even, however, when during the flow of blood the pulse becomes more free and developed, only a moderate quantity should be taken away at once—even should we have to repeat the operation after a short interval. Occasionally but a drop or two of blood will escape from the vein when it is first opened, but, after a short time, the circulation becoming more free, the blood flows in a full stream, with decided relief to the patient. It is only, however, in the early stage of the highly congestive forms of bilious fever that venesection is admissible, and even in these the flow of blood should be instantly arrested if the pulse is found to sink, or even to remain oppressed and undeveloped after a small quantity has been lost.

When prominent symptoms of hyperæmia of the brain, lungs, or other important viscera present themselves, the application of cups to the head, chest, or in the neighbourhood of the affected organ, will often be found decidedly advantageous.

As soon as reaction takes place, the use of quinia should be commenced with, in large doses, repeated at short intervals. Of the curative powers of the quinia in this form of fever we have the fullest and most unquestionable testimony. Many of the physicians of the southern and western portions of the United States place their chief reliance, in congestive remittent fever, upon the sulphate of quinia prescribed in enormous doses—twenty, thirty, and even fifty grains, repeated at short intervals until a solution of the disease is obtained; but although we are convinced of the propriety of large and frequent doses, those alluded to are certainly excessive and uncalled for.

The period of convalescence from the congestive form of bilious fever will require the same management as that from the more open and inflammatory forms.

In the highly malignant remittent, occasionally met with, in which the vital forces, crushed at once by the overwhelming intensity of the morbid cause, fail apparently to exhibit any resistance or reaction, the strength must be sustained, to use the words of Dr. Dickson, (*Essays on Pathol. and Therapeut.*, vol. i., p. 319,) by every means within our power, while we have immediate recourse to the most impressive revulsives. The hot bath, sinapisms, vesications, must be quickly and assiduously applied, while we stimulate, by the freest use of internal remedies—carefully selecting such as are best adapted to the circumstances presented. If, as is often the case, the patient suffers severely from pain in the chest or abdomen, large doses of opium or morphine must be administered, while we keep up his sinking strength by brandy, ammonia, or ether. In the superintendence of such cases we should rather incur the risk of over-

stimulating transiently, than fall below the requisite point of excitement in our use of stimulants. Dr. Dickson is fully persuaded, that he has seen more than one patient die from the timidity of his physician in this respect; who, keeping at a cautious distance behind the disease, would not venture on the exhibition of any excitant adapted to the feeble excitability, until the vital energies upon which alone stimulants can act, had become worn out and exhausted. If the stomach will bear it, it will be proper to administer, also, the quinine in full doses. The remedy will be often retained, and, whenever such is the case, will do good. "Alternate, combine, and recombine your stimulants, the effects of which may perhaps be aided by warm and nutritious fluids, wine-whey, arrow-root with wine, wine alone or spiced, brandy with milk or in mucilage. These energetic measures will usually be found effective within a very short period—improving the pulse, and arousing the general powers of the system. Yet you must not permit yourselves to be disheartened into inaction by their apparent inefficacy or slowness of impression. Persevere, not only while there is a reasonable hope, but even after all hope seems extinguished; and you will feel yourselves amply repaid by the gratification, which will not perhaps be denied you, of saving more than one fellow-creature from the destruction which seems inevitably to await him."—C.]

LECTURE XLIII.

Epistaxis. Bronchocele; Cretinism: their Phenomena and Probable Causes Medical and Surgical Treatment of Bronchocele.

I YESTERDAY finished what I had to say respecting intermittent fever: its symptoms; its cause; and its cure. The subtle poison which produces it, is thickly distributed over the fairest portions of the habitable globe: blighting human health, and shortening human life, more often, and to a far greater numerical amount, than any other single cause whatever. Known only by its noxious effects, holding out no signal of its presence, this unseen and treacherous enemy of our race has yet been tracked to its haunts and lurking-places, and detected in some of its habits. It was necessary, therefore, that I should enter somewhat fully into the history of the malaria, and show how it may sometimes be shunned, sometimes be averted; how, also, in this climate at least, the effects it has already produced upon the human body may be successfully combated. But I shall not pursue, in further detail, the ravages committed by this invisible agent, and the remedies they require, in hotter and less favoured regions than our own. Of these, personally, I know nothing; and I must refer you, for information on such diseases, to authors who have seen and treated them: particularly to Drs. Lind, Jackson, Bancroft, Johnson, and Sir William Burnett.

It is customary with writers and lecturers, to pass from the consideration of ague to that of continued fever. A paroxysm of ague has been regarded as exhibiting a paradigm or sample of fever in general. But this has always appeared to me rather an ingenious refinement, than a useful matter of fact. Practically, I see nothing to be gained by the association. Intermittent fever, it is true, does often run, in hot climates, into the remittent, and the remittent into the continued form. But these are very different disorders from the continued fevers with which, in these climates, and in this country, we have to do. Intermittent fever, and continued fever, as we see them, differ in their phenomena, in their cause, and in their treatment. They are alike, inasmuch as they both are called fever, and both are attended, in some part or other of their course, with *pyrexia*; but in essential symptoms, I have known many a compound fracture more like continued fever than any ague that we are likely to witness. I shall take up the subject of continued fever, then, in connexion with the eruptive febrile diseases, with which it has many strong links of analogy; and I

resume the consideration of the disorders that come within the province of the physician, according to their anatomical seat.

After what was stated of hæmorrhage in general, in an earlier part of the course, I hardly know whether *epistaxis* needs or deserves any formal notice. There are, however, some points relating to this simple, and commonly harmless hæmorrhage, which it may be worth while very briefly to touch upon. Sometimes it is a remedy; sometimes a warning; sometimes really in itself a disease. The readiness with which the mucous lining of the nasal passages pours forth blood is familiar to the experience of every school-boy: who "often wipes a bloody nose." A slight blow, brisk exercise, a strong bodily effort, a fit of sneezing, or the summer heat, is sufficient, in many boys, to make the nose bleed; and this facility of hæmorrhage furnishes, often, an index of some unnatural state of the circulation: and especially of undue fullness of the vessels of the head. But the import of this symptom is not always the same. Epistaxis may indeed be taken as affording an epitome of the various forms of capillary hæmorrhage. In childhood and early youth it is idiopathic, dependent upon active congestion, and probably arterial. It is nature's favourite mode of blood-letting at that period of life. In old age it is symptomatic, the result of passive or mechanical congestion, and probably venous. In some adult persons it happens periodically, and is habitual: and its *suspension*, rather than its *occurrence*, becomes a token of disease or of danger. In young women it is not seldom vicarious of suspended menstruation: in men it is apt to take the place of hæmorrhoids. Lastly, it may proceed from disease in the nares themselves; or form a part of a more general hæmorrhagic disorder.

It is unnecessary to go at length into the phenomena of epistaxis. The main phenomenon becomes obvious at once both to the patient and to those around him: and the accessory and incidental circumstances are easily discoverable when the attention is aroused to them by the sight of the blood. Usually the blood flows *guttatim*; in a succession of drops; but these may follow each other so fast as to constitute a little stream. Sometimes a few drops only fall; sometimes several pints are lost. A moderate hæmorrhage of this kind is generally succeeded by a sense of relief and refreshment. A large efflux of blood may cause pallor, faintness, debility, exhaustion, even death.

Active idiopathic epistaxis, as it occurs in children, is almost always salutary, and may be left to work its own cure. When it runs into excess, or is too often repeated, it may be checked by applying cold water to the forehead and to the bridge of the nose. The sudden contact of some cold substance with a distant part of the surface of the body will often have the effect of restraining the hæmorrhage: apparently by producing a general and sympathetic constriction of the superficial blood-vessels. This is doubtless a reflex phenomenon. The nursery remedy is to slip a cold key down the child's neck, between its back and its clothes. The aspersion of cold water is still better. The matico leaf, crumbled into a powder, and snuffed up the nostrils, will often stop the bleeding at once. Besides these external appliances, cooling laxatives should be given; and if the bleeding prove obstinate, some astringent internal remedy may be thought proper. Those which I have hitherto found the most efficacious are the gallic acid and the acetate of lead. But I have been recently informed by Dr. Latham that his experience has led him to trust much to *mercury* in the management of epistaxis; and that the same indications have governed him, in adapting its use to this form of hæmorrhage, as serve to guide him in cases of inflammation. Thus, when the hæmorrhage has been profuse and frequent, and moderate depletion by blood-letting, or by purgatives, has not arrested it, he has brought the constitution rapidly under the influence of mercury, and as soon as the mouth became sore, the hæmorrhage has ceased, not a drop more of blood has been lost. Again, when the epistaxis has been, not copious, but habitual or frequently recurring, without any excess of vascular action, or any other apparent ailment in the constitution at large, Dr. Latham has often cured his patient by a moderate salivation, gradually induced, and continued for a few weeks.

In conversing with Dr. Southey on the same subject, I find that he also has been taught by experience to rely upon mercury as almost a specific remedy for obstinate

hæmorrhage, occurring under similar conditions, from whatever organ of the body it may proceed.

This plan of treatment it is therefore my purpose to prove, as future opportunity may permit.

[Habitual epistaxis may often be suspended without danger to any internal organ by a blister to the back of the neck.—C.]

When epistaxis *begins* to show itself in advanced life, it is a symptom which cannot safely be neglected: for it indicates that the veins of the head are loaded. It implies a morbid condition that requires to be redressed. You will look for disease of the heart—or for threatenings of apoplexy—and take your measures accordingly. The blood-vessels which ramify upon and beneath the pituitary membrane, communicate by indirect inoculation with the veins and sinuses of the skull, as well as with the jugular veins. You see, therefore, how it is that hæmorrhage from this membrane may perform the office of a safety-valve, and protect the important organ within the cranium from impending mischief.

On the other hand, when epistaxis, which is known to have been habitual, fails to recur at or about the usual periods, you will look, with a jealous care, into your patient's state, and watch for and obviate any tendency to plethora capitis.

When epistaxis forms a part of more general hæmorrhagic disease—as when it occurs among other symptoms of purpura—its treatment merges in that of the whole malady.

In any case, if the flow of blood be excessive, and cannot be restrained by the ordinary remedies, but is exhausting the patient's strength, it becomes an absolute disease and it will be requisite to staunch the blood by manual expedients.

These consist in stopping the bleeding orifices mechanically; which is most effectually to be done by plugging the cavity. A dossil of lint must be carefully inserted into the bleeding nostril. Its mechanical effect, which is pressure, may be chemically aided by first wetting the lint with a saturated solution of alum. The mode of introducing these plugs it is the business of the surgeon—and not mine—to teach. The operation is not a very comfortable one either to bear or to perform.

A very simple mechanical remedy has been lately announced by Dr. Negrier, of Angiers; who discovered it (he says) by mere accident. The patient is to raise one or both of his arms above his head, and to hold them for some little time in that position. Dr. Negrier declares that, during an experience of three years, he has never known this method fail to arrest the bleeding. His explanation of its *modus operandi* is not very satisfactory. The expedient itself is however so easy, so prompt, and, even if unsuccessful, so harmless, that its real value deserves to be tested—and will soon probably be settled—by an ampler trial.

[In broken-down constitutions—especially in persons of long-continued intemperate habits—a form of epistaxis will often occur, which it is very difficult to control. Besides mechanically plugging up the nostrils in these cases, we have found the most effectual remedy to be quinine, given in conjunction with either the gallic or tannic acids, and a moderate portion of opium.—C.]

Before we trace this mucous membrane downwards, through the mouth, to the inside of the throat, let me turn your attention to a singular disorder which may be deemed external, for it is scarcely more than skin deep: I mean that enlarged state of the thyreoid gland to which the name of *bronchocele* has been given. This word is not merely derived from the Greek, but was used by the Greek writers in the same sense in which we now employ it. In Switzerland, where it is very common, and in France, the complaint is called *goitre*; a corruption, it is believed, of the Latin “guttur,” the throat. It is known in England as the *Derbyshire neck*; from its frequent occurrence in that country.

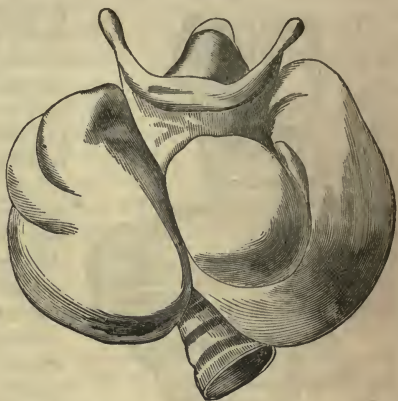
The term bronchocele has been sometimes applied indiscriminately to all protuberances or swellings in front of the throat; or, at any rate, to all enlargements of the thyreoid gland; whereas it should be restricted to *hypertrophy* of that part: an exaggeration of its natural structure, with augmentation of its volume. The texture of

the gland becomes coarser; its blood-vessels grow larger and more numerous; its cells are magnified, and filled with a thick, viscid secretion. It usually presents a soft, smooth, elastic tumour, which is neither painful, nor tender, nor discoloured. The lobes of the gland become more obvious. Sometimes the whole tumour is irregularly lobulated: sometimes the exact form and relative proportions of the gland are preserved, each lobe and portion being equally increased in size. Occasionally there is a soft uniform or irregular swelling, without much distinction of parts. Alibert states that the right lobe is more frequently enlarged than the left. Mr. Rickwood found it so in every instance of bronchocele that came under his notice in the neighbourhood of Horsham.

Unless the tumour be very large, it follows all the motions of the larynx: and this is a point of considerable importance whenever the diagnosis is at all doubtful. It is just possible that an enlarged lymphatic gland, or an encysted tumour in the neighbourhood of the larynx, or even a collection of pus thereabouts, might, in some degree, embarrass the diagnosis. But, by placing the head and neck in different successive positions, swellings of this accidental kind may, in general, be ascertained to be unconnected with the larynx: and they do not follow its up and down movements when the act of deglutition is performed.

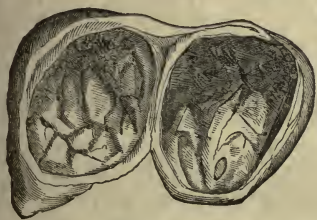
It is of importance to know, also, that the gland itself is subject to different kinds of enlargement. It may swell from inflammation, chronic or acute: and then it will be hard, and tender, and painful. But it does not seem very prone to inflame; and probably Dr. Copland is right in his opinion that inflammation occurs spontaneously in this organ in scrofulous persons only. Baillie and Alibert speak of it as being occasionally the seat of cancer; but that must be very rare. Sometimes cartilaginous

FIG. 29.



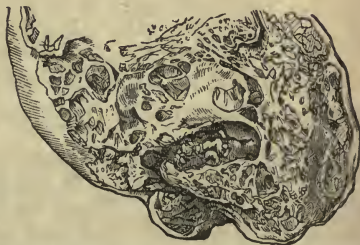
Goitre. From a preparation in Professor Mütter's collection.

FIG. 30.



Ossified thyroid gland. From a specimen in Dr. Gross' cabinet.

FIG. 31.



Section of a bronchocele, showing calcareous deposits. From the Middlesex Hospital Museum.

or ossific deposits take place in the gland. It is necessary, I say, to be aware of these circumstances, and to distinguish one kind of thyreoid tumour from another: for some of the morbid changes just referred to are clearly beyond the power of any medicine to remove; and if all forms of enlargement incidental to this part are lumped together under one common name of bronchocele, we shall be liable to arrive at false conclusions concerning the power of remedies over that disease.

Bronchocele is not, *in itself*, a painful disorder: nor does it taint the system, or affect the constitution in any way. It has no quality of malignancy about it. It is always, however, a deformity; and by its mechanical effects, that is, by its weight when large, and by the pressure it exercises on contiguous parts, it may occasion great distress, and suffering, and even death itself. The size, and the effects, of the

tumour both vary much in different cases; but its *injurious* effects are not always, though they are generally, in proportion to its bulk. Sometimes there is no more

FIG. 32.



Bronchocele, from the King's College collection. The œsophagus is seen to be pushed to the right side by the tumor.

than a slight fulness of the throat, which some persons, I believe, think rather graceful than otherwise. Now and then, the swelling, after its first commencement, develops itself with great rapidity; but its ordinary progress is slow. It often continues for months, or years, without reaching any extreme or very troublesome magnitude. Sometimes it remains stationary for a considerable time, and then *suddenly* increases, without any apparent cause. The worst effects of bronchocele are its interference with the circulation, and with respiration. By its pressure it may obstruct the free descent of the blood through the veins of the neck, and give rise to headache, giddiness, noises in the ears, confusion of thought, and a turgid condition of the head and face. Or, by pressing upon the wind-pipe, it may cause hoarseness, wheezing, and dyspnoea. It may even impede deglutition. But these effects, I say, do not depend altogether upon the actual size of the tumour. A very large goitre may produce no other inconvenience than what results from its weight, and its unseemly appearance. It may surround all the front and sides of the neck like a thick collar, and rise as high as the ears; or it may hang down in a pendulous lump, and be supported upon the chest. Nay, the tumour is said to descend, in some rare instances, so low as to be in contact with the abdomen: and Alibert mentions one case in which the swelling was of a tapering cylindrical shape, and reached to the middle of the thigh. On the other hand, a small tumour, not bigger than one's fist, especially if it happen to occupy the central portion, or what is called the isthmus, of the gland, may so press inwards upon the trachea as materially to hinder the breathing, and even to threaten suffocation. A pupil now attending the hospital has informed me of a case, which he himself saw, of death produced by the encroachment of a bronchocele; not so much, however, from suffocation as from starvation: for the swelling encircled the trachea, and came at last to press so much upon the woman's œsophagus, that she could not get food into her stomach. I suppose that the reason of these differences may be sometimes found in the manner in which the tumour grows, and in its relative situations. When it is bound down by the muscles of the neck, it presses, as it continues to enlarge, upon the parts behind it. When it is not so confined, the skin readily yields, and the entire growth of the tumour takes place anteriorly.

This disease is much more common in women than in men. Indeed we seldom see it, in this country, except in females. Yet I happen to have an example of it now (December, 1837) in a male among my patients in the hospital. Dr. Andrew Crawford states that forty-nine cases were admitted into the Hampshire County Hospital, in ten years, and forty-eight of these were in women. Of seventy patients admitted at the Chichester Infirmary in nine years, two only were males, and they were boys of a very feeble and feminine habit, and backward for their years. Among one hundred and sixteen patients of Dr. Manson's fifteen were men. Taking an average from these three lists, we have one male for twelve females. It is well to bear in mind that our fashion of dress renders a small bronchocele much more noticeable, much less easily concealed, in women than in men. In the former the swelling has been known to come on, or at any rate to increase rapidly, during their confinement in child-bed: and it is frequently observed to undergo a temporary enlargement at the menstrual period. Dr. Copland has seldom met with an instance in the female, unconnected with some kind of irregularity in the catamenial discharge, or disorder of the uterine functions; and he never saw a case in which the disease made its appearance before the period of commencing puberty. In Switzerland, and in some parts of India, where the complaint is much more prevalent than here, the proportion

of males affected is greater; and it begins, often, prior to the age of puberty, in both sexes. It seldom shows itself earlier than the age of eight or ten. Dr. Elliotson states, indeed, that he himself, when in Switzerland, saw goitre in a little boy only four years old; and the natives told him that it rarely made its appearance before the age of six. But children have been *born* goitrous. M. Godelle, physician to the hospital at Soissons, had a preparation of the body of an infant, which lived a few hours only, and which came into the world with a goitre; the mother being affected with the same disease. A case is mentioned in the *London Medical Repository* of a child born in Derbyshire with bronchocele of considerable size. The disease, therefore, undoubtedly may be *congenital*: and one of the facts I have just mentioned points to the question of its being *hereditary*. It is said to be so; and there is much probability in favour of that opinion. Children born of goitrous parents often have goitre. But that, you will say, may depend upon their being in the same place, and exposed to the same causes, which produced bronchocele in the mother or father. Dr. Crawford states, however, that he knew a woman, with goitre, whose grandmother, father, paternal aunt, and cousins, also had it, although they did *not* all live in the same place, and no other person in their respective neighbourhoods was affected by the disease.

Admitting, what seems probable, that the disease may sometimes be hereditary, in the sense in which I formerly explained that term, there can be no doubt that it is often *acquired*.

In the first place bronchocele is *endemic*—prevalent in certain localities, and scarcely occurring elsewhere. And persons who, being previously well, go to live in those localities, often become affected with the complaint: and persons who migrate *from* those localities, having the complaint upon them, sometimes get rid of it by the mere change of residence. The physical circumstances of the places thus selected by the disease have been studied with the natural hope of discovering what the cause may be of an effect so singular. Some morbid quality of the *air* was long suspected. The habitats of the unknown cause of bronchocele appeared at first sight to be very much like those of the malaria. Goitre abounds in the hollows of many mountainous districts; among the Alps, for example, and in the Pyrenees. This was notorious to the ancients. Juvenal asks—

Quis tumidum guttur miratur in Alpibus?

And it is in the deep, close, and humid valleys of Switzerland, which lie at the feet of, and between, high mountains, that bronchocele is most common. Several writers, who have personally investigated this subject in places where goitre is rife, concur in the belief that it depends upon insalubrity of the air, arising from the peculiarities of the situation. They affirm that it is most frequent in low, damp, confined spots, where the stagnant atmosphere is seldom stirred by wholesome breezes; and where the sun, in summer, has great power. Dr. James Johnson remarks, "We find in the Valais (one of the Swiss cantons) and in the lower gorges or ravines that open on its sides, both cretinism and bronchocele in the most intense degrees. As we ascend the neighbouring mountains, cretinism disappears, and goitre only is observed. And when we get to a certain altitude, both maladies vanish." Dr. Reeve, again, states that "all the crétins he saw were in adjoining houses in the little village called La Batia, situated in a narrow corner of the valley, the houses being built up under ledges of the rocks, and all of them very filthy, very close, very hot and miserable habitations. In villages situated higher up the mountains, no cretins are to be seen."

The *cretinism* mentioned in these quotations is a strange and melancholy disease: a sort of idiocy, accompanied by (and doubtless dependent upon) deformity and imperfection of the bodily organs. The mental affection exists in all degrees, from mere obtuseness of thought and purpose, to the complete obliteration of intelligence. Many of the cretins are incapable of articulate speech; some are blind, some deaf, and others labour under all these privations. They are mostly dwarfish in stature, with large heads, wide vacant features, and goggle eyes, short crooked limbs, flabby muscles, and tumid bellies. The worst of them are insensible to the decencies of nature, and obey, without shame or self-restraint, every animal impulse. In no other class of mortals is the impress of humanity so pitifully defaced.

More recent and extensive observation of the localities infested by goitre have rendered it improbable that the disease derives its origin from any deleterious properties of the air. Certainly it is not owing to anything that is common to *all* mountainous countries. Some parts of Switzerland are free from it. So are the Highlands of Scotland. It is met with also in flat situations—as in Norfolk. I have seen several cases of it in Cambridgeshire, which is a *very* flat county. In one village in particular, about five miles from Cambridge, it is extremely common. There are some striking facts collected by the celebrated and philosophic Humboldt, which go to show that the prevalence of bronchocele does not depend on any particular configuration of the surface of the earth, nor on any peculiar condition of the atmosphere. He tells us that in South America bronchocele is met with, both in the upper and the lower course of the Magdalen river; and in the flat high country of Bogota, 6000 feet above the bed of the stream. The first of these regions is a thick forest; while the second and third present a soil destitute of vegetation. The first and third are exceedingly damp; the second peculiarly dry. In the first the air is stagnant; in the second and third the winds are impetuous. In the first two the thermometer keeps up all the year at 22 or 23 degrees of the Centigrade scale: in the third it ranges between 4 degrees and 17.

The researches of Mr. McClelland, in India, lead to the same conclusion. He found goitre extremely frequent in one portion of the district which he surveyed, while the other portion was almost exempt from the complaint, “although an equality of moral as well as physical circumstances appeared to affect the whole. The external alpine characters of the province are the same in every part, the inhabitants all belong to the same tribes of Hindoos, and are subject to fewer irregularities in their mode of life than any other people in the world.”

The different localities of the villages, in the portion where goitre was *not* prevalent, he describes as being as diverse as can well be imagined. “Some are erected on narrow ridges, others in deep valleys, surrounded by abrupt and lofty mountains; others on rugged declivities, between lofty peaks on one side, and deep ravines on the other, into some of which the sun can scarcely penetrate. The different altitudes of these villages vary from 2000 to 6000 feet.”

Facts of this kind have turned the attention of scientific inquirers towards the only other obvious source to which the disorder could, with probability, be attributed, viz., the quality of the *water* used for drinking. Wherever goitre prevails, the popular belief assigns it to the water, as a cause: and the more accurately the search is prosecuted the more strength and likelihood does this supposition acquire. Its very universality is a presumption in its favour. The disease was formerly ascribed to the use of *snow* water: a notion which originated, I imagine, in its frequent occurrence in alpine regions. But the people in almost *all* the valleys of Switzerland drink the water that comes from the Glaciers; while bronchocele is known in *some* of the valleys only. It prevails also in certain spots where pump water is used, and *there* the people accuse the *pump* water of producing it. Besides, goitre occurs in other countries, where the snow never lies long, as in Derbyshire; and even in Sumatra, where there is *no* snow. Dr. Bally, a native of a goitrous district in Switzerland, believes that bronchocele is caused by certain *waters*, which issue from the hollows of rocks, trickle along crevices of the mountains, or rise from the bowels of the earth. And in support of that opinion he refers to some fountains in his own neighbourhood, the drinking of the water of which will produce, or augment, goitrous swellings, in eight or ten days. Such of the inhabitants as avoid these waters are free, he says, from goitre and cretinism. In Captain Franklin’s narrative of his expedition to the shores of the Polar sea, there is the following statement, made by his fellow traveller, Dr. Richardson:—“Bronchocele or goitre is a common disorder at Edmonton. I examined several of the inhabitants afflicted with it, and endeavored to obtain every information on the subject from the most authentic sources. The following facts may be depended upon:—The disorder attacks those only who drink from the *water* of the (Saskatchewan) river. It is indeed, in its worst state, confined almost entirely to the half-bred women and children who reside constantly at the fort, and make use of river water, drawn, in winter, through a hole made in the ice. The men, from being often from home on their journeys through the plain, where their drink is *melted snow*, are less affected: and if any of them exhibit during the winter some incipient symptoms

of the complaint, the annual summer voyage to the sea-coast generally effects a cure. The natives, who confine themselves to *snow water* in the winter, and drink of the small rivulets which flow through the plains in the summer, are exempt from attacks of this disease. A residence of a single year at Edmonton is sufficient to render a family bronchoceleous. Many of the goitres acquire great size. Burnt sponge has been tried, and found to remove the disease: but an exposure to the same cause immediately reproduces it. A great portion of the children of the women who have goitres are born idiots, with large heads, and the other distinguishing marks of cretins. I could not learn whether it was necessary that both parents should have goitres to produce cretin children."

We are able even to go a step further, and to announce a probable conjecture as to the specific quality of the suspected water. Bronchocele is very prevalent in Nottingham and its neighbourhood; and the vulgar there ascribe it (so Dr. Manson informs us) to the *hardness* of the water. You know that the rough practical distinction between soft and hard water is that the former *dissolves* soap, while the latter *decomposes* it. The hardness is generally occasioned by the presence either of *sulphate of lime*, or of *carbonate of lime*. In the one case the remedy is to mix the carbonate of an alkali with the water; in the other you simply boil it. Now the well water in and about Nottingham is more or less hard, and unfit for the purpose of washing. Dr. Coindet, of Geneva, declares that the use of hard or pump water in the lower streets of that town brings on the goitre very speedily. At Cluses, on the Arve, numerous cretins and goitrous persons are seen in the streets: lofty cliffs of limestone tower over the town, and through its caverns copious streams of water find a passage. The soil in the neighbourhood of Edmonton was found by Dr. Richardson to be calcareous, and to contain numerous fragments of magnesian limestone. In a *Treatise on English Bronchocele*, very recently published, Dr. Inglis states his belief that the presence of magnesian limestone always implies the co-existence of the disease. "Take (he says) that ridge of magnesian limestone running from north to south through the centre of Yorkshire, and marging the shires of Derby and Nottingham. All along that line we have goitre to a very great extent; whereas, on our diverging to either side, the disease is found to diminish."

These scattered indications that the hurtful quality of the water is somehow derived from its contact with limestone rocks, receive a powerful corroboration from the result of Mr. M'Clelland's minute and valuable inquiries, which were carried on in the province of Kemaon, south of the Himalayan mountains. I have not been able to obtain his book; what I am about to tell you I take from a full and instructive notice of it in the fifteenth number of the *British and Foreign Medical Review*. Mr. M'Clelland finding goitre very abundant (as I mentioned before) in one great section of a district, and almost entirely absent from another section, set himself to find out in what other particulars these sections were distinguished from each other. He ascertained that they completely agreed "in external aspect, altitude, and climatology," but differed remarkably "in their geognostic relations; and this distinction was even traced down to the very villages in which the disease is found, with such perfect nicety, as to enable one almost to predict *à priori*, on examining the rocks of a neighbourhood, whether the inhabitants are affected with goitre or not."

It would be impossible for me to give you even an abstract of Mr. M'Clelland's numerous observations; but I select one or two striking instances in favour of his opinion that the endemic prevalence of goitre is connected with the use of water impregnated with calcareous salts.

One extremity of the long village Deota, which occupies half a mile of the foot of Durge mountain, is inhabited by Brahmins; the other by Rajpoots and Domes. Of the first caste there are about twenty persons, all of whom are free from goitre. There are forty of the second, and two-thirds are affected, more or less. Of the third caste, forty-six in number, nearly the whole are goitrous. "To what cause can we ascribe the immunity of one caste of the inhabitants of this village, and the almost universal affection of the other two castes? They are all alike well-fed, and have little toil: their land producing the requisites of life almost without labour. Difference of caste does not here imply a difference of pecuniary circumstances, and consequently of the comforts of life. In these respects the three castes in this village are on perfect equality. Nor will hereditary predisposition acquired by intermarriages be sufficient

to explain the interesting fact: for the affected parties are confined to the Rajpoots and Domes, who cannot intermarry, while the Brahmins and Rajpoots may. The village is raised about one hundred feet above the level of the valley; and the mountain, at the foot of which it is situated, rises with a gentle slope, and is not in this vicinity at all rugged. It is chiefly composed of transition limestone, and the village is erected on a conglomerated rock, composed of calcareous tuff, inclosing fragments of other rocks. There is a spring in the valley, about one hundred yards from the village, bearing on its first appearance the character of a mineral spring. The water bursts forth with strong ebullition, in the quantity of at least forty gallons in a minute, and agglutinates the sand and gravel by which it is surrounded, by the deposition of calcareous tuff. The temperature and quantity of the water is the same at all seasons. The former inhabitants of the village, aware perhaps of the noxious effects of this spring, had an aqueduct formed, by which water is conveyed into the Brahmin portion of the village from a distant source. The aqueduct having been suffered to get out of repair, the quantity of water it transmits is reserved exclusively for the Brahmins; except during the rainy season, when, the water being plentiful, the Rajpoots also use that of the aqueduct; but the Domes have no alternative at any season but to use the water from the spring."

The valley of Baribice is elevated 4000 feet above the sea. Its eastern extremity is composed of *clayslate*, and in five villages, containing 152 inhabitants, there is not one goitre. The other extremity of the valley is partly composed of limestone; and of 192 inhabitants, distributed in six villages, 70 are affected with goitre: but Ducy-gong, one of these villages, supplied with water from clayslate, has not a single case of the disease; while Agar, only half a mile distant, and containing 50 inhabitants, has no less than 40 cases; and of that number 20 are cretins. They use the water which issues from an old copper mine in limestone, and which contains carbonate of lime, and of soda, but no sulphate.

Mr. McClelland affirms that in the course of his personal inquiries, which extended over 1000 square miles, and which were prosecuted without regard to any theory, no instance occurred in which goitre prevailed to any extent where the villages were not situated on, or close to, limestone rocks.

[In the United States the disease is prevalent in many of the valleys situated in the mountainous regions of New Hampshire, Vermont, New York, Pennsylvania and Virginia; many of these regions abound in limestone rocks. — C.]

Cretinism has a close, but an ill-understood, connexion with goitre. Wherever cretinism is endemic, bronchocele never fails to be abundant. But bronchocele may prevail in a place where there are no cretins. With but few exceptions, cretins are goitrous; whereas many of those who have bronchocele are not affected with cretinism. The two disorders either spring from the same cause, requiring for their joint production that this cause should be in active operation; or, if they have separate causes, these frequently co-exist and act in combination. It is said, I know not with what accuracy, that when both parents are goitrous for two generations in succession, the offspring, being in the third generation, are sure to be cretins. Certainly cretinism is most common where bronchocele is most common, and especially in mountainous places. It occurs in the Pyrenees as well as in the Alps, in the mountains of Syria, in the hilly parts of China, and in the Himalayan regions. Yet cretinism is confined within much more limited bounds than goitre. Saussure, Foderé, and Dr. Reeve, agree entirely as to the circumstances under which cretinism appears to be most commonly engendered in Switzerland. They say that the disease is usually met with in the valleys which are nearly surrounded by high and steep rocks, where there is but little circulation of air, and where the inhabitants are exposed to the direct rays of the sun, and to the reflexion of them from the rocks: and also to effluvia from marshes. It is in the filthy habitations built in these close, hot, and humid situations, that cretinism abounds most. The children that are taken away from the low valleys, and carried up, when young, into the high grounds, escape the disease; or even get the better of it if removed soon enough. And the amendment is said to be perceptible even in a very few days. These facts have led many persons to conclude that cretinism, if not bronchocele, depends on some condition of the *air*. It

appears to me probable that the exciting cause of both is the same, and that the local circumstances just now mentioned operate as predisposing causes only. Cretinism, as well as goitre, was observed, by Ramond, in the "open, well-watered, and well-ventilated valleys of the Pyrenees."

There are some difficulties opposed to the implicit reception of the opinions formed by Mr. M'Clelland and by others, respecting the origin of these diseases. And the facts upon which those opinions are grounded are not without apparent exceptions. Moreover, the actual substance which exercises or confers the noxious power, has yet to be ascertained. This etiological problem, so full of interest, is not solved. One step more, and probably one step only, remains to be taken. We look to the medical geologist for its complete solution; and I trust that, now, we have not long to look. The deleterious agent has been traced, with tolerable certainty, to water: and hence to some element of the soil washed by that water. And if what at present is probable only, shall hereafter be proved, — namely, that the hidden cause of goitre and of cretinism lurks in some chemical quality of man's natural beverage — it can scarcely be doubted that chemistry will be found ready to supply a simple and effectual corrective of the evil. This hope it is which makes it so important that medical men should be accurately possessed of the present state and bearings of the question; and prepared to take advantage of every opportunity that may arise for its practical determination. For surely it would be a noble achievement of our art, and a signal blessing provided for hundreds of human beings yet unborn, thus to prevent the deformity, the discomfort, and the sometimes danger, of bronchocele; and to forbid, in its very source and fountain, the more hideous and loathsome disfigurement, of mind as well as of body, that distinguishes the wretched cretin.

I ought, perhaps, to tell you, that other causes, many of them very vague and unsatisfactory, have, at different times, been assigned. Thus Valentin supposes the disease to be more common in women than in men, simply because women more frequently have the neck uncovered. It has been affirmed that young females who have taken the veil in catholic countries have lost their goitres in consequence of the change then made in their costume; and a medical man in Guatemala asserts that the same infirmity has sensibly diminished among the men in that part of the world, since cravats became fashionable there. These views of the matter accord with some whimsical notions entertained by the late Sir Anthony Carlisle. He held that the chief purpose of the thyreoid gland was to protect the delicate organ of the voice from the injurious influence of cold; and he consequently regarded the tumour of bronchocele as being rather of a sanative than of a morbid nature. He looked upon it as an additional greatcoat thrown over the important instrument of speech, in circumstances of extraordinary need. His theory agrees with some of the facts on which other theories, not perhaps more plausible, have been founded. Thus although snow water may not be, indeed I may say certainly is not, the cause of bronchocele from any peculiar quality that belongs to it, yet the disorder is confessedly frequent in many places where snow water is habitually drunk; *i. e.*, as Sir Anthony would have reasoned, where *very cold* water is frequently drunk; the swelling being a provision of nature to obviate the hurtful effects which might else be produced on the larynx by these cold potations. The summer change for the better, described by Dr. Richardson, consists also with the same theory: which would suggest the covering an incipient bronchocele continually with warm clothing, such as flannel, to check its growth, to obviate its necessity, and so gradually to occasion its subsidence. And this plan is mentioned, I see, by Bouillaud, among the curative indications.

I mention these theories, not because I have any faith in them myself, but as being curious specimens of the manner in which the human mind strives to account for obscure phenomena; and as showing how readily facts may be culled and pressed into the service of very slender and infirm hypotheses.

One very important point — preliminary to all rational treatment of bronchocele or of cretinism — is obvious; the removal of the sufferer, whenever that can be done, from the infested locality. This was the first care of that great friend of the unhappy race of cretins, the benevolent Dr. Guggenbühl, who has founded on the Abendberg in the Bernese Alps, at an altitude considerably higher than that below which these kindred disorders are generally met with, a hospital for the reception of cretin children. To their cause and comfort Dr. Guggenbühl has generously devoted his whole mind

The success of this philanthropic experiment, the beneficial influence of pure air, and of judicious medical and moral discipline upon these poor patients, has been signal and gratifying. Of the whole number of cretins admitted during a period of twelve years, one third were perfectly restored to health and reason, while the rest improved much both in body and in mind.

This most useful and praiseworthy institution has proved the parent and model of the Asylums for Idiots which now exist, and are happily multiplying, in England and elsewhere.

When removal to a more salubrious place has been accomplished, or when the disorder occurs sporadically, as it sometimes does, we may administer drugs with better hope and advantage. Now a great variety of empirical remedies have been recommended for the cure of bronchocele, concerning most of which it would be a waste of time for me to speak at all. The remedy which, as an internal medicine, has of late years superseded all others, and acquired the reputation of a specific against goitre, is iodine: and it certainly has a remarkable influence over the genuine unmixed form of the disease, whether endemic or sporadic—the hypertrophied gland; yet it does not, perhaps, merit the title of specific in the same absolute sense as that in which we say that bark is a specific for the ague, or sulphur for the itch. Dr. Coindet, of Geneva, was the first who made this remedy extensively known. Dr. Straub, however, of Hofwyl, has laid claim to the priority of its use. Probably that happened in this matter, which has often happened in others, viz., that the state of knowledge was ripe for such a discovery, and it was made by each of these physicians independently of the other. Dr. Coindet's name, however, has been inseparably connected with the application of iodine to the cure of bronchocele, in a work which, if he had no other claim to be remembered, would immortalize his memory;—I mean Sir J. Herschel's profound and beautiful *Discourse on the Study of Natural Philosophy*;—a book which I should advise those among you who have not already read it, to make themselves masters of as soon as they have leisure to do so. He thus strikingly illustrates an observation of his own, that mighty benefits often accrue to science from the casual experience of even unscientific or illiterate persons. “A soap manufacturer remarks that the residuum of his ley, when exhausted of the alkali for which he employs it, produces a corrosion of the copper boiler, for which he cannot account. He puts it into the hands of a scientific chemist for analysis: and the result is the discovery of one of the most singular and important chemical elements—iodine. The properties of this being studied, are found to occur most appositely in illustration and support of a variety of new, curious, and instructive views then gaining ground in chemistry; and thus exercise a marked influence over the whole body of that science. Curiosity is excited; the origin of the new substance is traced to the sea-plants from whose ashes the principal ingredient of soap is obtained; and ultimately to the sea-water itself. It is thence hunted through nature, discovered in salt-mines and springs, and pursued into all bodies which have a marine origin: among the rest into sponge. A medical practitioner, Dr. Coindet, of Geneva, then calls to mind a reputed remedy for the cure of one of the most grievous and unsightly disorders to which the human species is subject—the goitre—which infests the inhabitants of mountainous districts to an extent that, in this favoured land, we have happily no experience of, and which was said to have been originally cured by the ashes of burnt sponge. Led by this indication, he tries the effect of iodine on that complaint, and the result establishes the extraordinary fact that this singular substance, taken as a medicine, acts with the utmost promptitude and energy on goitre, dissipating the largest and most inveterate in a short time, and acting (of course, like all medicines, even the most approved, with occasional failures) as a specific, or natural antagonist, against that odious deformity.”

It is curious enough, and marks the accuracy with which the effects of remedies may be observed, that not only had burnt sponge been found efficacious in removing bronchocele, but the bladderwrack also, the fucus vesiculosus, the plant that, with others of the same family, yields the soda wherewith the iodine was found combined. Dr. Gairdner, who was the first I believe in this country to write on the effects of iodine, accounts for the frequent failure of even large doses of the burnt sponge, by showing that it was much adulterated with charcoal. Dr. Manson, of Nottingham, has published the following tabular statement of the results of the employment of

iodine by himself. He had treated one hundred and sixteen patients, of whom fifteen were men, and the rest women. Of the fifteen men, ten were cured, three were improving and under treatment at the time of his publication, one was dismissed for irregular attendance, and one was much relieved. Of the one hundred and one women, sixty-six were cured, nine much relieved, two received no benefit, ten were discharged for irregular attendance, and fourteen were improving under treatment. Of the whole one hundred and sixteen, therefore, there were seventy-six positive cures, or two-thirds of the entire number; and only two positive failures. Dr. Coindet was successful in about the same proportion of cases. This is strong evidence of the power of the remedy. Dr. Manson found that in some, but not in all individuals, after the preparations of iodine had been given internally for a certain time, they were apt to occasion headache, giddiness, sickness of stomach, with some degree of languor, and inaptitude for exertion. His plan in such cases was to suspend the use of the medicine, or to reduce the dose. The following effects of the iodine are stated by Dr. Coindet as having occurred in his practice:—Acceleration of the pulse, palpitation, dry cough, watchfulness, marasmus, and prostration of strength. Sometimes swelling of the legs supervened, tremors, painful hardness of the bronchocele, diminution of the breasts, or a remarkable increase of appetite; and he adds that in almost all the instances which he had observed, a very rapid diminution, or a disappearance more or less complete, took place, during these symptoms, of even hard, bulky, and old bronchoceles. His doses varied from somewhat less than a grain to somewhat more than a grain and a half. This was certainly, as Dr. Manson has suggested, too large a quantity of this powerful drug for the generality of patients. The management of the remedy is now better understood. I have never seen any of the unpleasant consequences enumerated by Dr. Coindet. These practitioners gave the iodine in the form of a tincture. But that is a bad mode of exhibiting it: for the tincture is decomposed in any aqueous menstruum, and the iodine thrown down to the bottom. Under such circumstances the patient may at one time get no iodine at all, and at another time a dangerous dose: for you are aware that iodine is an active irritant poison. But if you mix iodine with iodide of potassium, it is then *held* in solution: and this is the form in which it is now commonly given. In the last edition of the Pharmacopœia there is a *Liquor Potassii Iodidi compositus*, made by dissolving five grains of iodine, and ten of the iodide of potassium, in a pint of distilled water. This will doubtless, in future, be much prescribed. It is a dilute preparation. There is one grain of iodine in four ounces of the liquor. An ounce would therefore be a very *safe* dose, but you may begin with a couple of drachms, and increase the quantity, if need be, gradually: for not only Dr. Manson, but Dr. Copland also, who states that the remedy has been very successful in his hands, advocates *small*, unirritating, what are sometimes called *alterative* doses. Simple friction is said to have had the effect of diminishing the tumour; and friction with some ointment or liniment containing iodine should be employed whenever the internal exhibition of the remedy is contra-indicated; or *in addition* to its internal use. There is an authorized form for that purpose also in the Pharmacopœia—the *Unguentum Iodini Compositum*. A small portion of this may be rubbed upon the tumour night and morning. Or the tumour may be painted, night and morning, with the compound tincture of iodine, pauses being made whenever the soreness of the skin may require them. The tincture stains the surface of a deep orange colour, so that during this process the throat must be kept covered.

Among anæmic young women, more or less fulness of the forepart of the neck, from slight enlargement of the thyreoid gland, is of very frequent occurrence. Sometimes the eyeballs are at the same time unduly prominent. In these cases, if you cure the anæmia, you will commonly cure the bronchocele also, or greatly diminish it: and I would recommend you to address your first remedies to the general rather than to the local ailment. If, when the blood has been replenished and the health improved by the salts of iron, and by other suitable means, the gland should still remain tumid, you may then resort, with caution, to the use of iodine. I need not add that besides great care in watching for the specific ill effects of the iodine upon the system, no less care is to be taken in correcting the state of any other function which may be faulty, and in improving the general health.

Such is the best *medical* treatment of bronchocele; and such are the plans which

you will always do well to make watchful trial of in the first instance. And with respect to surgical treatment I may observe, that so long as the disease is merely a deformity, so long as it does not interfere with any of the important functions of the body, nor produce serious discomfort — does not distress the respiration by pressing upon the trachea, nor impede deglutition by pressing upon the œsophagus, nor derange to any great degree the circulation through the head by pressing upon the great blood-vessels of the neck, nor grievously encumber the patient by its weight — we should not be justified (in my opinion) in performing or advising any surgical operation for the removal or the diminution of the tumour. Of such operations the three principal are, extirpation of the whole gland; the passing a seton through the tumour, and so exciting suppuration in it, whereby its substance is broken down and destroyed; and, tying the arteries which supply it with blood. The first of these methods, extirpation, has been performed when the wen was small; but few patients, under such circumstances, would consent to the operation; and few surgeons would now-a-days, I presume, advise or undertake it: and in cases where it might seem more expedient, that is, where the swelling is large, and suffocation is threatened by its pressure on the trachea, the execution of this measure would be exceedingly difficult and dangerous; for the arteries are so much dilated in these cases that perilous hæmorrhage might be expected from their division, especially when their close vicinity to the carotids is considered. Such large goitres come to involve also, by their lateral extension, very important nerves there situate. In one case where excision was attempted, the hæmorrhage was so alarming that the surgeon was obliged to desist in the middle of his task; and the patient actually died of hæmorrhage a few days afterwards. I believe that this operation may be considered as wholly abandoned by surgeons in the present day.

The introduction of a seton into the tumour has been more successful. This practice was revived about thirty years ago by Dr. Quadri, of Naples; who supposed, indeed, that he had been the first to devise it. You will find an account of his mode of proceeding in the tenth volume of the *Medico-Chirurgical Transactions*, by Dr. Somerville. The seton is passed through the substance of the gland, and retained there a considerable time; the chief caution necessary is to avoid the enlarged blood-vessels. Dr. Quadri affirms that the larger trunks of the thyreoid arteries will not be endangered, unless the seton needle be passed so deeply as almost to touch the thyreoid cartilage; and that hæmorrhage from any of the branches of those arteries that may be wounded when the seton is inserted more superficially, will not be attended with hazard. This plan was followed in one case by ulceration and sloughing, and the patient died. An example of the success of this treatment occurred in the practice of Mr. James, of Exeter; the tumour was almost entirely removed, and the patient was for some time in London for the purpose of showing himself to the medical men here. But he also had passed through a good deal of suffering and of peril.

The expedient of tying one or more of the thyreoidæal arteries, and so *starving* the tumour, has been attended with varied success. It has been done on the continent; and several times in this country: there is a case of it by Mr. Coates, of Salisbury, in the same volume of the *Medico-Chirurgical Transactions*. His patient was much relieved for a time, and supposed herself cured. But the tumour gradually returned, and caused her death by suffocation. Sir B. Brodie has also performed a similar operation: and I have myself seen one very satisfactory instance of the same thing, the operation having been done by the late Mr. Earle in St. Bartholomew's Hospital. The patient was a young woman with an immense bronchocele, which for some time previously had obstructed her breathing, and of which the effect on the trachea seemed to be every day increasing. There was loud wheezing, and great difficulty of respiration, and cough, and extreme emaciation: and it was plain that the girl must soon die strangled if nothing were done for her relief. One of the arteries, I think the superior thyreoidæal, was tied, without much difficulty, on one side. After the operation, the tumour on that side shrunk considerably; the distress of breathing was removed; and the patient presently recovered so much of her former strength, and flesh, and comfort, that she was unwilling to have anything more attempted, and left the hospital. After some time, however, she came back again of her own accord, and requested that the artery on the other side might be tied also. This was accordingly

done, and a further reduction of the tumour took place. The shrinking, however, if I remember rightly, was not so striking or complete after the second operation as after the first: but the patient was certainly rescued, upon the whole, from imminent death, and put into a condition of ease and at least temporary safety; the tumour that remained constituting merely a deformity. I never heard of her afterwards. In a case very similar to this, related by Mr. Crawford in the *Cyclopædia of Practical Medicine*, Mr. Wickham, of Winchester, tied one of the arteries, with much immediate, but no ultimate benefit. The goitre gradually diminished for about six weeks, and then (in consequence, I presume, of the establishment of a collateral arterial circulation) it as gradually enlarged again till it was as big as ever.

Of these surgical expedients there is not one, of which the *average* results have been sufficiently prosperous to warrant its repetition, except in cases where life is put in jeopardy or made miserable by the swelling; and where other methods, and particularly the treatment by iodine, have been tried, and have failed. One exception, perhaps, I should here make. The tumour sometimes evidently contains a quantity of fluid, either in one of its large cells, or in a distinct cyst. Now the cell or cyst, may in such cases be punctured, and the contained fluid let out, without much risk. This was done in one instance by my colleague, Mr. Arnott. He kept the orifice open; and the cyst shrank, and was at last obliterated, and the woman was much gratified by this diminution of her load. I have since met with a precisely similar case.

LECTURE XLIV.

Cynanche Parotidæa. Spontaneous Salivation. Aphthæ. Cynanche Tonsillaris.

THE Greek writers on medicine apply the terms *συναγχή* and *κυναγχή* to inflammatory affections occurring about the *throat*, and more or less interfering with the functions of respiration and deglutition: and the Latins employ the word *angina* in nearly the same sense. Cullen, in his *Nosology*, has made a *genus* of *Cynanche*, although the diseases which he has included in that genus have but little connexion, except that the parts they occupy lie *near* to each other. Some of them indeed have their seat in different, though almost contiguous, portions of the *same membrane*; and are apt, sometimes, on that account, to pass one into the other. In general they are allied rather by proximity of situation, than by community of symptoms.

I mention these things, because there being a great disposition in the present day to *re-name* diseases, and to affect a more precise and scientific nomenclature than sufficed for Cullen, if I adopted the more modern appellations without adverting to the old ones, which have been current so many years, you might experience some difficulty in your reading, in determining what disease was intended, when it was merely *named*. For my own part, I think there is much inconvenience in altering the established nomenclature; and especially in changing such arbitrary terms as, though they may not be *scientific*, are yet *definite*, and convey no erroneous notions respecting the nature of the disorder. I shall give you, however, in most cases, both the old and the new denominations.

Now one of the maladies in Dr. Cullen's genus *Cynanche* — though not the first in the order he follows — is *cynanche parotidæa*. It is an inflammatory affection of the salivary glands, and of the parotid gland in particular. Accordingly it is called *parotitis* now-a-days. It is not, however, mere inflammation of the parotid, arising from any cause whatever; and therefore parotitis, unless some epithet be added, is less exact than *cynanche parotidæa*. The vulgar have given it just as good an arbitrary name as the learned; and they call it in this country the *mumps*. With the Scotch it is, I believe, the *branks*.

This disorder need not detain us long. The parotid swells; tumefaction takes place beneath the ear; and if the submaxillary and sublingual glands are not impli-

cated in the outset, they soon participate in the tumefaction, in most instances; so that the swelling extends from beneath the ear along the neck, towards the chin, and the swelled parts are hot and painful, and very tender when touched. The aspect of the patient becomes curiously deformed. Sometimes one side only is affected; sometimes both sides at once; but most commonly of all, first one side and then the other. These local symptoms are attended with slight fever. But the only function that is materially affected is the motion of the lower jaw, which is impeded by the swelling. The inflammatory condition almost always terminates after a few days, in resolution, under the use of the antiphlogistic regimen, and the application of external warmth. The disease reaches its height in about four days, and then begins to decline; and its whole duration may be computed, on an average, at eight or ten days.

This complaint often prevails epidemically: when it affects one person in a family, or school, it usually affects several others, simultaneously or in succession. It chiefly attacks children and young persons. There can be no doubt that it spreads by contagion; and it seldom happens that the same person is twice affected by the mumps. These are remarkable circumstances, and give the malady a peculiar and specific character. I do not dwell upon them now, because they belong also to a very interesting *group* of diseases, which will require to be particularly considered hereafter.

Another curious circumstance connected with the disease, and one which has some bearing upon its treatment, is that, in many cases, upon the subsidence of the swelling of the neck and throat, and particularly when it subsides *quickly*, the *testicles*, in the male sex, become swollen and tender, and the *mammæ* in the female. It is said, but I do not know whether the observation be constantly true, that the testicle, or the breast, of *the same side* with the inflamed parotid, suffers. Sometimes the testicle wastes away after the swelling recedes; a circumstance which is known occasionally to happen when inflammation of that part arises from other causes. This, however, is not usual. In general the inflammation subsides and ceases in the one gland as it does in the other; the swelling is neither very painful nor long continued. But sometimes a more serious transference takes place, from the testicle to the brain: this I have never witnessed; but then, to say the truth, I have not often been called upon to treat the mumps, and my personal experience of it is limited. I find it stated that the metastasis to the testicle is considered as rather a fortunate circumstance, because it serves as a sort of protection against metastasis to the brain; but I suspect this to be a mistake. Inflammation of the brain, or of its membranes, has sometimes occurred on the disappearance of the *parotid* swelling; but it has much oftener supervened, I believe, upon the retrocession of the inflammation from the *testicle* or *mamma*. It is said also that the inflammation sometimes returns from the testicle to the parotid, and back again; oscillating thus two or three times between the two glands. Fortunately, the metastasis to the brain is much more rare than that to the testicle.

The treatment of the mumps is simple. It consists in the observance of the antiphlogistic regimen; mild diaphoretics; laxative medicines if the head ache, or the bowels are confined; and warm fomentations, or dry warm flannel, to the neck and throat. The tendency observed in this complaint to a change of place—to metastasis to more important organs—forbids us from using very active measures to *check* or *subdue* the inflammation. Nor are such measures necessary. We are not to bleed, nor violently to purge such patients, nor to apply cold to reduce the swelling. Luckily hot applications are not only the most safe and proper, but the most grateful also to the feelings of the patient. If suppuration should ensue—which is unusual and unlikely, but which sometimes does occur from extension of the inflammation to the neighbouring areolar tissue—poultices must be substituted for the fomentation. Warm applications, and rest in the horizontal posture, are to be recommended when the inflammation leaves the salivary glands, and attacks the testicles; or if the patient will *not*, or cannot, lie up, the testicle must be supported by a suspensory bandage—a bag-truss. If the inflammation of the testicle or mamma be very violent, we must apply leeches, and afterwards poultices; but this will not often be required, or advisable. Finally, if the inflammation should fly to the brain, we must lay aside our previous caution, and treat the disease in that active manner which the inflammation of so important a part of the body demands. No worse metastasis can occur on the cessation of the phrenitis. I have fully spoken heretofore of the treatment to be pursued in that disease, and I have nothing to add respecting it now, except that it may

be right, as an auxiliary expedient, to try to reproduce the inflammation in the parotid, or testicle, or mamma, by irritating applications—mustard poultices, for example—in the hope of thus producing what is called *revulsion*, and of diverting the disease from the brain to the part which it previously occupied.

You know that there is another specific form of *parotitis*, which is apt to be induced by mercury. Of this I have already spoken. When it is severe, it may be treated by leeches, without any dread of such metastasis as occurs in the mumps. It is usually, though not always, accompanied by a profuse discharge of the secretion proper to the glands affected; and it is attended also by sponginess and swelling of the gums.

I presume that when inflammation of these salivary glands is *not* attended with ptyalism, the parenchyma of the gland, or the areolar tissue which enters into its composition, is principally affected; and that when there *is* much salivation, the membrane lining the secretory and excretory ducts is implicated. We see the same distinctions in other analogous organs.

Profuse ptyalism sometimes occurs without any obvious cause; and is then said to be *idiopathic*, or spontaneous: and this is a circumstance which it concerns you to be aware of, both as practitioners, and as medical jurists. The same *tenderness and swelling* of the salivary glands, the same copious *secretion and excretion of saliva*, nay, even the same *fœtor*, or a smell which can scarcely be distinguished from it—the same collection of symptoms which is familiar to you as indicating the specific action of mercury upon the human system—will arise sometimes (but very rarely) when not a particle of mercury has been administered. Several other substances are well known to have the occasional effect of producing an increased, and even a profuse flow of saliva: preparations, for example, of gold, of copper, of antimony, and of arsenic. The castor oil is said to have sometimes the same consequence. Digitalis certainly has; and the iodide of potassium; and sometimes, I believe, opium. Now and then ptyalism is met with as a symptom, among others, of pregnancy. Occasionally it results from some local irritation within the mouth; from a decayed or misplaced tooth. But what I principally wish to call your attention to is the fact that *salivation* may occur as an *idiopathic complaint*. In the twenty-sixth volume of the *London Medical and Physical Journal*, there is an instance of it described by Mr. Davies, in which two or three pints of saliva were discharged daily for some time. This flux at length ceased under the use of laxative medicines. In the second volume of the *Transactions of the College of Physicians* is an extraordinary example of the same thing, related by Mr. Power. A young lady, of sixteen, spat from sixteen to fourteen ounces of saliva daily, for upwards of two years. Mr. Power believed that the ptyalism in this case was originally excited by wool, which he found, in a fœtid state, in her ears. In the *Revue Médicale* there is an account given of a patient who was cured of a spontaneous ptyalism after spitting nine pints daily for nine years and a half. You may see another instance, as related by Dr. Prout, in the old series of the *Annals of Philosophy*. Dr. Pereira states that he has seen a dozen such cases; and he describes one which was fatal, not from the ptyalism however, but from sloughing of the cheek: and this is no uncommon circumstance. In certain cases of idiopathic inflammation and ulceration of the gums or cheeks, from some constitutional unsoundness, there may be extensive sloughing, ptyalism, and a very offensive odour, much resembling that which mercury produces. I have met with one example only of well-marked spontaneous ptyalism; and some of its circumstances were so peculiar, that they may be worth relating. I was taken out to Bayswater, by a medical friend, in the beginning of the year 1833, to see a little girl ten years old, who had been in a state of salivation from the 5th of November in the preceding year. Up to that time she had been a healthy, lively child, with nothing very remarkable about her, except that she was habitually subject to profuse perspirations, which had a very acid smell: so that the washerwoman was always aware which were her clothes, when she came to wash them, by this *smell*. She then suddenly became indisposed, had a little headache, and began to spit a good deal. This was noticed by her mother, and pointed out to her medical attendant, before *any* medicine was given her; and mercury, on that account, was religiously withheld. But in spite of all treatment the ptyalism went on increasing. When I saw her she was spitting three pints of saliva in twelve hours: transparent, rather dark-coloured, and with a small quantity of foam on its

surface. There was nothing amiss with her teeth, or her gums, and no foetor of the breath. She was greatly emaciated, and resembled, in some respects, a person worn down by diabetes. From the very commencement of the spitting, the acid perspiration had ceased, and even the vapour bath failed to make her sweat. A great variety of remedies were tried under Dr. Nevinson's superintendence, but without the least good effect. At last came the visitation of the influenza, in April of that year. The girl became severely affected by that disorder; and thereupon the salivation disappeared, and has not returned. I heard to-day that she is in excellent health.

Should you meet with cases of the same kind, you will search for some cause of irritation in the neighbourhood of the salivary glands, and especially in the state of the teeth and gums; and finding none, you will seek further for the cause of the salivation in some deviation from the natural condition of one or other of the principal functions of the system: and you will regulate your treatment accordingly. I do not know of any specific plan of cure to be recommended: but it is certainly of importance that you should be acquainted with the fact, that ptyalism sometimes exists as a separate and independent malady. Astringent washes are found, sometimes, of service; a solution of alum, or the infusion of catechu, or a few drops of creasote suspended, by the help of mucilage, in water.

Before I proceed to the diseases pertaining to the interior of the fauces and throat, let me take this opportunity of saying a few words in respect to *aphthæ*. They form the characteristic symptom of an especial disease of infancy: and they are apt to occur in the course of other diseases in adult age; and are then of some importance as guides in forming our prognosis, and even in determining our plan of treatment.

Aphthæ consist in small, irregular, but usually roundish white specks, or patches, scattered over the surface of the tongue, and the lining membrane of the cavity of the mouth and fauces; the angles of the lips, the cheeks, the palate, the pendulous velum, the tonsils, the pharynx. They look like little drops of tallow, or morsels of curd, sprinkled over those parts; they project a little above the surrounding surface; and, in fact, they are mostly formed by elevated portions of the mucous epidermis, covering a small quantity of a serous or gelatinous fluid, which separates the epidermis from the subjacent corium. These portions of the epidermis detach themselves, and fall off; leaving behind them a reddish raw-looking surface, or sometimes a foul and ash-coloured spot: and successive crops of these aphthæ are apt to be formed.

Now children in arms who exhibit these aphthæ, are said to have the *thrush*. This occurs at an early age; seldom, or never, I believe, after the period of lactation is over. The spots occasion some inconvenience in themselves—the mouth is rendered hot and tender by them. The child may be eager enough to take the breast, but is observed to do so with pain and wailing whenever the mouth is applied to the nipple, and attempts to suck or to swallow are made.

But these aphthæ, thus occurring in infants, are attended with other symptoms of disorder; such as drowsiness, sickness, diarrhœa, and some feverishness. And a general notion prevails, I believe, that the same apthous condition which is visible in the tongue and mouth, pervades, in such cases, the whole of the alimentary canal. But this must be a mistake. That *some* morbid condition exists throughout that tract is highly probable, but true aphthæ can only form on those mucous surfaces which are provided with a continuous *epidermis*. This erroneous notion has been strengthened, perhaps, by the observation of apthous spots on the pharynx and œsophagus. The complaint sometimes appears to be the result of improper diet, in children brought up by hand; or of milk of a bad quality, from an unhealthy or intemperate nurse. It generally lasts eight or ten days. It is not attended with much danger, except in certain cases, when the surface is left brown or bluish after the loosening and separation of the crusts. In such cases, the local affection is apt to run into a bad kind of gangrenous ulceration, and the discharges from the bowels become slimy and shreddy.

In almost all instances of the thrush in children, there is acidity of stomach present. Care, of course, is to be taken to discover and to correct any error of diet; and any unwholesomeness in the quality of the food. And *antacids* are to be administered. I know of no form of medicine better adapted to remedy the diarrhœa of infants, than the *Pulvis Sodæ cum Hydrargyro* of our hospital Pharmacopœia; composed of two

parts of the Pulvis Cretæ Compositus, two parts of the dried Carbonate of Soda, and one part of Hydrargyrum cum Cretâ. From three to five grains of this powder may be given thrice daily: and for the local affection of the tongue and mouth, the *mel boacis* is a capital application. It may be painted on the aphthous parts with a camel's-hair pencil.

Aphthæ occurring in adults, in the course of other diseases, are often the harbingers of dissolution. They denote considerable *debility*; and they point out the propriety of sustaining the patient's strength by bark, wine, and nourishing food. It is remarkable how treatment of this kind will sometimes *tell*. I had a patient last summer who lived for some months, and in tolerable comfort, after a second attack of apoplexy. Every now and then he would have a crop of aphthæ appear, which was always an admonition to us that he not only would bear, but that he required some tonic. A more generous diet, with bark, would dissipate them in a day or two.

Borax is an excellent application for aphthæ, whether they occur in adults or in infants. I have known it afford great comfort to patients who were in the last stage of phthisis, and to whom the aphthous state of the mouth was a source of considerable distress. Equal parts of Mel Boracis, and of Syrup of Poppies, make a good form. Or an agreeable as well as useful gargle may be made by mixing two drachms of Borax with an ounce of Mel Rosæ, three ounces of Decoction of Quince Seeds, and six ounces of water.

Aphthæ seem sometimes to depend upon mere derangement of the stomach. A nobleman who is well known as a *bon vivant*, can never eat shell-fish (so I am told by his physician) without finding, within two hours, that his mouth is full of aphthæ. Even lobster-sauce will serve him thus. I look upon this as a sort of internal urticaria.

[The disease of the mouth occurring in infants during the period of suckling, in which the tongue, and the inner surface of the lips and cheeks are covered, to a greater or less extent, with minute portions of a white matter resembling curd — and which constitutes the *muguet* of the French writers, the *thrush* or *children's sore mouth of nurses*, and the *aphthæ lactumina* and *aphthæ infantiles* of the older physicians, is evidently a very distinct affection from the *aphthæ* which occur in the adult as well as in the infant. The first depends upon an erythematic inflammation of the mucous membrane of the mouth, in which an altered secretion, in the form of small and curd-like particles or flocculi, or, as in other diphtheritic inflammations, of large patches of a soft pseudo-membranous matter, takes place upon the surface of the inflamed membrane. Upon the separation of these morbid exudations, the membrane beneath is found to be smooth, and without any solution of continuity. According to Guersent, the curd-like exudation is deposited beneath the epithelium, and its separation is consequent upon the rupture of the latter; Plumbe is of a similar opinion; Guyot and Billard, however, never saw it, excepting upon the surface of the epithelium, and this accords with our own experience.

In mild cases, the inflammation of the mouth quickly disappears, the particles of curdy matter becoming detached, fall off, and leave the membrane beneath of a smooth and healthy appearance. In other cases the inflammation continues for a greater length of time with little abatement: the spots of curd-like matter increase in number, and, occasionally, form by their union large patches, often of considerable thickness — these patches, sooner or later, become detached and fall off, and their place is quickly supplied by a new exudation: and this separation and renewal of the patches continue as long as the inflammation lasts. In some instances, the patches upon the tongue, lips, and cheeks, coalesce, and the whole of these parts become coated with a whitish, granulated crust, which not unfrequently extends into the fauces, pharynx, and, occasionally, into the larynx.

In situations where a number of children are crowded together, in ill-ventilated apartments, the patches of exudation acquire often a dark colour, the breath of the infant becomes fetid — the submaxillary glands enlarged and painful, the face swollen and of a dusky-red hue — the lips and gums become tumid, and bleed upon the slightest touch, while a fetid sanious saliva flows constantly from the mouth. Diarrhœa often attends, the discharges being dark-coloured and highly offensive. The disease, in its milder forms, is rather troublesome than dangerous — and it may be removed very speedily by an appropriate treatment; but when it is attended by the

symptoms last described, it is very apt to terminate fatally, death being occasionally preceded by a deep comatose condition.

There exists some difference of opinion among medical writers as to how far the morbid exudation characteristic of this affection extends beyond the mouth and fauces. It is generally admitted that, in severe cases, it has been found in the œsophagus, as far down as its cardiac orifice: but, while some, including Billard, have asserted that it has been observed throughout the whole tract of the alimentary canal, others, with Veron, maintain that, in no instance has it been met with beyond the the œsophagus—the curd-like crusts, occasionally found in the stomach being, as they suppose, conveyed thither from the mouth by deglutition. Guyot gives a case, in which the disease was detected, after death, throughout nearly the whole tract of the intestines. In the few opportunities we have had of examining the state of the alimentary canal in infants who have died whilst labouring under this disease, we were unable to trace the exudation beyond the œsophagus—a similar statement is made by Dewees and Eberle.

We are to recollect that the disease just described is the result of an erythematous inflammation of the mucous membrane of the mouth; while *aphthæ* are produced by a follicular inflammation of the same membrane. The latter affection is more commonly observed about the period of dentition than at an earlier age—it is particularly liable to occur in children of a lymphatic temperament, or in whom hæmatisis has been rendered imperfect, by improper or innutritious food, a damp and cold, or impure and stagnant atmosphere, exclusion from the light, and neglect of cleanliness. It is, also, of very common occurrence during most of the chronic affections, especially of the intestinal canal, in persons of all ages.

In the commencement of the disease the mucous membrane of the mouth becomes increased in redness, and upon the inner surface of the lips and cheeks, on the sides and inferior surface of the tongue, and, occasionally, over the greater part of the soft palate, there soon appear a number of distinct white specks, which, upon examination, are found to be small transparent vesicles, of a greyish or whitish colour, each being surrounded at its base by a raised, hard ring of a red colour more or less decided. These vesicles often occur in groups, or they may cover the whole of the lining membrane of the mouth and fauces; extending, in some cases, into the œsophagus, and throughout the alimentary canal; while in other cases they penetrate into the pharynx, and, according to some writers, into the larynx and even the trachea. The follicular inflammation will often continue for some time, as just described, without making any further progress, and it may often be arrested before ulceration ensues. Usually, however, the vesicles enlarge in size, rupture, and give discharge to a whitish matter; a superficial ulcer now occurs, with slightly elevated edges, and surrounded by a circle of inflammation. These ulcers often secrete a white, curdy matter, which adheres, at first, to their surface, but becomes subsequently detached, and is either swallowed or ejected with the saliva. It is often succeeded by a new exudation of a similar matter, and thus, in protracted cases, the crusts of curdy matter are repeatedly detached and renewed; or only a portion of the crusts are detached, while the general layer of curdy matter remains adherent, often for weeks. When the aphthous ulcerations are numerous, and in close contact, this curd-like exudation may spread from one to the other, and form a layer, often of considerable thickness and extent. Occasionally, the exudation from the ulcers is mixed with a small portion of blood; it then forms dark-coloured crusts, which have, not unfrequently, been mistaken for gangrenous sloughs.

Upon the subsidence of the inflammation the ulcerated follicles readily cicatrize without leaving any permanent scar.

Occasionally, particularly in children of debilitated habits, and who are deprived of proper hygienic influences, the aphthous ulcerations become gangrenous, their edges shrink, and assume a ragged, flabby appearance; a brownish slough forms in their centre, which, on separating, leaves a granulated surface of a vermilion hue; or, in place of a slough, the ulcers become covered with a brown, creamy fluid, that exhales a very decided gangrenous odour. The parts surrounding the ulcers become tumid, soft, and of a violet hue. From the half-open mouth of the child is discharged a ropy, often fetid, saliva. The countenance becomes pale and puffy; the pulse feeble; and the entire surface of the body pallid, and deficient in sensibility:—

repeated vomiting, profuse diarrhœa, and a tympanitic condition of the abdomen, generally occur; and occasionally, hiccup with frequent eructations, and the patient finally dies, according to Billard, without febrile reaction or cerebral excitement.

Recently, M. Gruby has attempted to show that, aphthæ and muguet are invariably produced by the development of a cryptogamic vegetable.

According to the researches of this gentleman, the affection consists, at first, of small, conical, whitish elevations, twenty-five millimetres in diameter, dispersed over the mucous membrane of the mouth. These elevations quickly augment in size, and extend rapidly over the adjacent surface, until, finally, the whole of the mouth, fauces, and sometimes the entire extent of the alimentary canal, appear to be covered with a pseudo-membranous layer, from two to three millimetres thick, and strongly adherent to the subjacent tissue. When a portion of this substance was examined under a microscope, M. Gruby found it to be composed entirely of a collection of cryptogamic plants, the roots of which, of a cylindrical form, transparent, and about one-four hundred and eightieth part of a millimetre in diameter, are implanted in the cellules of the epithelium. During their development, projections from these roots penetrate the entire series of cellules of which the epithelium is composed, to arrive at the free surface of the mucous membrane.

According to M. Gruby, these cryptogamic plants have considerable analogy with the sporotrichium described by some botanists. Being very fragile, they become detached by the movements of the tongue and lips, and mixing with the food, are carried into the alimentary canal, a considerable portion of the mucous coat of which they subsequently cover. Those children in whom this extension of the disease takes place to any great extent, fall into a state of marasmus, and soon die.

M. Gruby, having never detected in the white substance of aphthæ or muguet any thing else than the cryptogami just described, and the cellules of the epithelium,—there being, in no instance, any product of inflammation present,—considers himself authorized to conclude that aphthæ are neither more nor less than a vegetation occurring upon the living mucous membrane. Views precisely similar to those of M. Gruby have been advanced by Dr. Berg, of Stockholm.

Dr. Oesterlein has also submitted the matter of aphthæ and muguet to microscopic examination, and has been able to detect in it a vegetation similar to that described by the observers just named. This vegetation he has met with, however, only at the period of the fullest development of the aphthæ or muguet, which latter he considers to be the product of an exudation resulting from an inflammatory condition of the mucous membrane; the production of the confervæ being purely accidental. He attempted, repeatedly, to transplant them to other animal tissues or fluids, but without success. —(See *Condie on Diseases of Children.*)—C.]

Hard by the salivary glands lie the tonsils: and one of Cullen's species of cynanche is the *cynanche tonsillaris*: in more modern language, tonsillitis, or amygdalitis; or, in the vernacular, quinsy, common inflammatory sore-throat: a disease which, though internal, is yet within the reach of our sight, and easily recognised.

The popular term quinsy is in truth traceable — through the French *esquinancie* — to the scientific term cynanche.

This common and troublesome disorder occurs with very unequal severity in different cases. The differences depend upon the extent of the disease, and upon the number and variety of the parts which it involves: for it is seldom limited entirely to the tonsils, but spreads to the uvula, the velum palati, the salivary glands, the pharynx, and even to the root of the tongue, and the neighbouring areolar tissue. When the inflammation is superficial it does not produce any great distress, even though it may be diffused. When it penetrates through and beyond the mucous membrane, it is apt to end in suppuration, and to harass the patient much: the tonsils swell to an enormous size, and at length deep abscesses form in them. The disease is worst of all when the back part of the tongue, and the muscular and areolar tissues thereabouts, become implicated: it may chance to reach even the larynx, and then it is always and extremely perilous.

Under its more ordinary form, cynanche tonsillaris generally manifests itself, at first, by a slight degree of uneasiness and difficulty in swallowing; with a constant dryness and sense of constriction in the fauces, and a feeling as if some foreign sub-

stance were sticking there. Upon inspecting the throat, more or less of inflammatory redness and swelling is seen of one or both of the tonsils. Sometimes both of them are affected at once. Very frequently one only is first attacked: and the swelling begins in the other as it subsides in the first. This is just what occurs in many instances also of cynanche parotidæa. The uvula is commonly enlarged and elongated, and of a scarlet colour. Sometimes it drags upon the back part of the tongue, or dangles into the pharynx, causing the disagreeable sensation of a foreign body continually present, and provoking, by its mere contact, painful and fatiguing acts of deglutition. More frequently the uvula may be seen to be adherent to that tonsil which is most swollen. The dryness of the fauces soon gives place to a copious secretion of transparent mucus, which is frothy and viscid, and sticks to the inflamed surface, so as to be detached with difficulty; and the patient is tormented by continual and painful efforts to hawk up, or to swallow, this mucus. In an early stage of the disease, opaque whitish spots appear upon the red tonsil. They are exudations from its surface, or the discharged contents of the mucous crypts there situate. It is important that you should be aware of this, lest you mistake such specks for ulcerating or sloughing points, such as occur in some other affections of the throat, but which are not common in the outset at least of this.

When the inflammation is violent, the submaxillary and parotid glands sometimes swell, and become tender on pressure; and, less frequently, the patient is troubled by profuse ptyalism. In other words, the inflammation spreads from the tonsils to the salivary glands, and secondary parotitis occurs; sometimes with and sometimes without an augmentation of their natural secretion. Unable, or unwilling, to swallow the abundant saliva, the patient allows it to dribble from his mouth.

Now and then, although the act of swallowing is difficult and painful, you perceive, on looking into the fauces, no appearance which can account for these symptoms. The inflammation is seated lower down in the throat; out of sight. This cannot with propriety be called cynanche tonsillaris; indeed, it forms a distinct species, the *cynanche pharyngea* of Cullen. I mention it here because it really does not require any separate consideration.

The pain in cynanche tonsillaris is felt almost solely during the act of deglutition; which is difficult also from the mechanical narrowing of the passage by the enlarged glands. When both tonsils are affected at once, and much swollen, they push forwards the anterior pillars of the velum palati, and project in the shape of two great balls of flesh, into the arch of the fauces, so as to leave a very small space only between them; and they sometimes even meet and touch each other, and suffer ulceration from their mutual pressure. When attempts are made to swallow liquids, they are apt to return through the nose; the backward passage can no longer be shut in consequence of the tumid and fixed condition of the velum palati. The patients are unable to swallow even soft solids; indeed, the pain of swallowing is so great, that they are not easily persuaded to try. In severe cases pain shoots from the throat to the ear along the course of the eustachian tube; and this is considered important, as being indicative of suppuration. I believe that suppuration does occur in the majority of the cases which are attended with this symptom. Sometimes there is tinnitus aurium, and partial deafness, from the obstruction produced to the passage of air through the eustachian tube: either by closure of its extremity in consequence of the swollen state of the parts about it, or by some thickening of its lining membrane from an extension of the inflammation along its channel.

When the inflammation is intense, and involves the root of the tongue, the patient becomes unable to open his mouth sufficiently to allow the fauces to be seen; and the inflamed parts can be examined only by means of one's finger. In some instances the power of separating the jaws is so limited, as not to permit the introduction of the finger, and the tongue is incapable of any motion.

In ordinary cases, however severe the disease may be, there is scarcely any affection of the breathing. But the throat is so blocked up, and the free play of the velum palati so impeded, that the speech is altered; becoming thick, guttural, and inarticulate. You may often recognise the disease by the peculiar sound of the patient's voice, without looking into his throat. When the swelling of the tonsils is very great, the breathing does sometimes become impaired; and it is in such cases alone, or nearly in such cases alone, that this disease is at all alarming.

Cynanche tonsillaris is commonly attended, from the very outset of the inflammation, by smart inflammatory fever, severe headache often, and a rapid pulse, rising to 120 beats in the minute, or more. The constitutional disturbance runs higher than we might have expected, considering the limited extent of the local inflammation, and the comparatively small importance of the part inflamed. At the same time there is very little of that debility and anxiety which are observed, as we shall see hereafter, in common continued fevers.

The inflammation often terminates by resolution; but when it is violent or long-continued, it very frequently leads to the formation of pus. When the mucous secretion increases in quantity, and becomes less viscid, while the difficulty of swallowing diminishes, and the febrile symptoms decline, resolution may be expected. Suppuration, on the other hand, may be looked for when the inflammation is unusually intense; when, by the swelling of the tonsils, the breathing is impeded; when a pulsating pain is felt, shooting to the ear; when the patient can scarcely open his mouth, or protrude or move his tongue; when there is more than usual external swelling; and when the symptoms increase, or even fail to remit, after five or six days have elapsed. Rigors sometimes accompany and announce the suppuration: and afterwards the pus often may be seen shining through the membrane covering the tonsils. In many cases, however, it lies so deep that it cannot be detected by the most careful examination. At length the little abscess bursts; and the relief thereupon experienced by the patient is sudden and striking. All at once the pain ceases; he can swallow, and he feels himself well; and often indeed he is well, or nearly so. The matter discharged has always a nauseous taste and a remarkably fœtid smell: and sometimes the fœtor, or the ill taste, is the only sign, besides the improvement in the symptoms, that indicates the rupture of the abscess; the pus being so small in quantity as, when mixed up with other matters, easily to escape notice; or (what frequently happens), passing backwards into the stomach, under an involuntary movement of deglutition. Suppuration sometimes, but rarely, takes place externally, in the areolar tissue of the neck, as well as internally.

The termination in gangrene is spoken of in books: and it is just possible; but it never happens, I imagine, except in unhealthy constitutions; or when tonsillitis occurs as a complication of more general disease, such as scarlet fever. It is much more common to see repeated attacks of cynanche tonsillaris produce what used to be absurdly called scirrhus, a permanent enlargement and hardening of the tonsils. While they are in this state, a low kind of inflammation is very readily re-excited in them. The enlarged tonsils are generally irregular, and notched; something like the surface of a walnut shell; and a white or yellow secretion is often to be seen lying in the hollows. This appearance may easily be mistaken, by an inexperienced eye, for ulceration.

There is not much risk of your confounding cynanche tonsillaris with any other complaint. The various species of cynanche enumerated by Cullen are separated each from the others, by certain broad lines of distinction: all derived, however, from different combinations of two symptoms; — *dyspnœa* and *dysphagia*. Thus, in cynanche tonsillaris, deglutition alone is difficult; and when you look into the throat, you see at once why it is difficult. There is equal difficulty of swallowing, and equal freedom of respiration, in cynanche *pharyngea*: but the cause of the dysphagia is not visible. In cynanche *trachealis*, the respiration is much affected; the facility of swallowing not affected at all. In cynanche *laryngea* both these functions are implicated; there is difficulty of swallowing, as well as difficulty of breathing. Cynanche *parotidæa* is distinguished by the *absence* of both the symptoms; its title to the name of cynanche is therefore equivocal. Many of these points will soon come under our notice again.

Cynanche tonsillaris is supposed to be most frequent among the young and plethoric. But it certainly occurs very often also in persons who are pale, and spare, and feeble; and in those of middle age. Some individuals appear to have, by nature, a strong predisposition to the disease; and in them its attacks are more or less periodical; recurring at particular seasons, and commonly during the variable weather of spring and autumn. The peculiarity runs sometimes in families. The liability to the complaint is also very much increased by repetitions of the attacks.

The only exciting cause worth mentioning, almost the sole cause indeed, is expo-

sure to cold. Not unfrequently it assails so many persons at the same time as to be epidemic; and for that reason it has sometimes been thought contagious. But it has no contagious property whatever. Although we often see several members of the same family affected by it at once, yet we may learn, upon careful inquiry, that its commencement in the different cases has been too nearly simultaneous to admit the supposition of its having been communicated from one to another. The patients have all been exposed to the same unwholesome influences, which operating upon similar constitutions, such as those who are sprung from the same parents may be expected to possess, have produced similar effects. This prevalence of the disorder at certain times and places, is connected, no doubt, with some peculiar conditions of the atmosphere.

The prognosis is almost always favourable. Not but what death may be produced by this disease, under peculiar circumstances, and when the inflammation is unusually violent and extensive. The late Dr. Gregory, of Edinburgh, used to mention in his lectures one instance, the only one he had met with among many hundred cases, of death from cynanche tonsillaris. He did not see the patient till he was moribund; and he suspected that it was combined with typhus fever. The only fatal case that I ever witnessed, occurred some years ago at the Middlesex Hospital, in one of my own patients. He was a stout young man, 26 years old, a private coachman. The complaint was clearly traced to his having got wet through, more than once, a day or two before it came on. Besides the ordinary symptoms of cynanche tonsillaris, there was great external swelling on both sides of the throat, and the patient was unable either to open his mouth, or to move his tongue. The inflammation involved not the tonsils merely, but the base of the tongue, the salivary glands, and the surrounding areolar tissue. At length suppuration took place. The abscess broke internally, and pointed also externally, just below the symphysis of the chin, where it was opened with a lancet. Two days after, there was a sudden gush of blood from the mouth. So immovable were his jaws that it was impossible to determine from which side the hæmorrhage proceeded; it was stopped, however, apparently by the treatment adopted. A fortnight later, the bleeding recurred profusely. It was now evident that the blood was arterial, and that it came from the left side of the throat. Preparations were made for tying the common carotid on the left side; but just as the operation was about to be begun, the patient expired in our presence. His death was shocking, but full of pathological interest. He did not sink, as you may have supposed, in the way of syncope, from loss of blood; but by suffocation. The blood passed down the trachea and into the lungs; and he had been so weakened by the previous hæmorrhage, that he could not expel the blood thus introduced, which actually choked him. A large clot was afterwards found, filling up the wind-pipe. I felt this man's pulse beat firmly and regularly, for a minute perhaps, after his last effort to breathe. On examining the body it was discovered that the abscess had opened internally behind and below the left tonsil. The lingual branch of the carotid artery crossed the site of the abscess; and had been severed and laid open by ulceration. From this vessel the fatal hæmorrhage had come.

It should be borne in mind also that cynanche tonsillaris does sometimes, by extension of the inflammation to the neighbouring parts, superinduce that very formidable species of cynanche, of which I am soon to speak, the *cynanche laryngea*. All cases in which the *breathing* is in any degree affected, should excite suspicion, and strict scrutiny; although the dyspnoea may be produced by the mere swelling of the tonsils.

You will understand, then, that cynanche tonsillaris may, under unusual and untoward circumstances, prove a fatal disease; but that it is so very rarely indeed. In almost all cases we may say that the life of the patient is not in danger.

In the uncomplicated and milder form of the disease, when the inflammation is superficial and the fever slight, no great activity of *treatment* is requisite. The patient should be kept within doors, and even in bed: for a troublesome tendency to a recurrence of the disorder may be fostered by neglect or imprudence. Cooling saline purgatives will be proper, and the antiphlogistic regimen. A strip of flannel may be put round the neck, and some stimulating embrocation applied to the exterior of the throat, beneath the ramus of the jaw: the compound camphor liniment is well adapted to this purpose. Some such plan as this will generally suffice, not indeed to

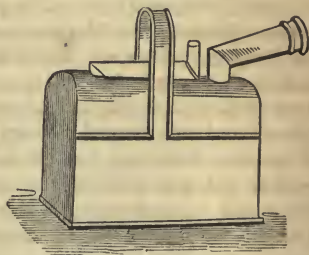
stop the inflammation of a sudden, nor to put an end at once to the fever, but to cause the complaint to run its course evenly, and to go on to resolution in a few days. Commonly it is not completely over until both the tonsils have been attacked in succession.

When you catch the disorder in its very outset, I believe you may sometimes succeed in cutting it short by an emetic: a scruple of ipecacuanha, for example, with a grain of tartarized antimony. In all cases it is right to administer a brisk purgative.

A great variety of astringent, acid, and other gargles, have been employed in this disease; and their good effects have, I apprehend, been much overrated. Many cases would do quite as well, or better, without them: for in the early stages strong astringents, and the straining and movements of the throat that accompany their use, may even be hurtful, and increase the pain and the inflammation. The only gargle which I should consider admissible in the commencement of the malady is a gargle of warm milk and water. I have known of one instance in which quinsy suddenly attacked a gentleman who was extremely anxious to use his throat, in public speaking, the next day. He occupied himself perpetually for some hours in this sort of fomentation of the tonsils with hot water; and with such good effect, that on the day following he was able to accomplish his object. Still there are cases in which, at certain stages of the disease, detergent gargles are serviceable, by assisting the excretion of the mucus that collects in the fauces, and by correcting fœtor. A weak solution of chlorine in water answers well. In more chronic sore-throats, stimulating gargles may often be employed with advantage. When the inflammation is violent, a slightly stimulant linctus is preferable; it *cuts the phlegm* as they say, *i. e.*, it promotes its detachment and removal. Of this kind, current jelly is one of the best.

But far better than anything else, as a local application to the inflamed fauces, is the steam of hot water; whether we are hoping for resolution of the inflammation, or whether we desire to promote and hasten the process of suppuration already begun. The inhaler introduced by myself into the Middlesex Hospital, and elsewhere, though somewhat clumsy in appearance, is the most convenient and effectual that I am acquainted with. I show it to you. It was invented in Edinburgh by a friend of mine long since dead, Mr. Hercy. It will stand upon a table, or lie upon a pillow; and a large volume of steam is carried inwards against the fauces by the mere natural breathings of the patient. Most of the inhaling machines that I have seen require a sucking effort, like that made in smoking a pipe: an effort that is apt to be irksome and fatiguing, especially in pulmonary diseases; for some of which this method of applying vapour directly to the suffering part is as useful as it is for sore throats.¹

FIG. 33.



Blistering the outside of the throat is a favourite remedy with many. When early applied, a blister often does much good, and probably prevents suppuration in some cases. But I have found blisters of uncertain efficacy; they leave a mark which lasts for some time, and which patients of the other sex are apt to complain of. For these reasons I prefer mere rubefacients; the liniment I mentioned before, or the compound soap liniment, or a mustard poultice folded between two layers of thin linen.

In more severe cases leeches applied to the upper part of the throat, just below the angles of the jaws, have been found to give sensible relief: and in the worst degrees of the disorder, when there is much outward swelling, and the jaws and tongue are fixed, leeches are absolutely requisite. It may be proper to take blood from the arm also. The necessity for active depletion must be measured by the severity of the local symptoms, the intensity of the fever, and the general strength and condition of the patient: and of these things a little experience will teach you to judge.

It is not to be expected that either leeches or blisters will be of much use after the process of suppuration has commenced; nay, they may sometimes be injurious by

¹ It would be wrong not to state here that Mr. Squire, of Oxford-street, has devised an inhaler which, combining the advantages of Mr. Hercy's, is less clumsy in form, and as convenient for use.

retarding it. It is frequently a difficult matter to determine whether pus has yet formed or not.

I have already admonished you to make a careful examination of the throat, and to watch your patient narrowly, whenever he experiences any difficulty of breathing. Dyspnoea may be produced by the mere swelling of the inflamed part: and when it concurs with much enlargement of the tonsils you had better pierce them with a lancet. If they contain matter, it will be evacuated; and if not, the bleeding produced by the puncture will generally reduce the swelling somewhat, and relieve the patient. There is an instrument made on purpose for this small operation, consisting of a lancet enclosed in a flat silver sheath, from the end of which it is made to protrude, to a certain extent only, by pressing upon a spring. The instrument should be directed towards the centre of the fauces, and not outwards; in order to avoid wounding important vessels or nerves. Dr. Cullen indeed says, "this does not require much caution:" but notwithstanding this high authority, I must warn you that puncturing or scarifying the tonsils is an operation not to be carelessly, or rashly, or wantonly performed. Portal mentions a case in which a skilful surgeon in scarifying the tonsil of his patient, wounded as he supposes some ramification of the internal carotid, and the patient was presently dead. The artery lies, as you know, very near the tonsil; and only a few years ago, in Ireland, it was struck by a surgeon while scarifying a gentleman's tonsil; and the gentleman died in three minutes. This I was told by the late Dr. Barclay. The case I related just now of fatal hæmorrhage from the lingual artery points to the same danger: and since that case occurred two others involving similar hazard have fallen under my own notice, and impressed me with a strong feeling of the necessity of caution. A man was brought into the hospital with profuse hæmorrhage from the right tonsil or its immediate neighbourhood, the consequence of syphilitic ulceration of those parts. He had lost three or four quarts of blood and was nearly dead. His life was saved by Mr. Mayo, who tied the common carotid on that side.

Last February (1838) a boy, from Harrow School, was placed under my care, in whom cynanche tonsillaris came on during convalescence from scarlet fever. So much swelling was there of both tonsils, that they met, and pushed the uvula outwards before them, and the breathing was much impeded. A surgeon who was in attendance with me punctured the tonsils. The next day a good deal of hæmorrhage took place; and this recurred several times, to a considerable and even an alarming amount. When the clots that formed were wiped away with a sponge, the blood could be seen welling out in a little stream, with a pulsating motion, from a small incision in the left tonsil. After much trouble and anxiety, the hæmorrhage was ultimately arrested, by applying a pencil of lunar caustic freely, within the bleeding orifice. Lint, wetted with the muriated tincture of iron, or with a saturated solution of alum, is a fit application in similar accidents.

Mr. Lawrence, who saw this case, told me that he once knew a patient die of hæmorrhage from the tonsillar artery.

I ought, perhaps, here to add, that Mr. Joseph Bell, of Barrhead, has strongly recommended the internal administration of powdered guaiacum, in large doses, as being almost specific in the cure of cynanche tonsillaris. He gives as much as half a drachm, suspended, by means of mucilage, in a draught, every six hours. Mr. Bell has no doubt that this remedy, if timely administered, will cut the disease short in ninety-nine cases out of a hundred. It has been found successful in other hands also. I have never had an opportunity of trying it.

The chronic enlargement of the tonsils, to which I have already adverted, is sometimes productive of great inconvenience and distress, and even of danger. Its occasional consequences are—habitual trouble in swallowing; confused and inarticulate speech; deafness in various degrees, from occlusion of the eustachian tubes; more or less impediment of breathing; and even spasm of the glottis and impending suffocation. The enlargement may in such cases be somewhat reduced, I believe, by repeatedly passing a stick of lunar caustic over the surface of the tonsils; or by repeatedly painting them with the compound tincture of iodine; but a much readier and a preferable plan is to amputate them, in part at least. This may be done by a ligature; or still better by scissors, or by a sort of small guillotine invented for that purpose.

It is not a very painful operation. Mr. Arnott removed one lately from one of my hospital patients; and but a very few days ago (December 1838) Mr. Mayo brought two, in a piece of paper, to the hospital. He had just before cut them off for a patient whose respiration they had much embarrassed.

[HYPERTROPHY OF THE TONSILS IN CHILDREN. — In young children the tonsils are subject to a chronic enlargement, the effects of which are far more serious than the entire neglect which the affection has met with from medical writers would lead us to suspect. In many cases there is no doubt that the enlargement is due to a sub-acute inflammation of the tonsils; in general, however, it would seem to be dependent simply upon the irritation of teething. It is commonly developed in children between six months and two years of age, thus coinciding with the most active period of dentition. In proof of the enlargement being due to the irritation of teething, M. Robert, who has examined the subject with some care, (*Bull. Gen. de Therapeut.*, May, 1843) remarks, that he has seen the evolution of the dens sapientia in the adult attended with a similar inflammation and hypertrophy of the tonsils.

The enlargement is always in both tonsils, and becomes, in many cases, very considerable. When of any extent it affects the voice, giving to it a peculiar nasal tone, and, by its pressure on the Eustachian tubes, impairs in a great degree the sense of hearing. By forcing up the velum palati, it also interferes with the freedom of respiration, hence those affected with it sleep with their mouths open. They are affected likewise with a constant troublesome dry cough; and the air in respiration being prevented from passing through the nares, causes the nose to remain undeveloped in breadth, and the anterior part of the face thin and, as it were, pinched. The most important result, however, of the enlargement of the tonsils is a flattening of the chest, to which Dupuytren first directed attention. This flattening M. Robert supposes to be produced by the enlarged tonsils preventing a sufficient quantity of air being admitted at each inspiration, sufficiently to fill the chest, or to cause from within the lungs a pressure equivalent to that of the atmosphere without. The deformity once established, it necessarily gives rise to dyspnœa, palpitation, and the usual results of interrupted respiration and circulation; hence the children in whom it exists are commonly pale, thin, and feeble.

When enlargement is once established, the hypertrophied tonsils never diminish in size; their excision consequently is the only means by which the inconvenience and injury resulting from their presence are to be remedied. For diminishing the deformity of the chest, M. Robert suggests various plans and different forms of gymnastic exercises.

Recently the application to the enlarged tonsils of the iodide of zinc, is said to have the effect of causing their rapid absorption. The article is prepared by placing a clean plate of zinc over a jar or vial, and sprinkling iodine over it. In a short time the iodide is deposited in the vessel, in the form of a semi-fluid deliquescent substance. This is to be applied pure, to the surface of the enlarged tonsil, by means of a camel's-hair pencil, or a piece of sponge, secured to a suitable handle. It is to be held on for a short time, and repeated every two or three days, until the object is accomplished. The application is followed by a pungent smarting, which lasts for twenty or thirty minutes, but by no other inconvenience. Dr. Goddard, of this city, we are informed by Dr. Parrish, in his annual Report on Surgery, read to the College of Physicians, has used the remedy extensively, and speaks very favourably of its effects. He has found it to possess the property of inducing a rapid absorption of the enlarged tonsil, by a sort of shrivelling process, without the formation of a slough. It does not, like the chloride of zinc, spread to the surrounding healthy structure, and hence, may be used without the fear of injury from being swallowed. (*Summary of the Trans. of the College of Phys., of Philadelphia*, No. vii, page 191.) — C.]

LECTURE XLV.

Acute Laryngitis. Symptoms. Treatment; Blood-letting. Tracheotomy, Mercury, Antimony. Anatomical Characters of the Disease. Causes. Secondary Laryngitis. Œdema of the Glottis. Chronic Affections of the Larynx.

THE disease of which I have next to speak is of far more serious character than those which were considered in the last lecture. *Cynanche laryngea*, or acute laryngitis, has proved rapidly fatal in a large proportion of the instances in which it has been known to occur. Yet when the patient is seen tolerably early, and the nature of the malady is clearly perceived, and the source of peril thoroughly understood, I believe that our art is sufficient, in most cases, to rescue the sufferer from the fate that hangs over him. It is of the greatest importance, therefore, that you should be able to recognise laryngitis when you meet with it, and that you should comprehend the principles according to which it requires to be treated.

What is laryngitis? It consists, as the term implies, in inflammation of the parts composing the larynx; and especially of the mucous membrane that covers the laryngeal cartilages, including the epiglottis. The inflammation may be, and sometimes is, exactly limited to the larynx; but frequently it extends also to the posterior fauces, the velum palati, and the tonsils.

The *symptoms* of acute inflammation of the larynx are these. The patient complains of *sore-throat*. If you look into his throat you will commonly perceive some redness of the velum and uvula, and of the fauces generally. But there is a degree of restlessness and anxiety about the patient more than proportionate to the apparent inflammation. Among the earliest of the symptoms that bespeak danger, and ought to excite alarm, is *difficulty of deglutition*, for which no adequate cause is visible in the fauces; and to this is presently added *difficulty of breathing*, for which no adequate cause can be discovered in the thorax. The mode and character of the respiration are peculiar; it is attended with a throttling noise; the act of inspiring is protracted and wheezing, as though the air were drawn in through a dry, narrow reed. If you ask the patient what is the *seat* of his distress, *where* the disease is situated, he points with his finger to the *pomum Adami*. If he cough, he coughs with a peculiarly harsh, stridulous, husky, abortive sound. He either speaks quite hoarsely, or (what is more common,) all power of audible voice in the larynx is lost, and he speaks by means of his lips and tongue only, in a whisper. There is tenderness of the laryngeal cartilages; they are painful when pressed externally. The face is flushed; the skin hot and dry; the pulse hard. As the disorder advances, the patient's general distress increases: but some of the symptoms alter: his countenance becomes pale or livid, anxious and ghastly; his eyes protrude; he is miserably unquiet, impatient for some relief, declares or makes signs that he wants air, and begs that the windows may be opened; and if he does not *obtain* timely relief, he perishes—he dies strangled.

The pathology of this terrible disease is extremely simple. The membrane covering the interior surface of the instrument of the voice suffers inflammation. One effect of inflammation in mucous membranes is a thickening of those membranes; they become turgid and swollen. Another frequent effect is the effusion of serous fluid in the subjacent areolar tissue. By such tumid thickening of its lining membrane, the chink called the *rima glottidis* is narrowed: it is still further diminished in breadth whenever the membrane is lifted and protruded by infiltration of the tissue beneath it: it is so nearly closed up, that air cannot pass inwards in sufficient quantity to sustain the vital functions: a small portion only of the blood returned to the lungs from the right side of the heart undergoes the requisite change from venous to arterial. The miserable patient grows drowsy and delirious, and dies by a slow process of strangulation. If the rima glottidis become *quite* closed up, his sufferings, and his life, are quickly at an end.

This disease affords a good instance of a truth which was announced in a former

part of this course of lectures; viz., that the *danger* of a morbid change may depend entirely upon its situation. It is so, eminently, with laryngitis. The inflammation is sometimes limited to a spot of membrane not bigger than a square inch. If a square inch and no more of the same membrane, a little lower down in the trachea, were inflamed in the same manner and degree, the complaint would be quite unimportant. Cynanche laryngea derives all its peril from the circumstance that the inflammation tends to shut up what may be called the *janua vite*. The part affected subserves two purposes; it is the organ of speech; and it forms a portion of the channel through which air is conveyed from without into the lungs. Both of these purposes are impeded in laryngitis. Now the animal function of speech may be entirely and permanently suspended without any danger to life. The function of respiration, which, though under the influence of the will, is an organic function, will not bear to be *suspended*, even for a few minutes; and life cannot be *long* sustained when it is *much impeded*.

The difficulty of swallowing is a remarkable symptom, and almost always present. Yet it is not absolutely universal; for Mr. Lawrence describes a case in which it did not occur. It appears to result, in some measure, from the tumid and tender condition of the whole membrane which is common to the larynx and pharynx, and which is pressed upon as the larynx rises in the act of deglutition. But this symptom depends also, and in a greater measure, upon the state of the epiglottis, which is often enlarged, and fixed by the swelling in an erect position, and unable to execute its natural valvular office: so that when the patient makes efforts to swallow, a portion of the food or drink gets into the larynx, and a paroxysm of choking dyspnœa ensues. By pressing down the back part of the patient's tongue, and getting him at the same moment to make a coughing effort, you may sometimes obtain a sight of the tumid, red, and upright valve.

The dyspnœa is constant: yet there are pauses of comparative ease and quiet; and there are, commonly, periods of severe aggravation and urgent distress. It is probable that the permanent narrowing of the chink by the inflammation and its consequences is from time to time increased by a spasmodic contraction of the muscles that close the glottis.

This is the first disease that has come before us, in which the respiration has been *primarily* impeded. If you call to mind what was stated in one of the early lectures respecting death by apnœa, you will be at no loss to understand the manner in which life is destroyed in laryngitis.

This formidable malady has always existed; for you may trace examples of it, under various names, even in the writings of the ancients. But it is only in recent times that it has been singled out from the rest of the anginæ, and made a separate object of study. It has numbered some distinguished medical men among its victims: Dr. David Pitcairn, Sir John Macnamara Hayes, Sir George Tuthill. The celebrated General Washington died of it. When it has caused death it has generally run a rapid course, and proved fatal before the fifth day. It *has* carried the patient off in less than twelve hours.

It is of the utmost consequence to make an accurate diagnosis. Laryngitis is easily distinguished from cynanche tonsillaris by the extreme and peculiar dyspnœa which attends it. There may be difficulty of breathing in the latter disease, from enormous swelling of the tonsils; but then such swelling will mostly be *visible*. In laryngitis, the marks of inflammation to be *seen* on inspection of the fauces are generally slight and trifling, and quite inadequate to explain the difficulty of swallowing. Do not, however, forget, that laryngitis may *supervene* upon cynanche tonsillaris. Again, cynanche laryngea is readily discriminated from cynanche pharyngea; in which complaint there is great pain and difficulty in deglutition; but the breathing is quite free. In cynanche trachealis, or croup, which I shall next describe, the breathing is affected, and the swallowing is *not*.

What is to be done for a patient labouring under acute laryngitis? How and when are we to employ the great remedy for acute inflammation—blood-letting? or are we to employ it at all? These are points concerning which it is quite necessary that your minds should be prepared and prompt to decide. If you look merely to the results in the recorded cases of this fearful complaint, you will scarcely find an answer to the question. In some of them, copious bleeding appeared to save the patients;

in others, it was of no service, but rather seemed to accelerate their death. Sir John Macnamara Hayes suffered two attacks of cynanche laryngea. In the first he was freely bled. Dr. Roberts, of Bishop Stortford, informs us that the first bleeding was attended with *considerable relief*; the second also with *manifest advantage*; and by the third, *his safety appeared to be insured*. Fifteen years afterwards he died of the same disorder, for which he was again bled and leeches, under the care of the late Dr. Baillie. Washington was largely bled, and died. Again, Dr. Francis, of New York, recovered from acute laryngitis after copious venæsection, and still survives, enjoying a well-won reputation, in that city. It is evidently needful to consider and determine the circumstances under which we are to use, or to withhold, the lancet.

Bleeding, to be serviceable, or safe, must be performed *early*. There is, perhaps, no disease in which the *καιρος οὖν*, the fleeting opportunity, is more conspicuous than in this. When I say that you must bleed early if at all, I do not mean that you are to reckon so many days or hours from the commencement of the disorder; but you must ascertain what progress it has made; for it travels sometimes at a railroad-pace. You must look to your patient's actual condition; and I apprehend that your practice, in respect to blood-letting, may be safely guided by the following rules. When there is high inflammatory fever present, and the skin is hot, the pulse firm and full, and the cheeks are red, and the lips florid, you may bleed your patient with decision and advantage. But if his powers are beginning to sink under the poisonous influence of imperfectly aerated blood, if his skin be cold, or even cool, his face pale or leaden, his lips blue, his pulse small and feeble, his mind wavering — you will do no good by blood-letting: nay, you will increase the debility which already exists, and hasten the fatal catastrophe.

With regard to *local* blood-letting, and to counter-irritation, there is one remark made by Dr. Farre of much practical importance. It is a common practice in affections of the throat, to apply leeches over or near the laryngeal cartilages, and afterwards to place a blister there. Now serous infiltration of the neighbouring parts often follows leech-bites; and the effect of a blister in producing serous effusion often extends beyond the skin; and the cartilages of the throat lie very near the surface: and it is possible that œdema of the glottis might be produced, or augmented, in consequence of these topical remedies. It will be better, therefore, when we wish to take blood locally, to take it by cupping from the back part of the neck: and when we desire to produce counter-irritation, it will be prudent to lay a blister on the upper part of the sternum, rather than to the front of the throat.

In the advanced stage of the disease, *medicine*, I fear, can effect but little.

But *surgery* may be more successful.

The danger arises from the *mechanical* obstacle to the entrance and exit of air into and from the lungs; and this state of peril admits of a *mechanical remedy*. If an artificial opening be made between the obstructed part and the lungs, the air is again freely inhaled and freely expelled; the blood undergoes the vital change from purple to scarlet; and the patient is placed in a condition of safety. He continues to respire through the hole thus drilled in the trachea, until the inflammation of the larynx has subsided; the thickening of the membrane disappeared; the submucous infiltration been re-absorbed; and the vocal instrument been restored to its natural integrity: and then the aperture in the windpipe may be suffered to heal, and the patient will again draw his breath through its natural channels.

This is one of the triumphs of the healing art. It requires a knowledge of the general *pathology* of the disease, *i. e.*, an acquaintance with the facts that acute inflammation may affect the larynx almost exclusively, and that its tendency is to narrow the fissure of the glottis, and destroy life by suffocation. It requires a knowledge of the *symptoms* of such inflammation: and it requires an accurate knowledge of all the essential circumstances of the particular case. For it is not every case in which the transit of air through the slit in the larynx is hindered, that is a fit case for the operation of tracheotomy. Some years ago there was brought to me by a surgeon, a man breathing with considerable labour and constraint, the air passing through the larynx with an audible hissing noise. The surgeon wished to know my opinion of the propriety of opening the patient's windpipe. He had come to the conclusion that there was ulceration of the membrane lining the larynx, with thickening;

that the cause of the sibilous respiration was partly mechanical, partly spasmodic; the little muscles that close the glottis acting with injurious energy in consequence of the neighbouring irritation: and he thought that this mischief in the larynx would have a better chance of being repaired, if the functions of the organ could be for a time suspended. He was aware, however, of the necessity of ascertaining what was the condition of the *lungs*; and he had not studied auscultation long enough to trust his own ear in that matter. The patient was pale and thin, and emaciated; and three minutes sufficed to convince me that his lungs were extensively disorganized. His respiration was not so difficult as to threaten suffocation; he was not dying of the laryngeal obstruction; and I recommended that he should not be subjected to an operation which might curtail his existence, but could not effect a cure. The man died soon after: and we examined his body together. There was, as my friend had supposed, ulceration of the membrane near the chordæ vocales, and the lungs were full of suppurating or softening tubercles. I mention this case to show you that it is necessary to ascertain the condition of the thorax generally, before we perform or sanction such an operation as tracheotomy. Not that there is anything very formidable, or painful, or dangerous, in the operation itself. But if we cut a hole in a patient's throat, who is sure to die soon after of some other incurable complaint, we shall incur the risk of being charged with having killed him. Do not misunderstand me, however. If a patient's *life* be threatened by acute laryngitis, or by laryngeal œdema, and we are sure of that, and if at the same time we are sure that he carries another mortal disease about him, we are not for that reason to let him die, if we can help it, of the *laryngitis*; any more than it would be lawful for us to administer a drachm of prussic acid to a man condemned to be hanged the next morning. But we must state the whole of the case plainly to the patient's friends, and propose the operation as a mode not of effecting an absolute cure, but of staving off the *immediate* danger.

And here let me repair an omission of which I was guilty when speaking just now of the diagnosis. My object was to guard you against mistaking laryngitis for some other malady: but I must also warn you against the converse error, that of mistaking some other malady for acute laryngitis. I can assure you that such a mistake has been made; and tracheotomy has been performed, too, when there was no disease in the larynx; and the practitioners by whose authority it *was* performed have been ungenerously reproached for their error, although no harm beyond the slight pain and inconvenience of the operation resulted from it. The cases in which this blunder has been committed have nearly all, I believe, been cases of *aneurism of the thoracic aorta*, which, by its pressure on the first divisions of the air-passages, or on the nerves thereabouts distributed, had caused that kind of laboured and stridulous breathing which is characteristic of laryngitis. I may venture to say that no person who has had opportunities of educating his ear for the purposes of auscultation, and has made a proper use of those opportunities, could ever overlook such a complication as this. I have myself seen a woman (I mentioned her case before), whose trachea was laid open by a surgeon while she was suffering under mere hysteria; so closely did that disease mimic laryngitis.

When you have good evidence that a mechanical obstruction to the passage of the air exists in the larynx, and that the tubes *beyond the larynx* are pervious and free; there are two things which I would urge upon you. First, I would most earnestly advise you not to *wait too long* before you propose or perform tracheotomy; and secondly, never to omit performing it *merely* because it may appear to be then *too late*. If, in the acute and limited disease, an artificial opening be made while the patient's strength is yet entire, and before his whole system is poisoned with venous blood, or his lungs are overwhelmed with sanguine congestion and serous effusion, it will almost infallibly save his life. But if the sinking of the vital power have got beyond a certain point, tracheotomy will not, in that case, rescue him. It is bad and foolish practice to wait, and try other methods, and postpone the operation as a *last resource*, when the circulation is evidently loaded with unarterialized blood. In my own case I should choose to be operated on early; the moment that I found early blood-letting was not *telling* upon the local distress, and that any shade of duskiness became perceptible in the skin: just as I should choose to be operated on at once for strangulated hernia, after one fair attempt had been made by a skilful hand to return the

bowel; without waiting till inflammation set in, or had been *caused* by the taxis. On the other hand, if you do not see your patient until his powers are nearly exhausted, do not abstain from the operation even though you may feel convinced that it will be unsuccessful; for if it do not save life, it will disarm death of its agony. A patient will lie sometimes for hours, painfully labouring for breath in deep and strong catches, at considerable intervals from each other: in fact, he is just in the condition of a man with a cord round his neck, not pulled quite tight enough to suffocate him at once. Besides, it is not always easy to say whether the period of possible recovery is yet gone by. I had a female patient in the hospital who had suffered one or two attacks of frightful dyspnœa, in which the main difficulty was referred to the larynx; but she had rallied from them before any steps could be taken for performing tracheotomy. On the next occasion, however, the seizure was so sudden and rapid, that although Mr. Arnott was luckily in the hospital at the time, the woman was, to all appearance, dead, before he could be found and brought to her bedside. Respiration had entirely ceased. This quietude of the larynx rendered the operation more easy. Mr. Arnott speedily made an opening into the trachea; some air was blown in through the aperture, and then pressed out again; and presently the natural respiration was renewed. The woman recovered; the orifice healed up, and she left the hospital. Three or four months afterwards word was brought that she had died at her own home after a short attack, and when there was no one at hand to open her windpipe. We got permission to examine the body, and found a large ulcer in the trachea, near the larynx; which ulcer by its irritation had occasioned, as we presumed, the spasmodic closure of the glottis. A preparation exhibiting the diseased parts is on the table before you. You see that there was enlargement of the thyreoid gland. This had probably nothing to do with the symptoms. There was also a large ulcer in the left bronchus.

Mr. Goodeve, surgeon to the Clifton Dispensary, operated on a patient in whom "no pulse could be found at the wrist; his face was suffused with blood, and his lips livid; and it was hard to say whether he breathed or not:" yet he recovered.

It so happens that there is at present (December, 1838) under Dr. Wilson's care, in the hospital, a woman named Slack, who was rescued when almost *in articulo mortis*, by the same expedient. She had chronic disease of the larynx; but a sudden aggravation of the symptoms occurred; she became stupid and comatose, her countenance was cadaverous, her skin covered with a cold clammy sweat, and her breathing, which had been stridulous and laryngeal, had almost, if not quite, stopped. She was making short, gasping efforts to respire, not oftener than twice in a minute. Her pulse was intermittent, and extremely feeble. In this conjuncture the house-surgeon (Dr. William Merriman) made a small incision in the skin over the cricoid cartilage, and then thrust a large trocar into the windpipe. Air rushed through the opening, the respiration returned, the pulse revived, and the stupor passed away. This happened on the 10th of October. She is still in the ward; the aperture has closed up: and though she is not well, she is *living*.

What is the reason, you may ask, of these different and inconsistent results? How is it that tracheotomy shall reanimate one patient, whose last breath, but for its help, was already drawn, who was already motionless in apparent death; and yet shall fail to save another patient, who is still alive, and sensible of his danger, and struggling with his disease? The difference depends, I make no doubt, upon the time that elapses between the commencement of extreme dyspnœa, and the performance of the operation: upon the slow or the speedy completion of the strangling process. And this, again, obviously depends upon the manner and degree in which the passage is narrowed. When the obstruction, though considerable, is incomplete, and does not rapidly augment, the respiration continues to be performed, however imperfectly. Meanwhile the brain gets oppressed, the circulation tends to stagnate, and above all, the lungs become gorged with black blood, and clogged up by effusion into their cells and substance. Secondary causes of apnœa are thus established, which do not cease when the primary cause is at length removed, by the unbarring of the main channel for the admission of air. Whereas when the access of the atmosphere is suddenly or soon shut out, the lungs are not thus mortally injured, but remain capable of resuming their functions when they are again supplied with air.

Tracheotomy, then, will be the most likely to succeed, while the patient is still lively and strong: and after that, the chance of success will be worse in those cases in

which the apnœa has been *slow* in its progress, than in those in which it has been *rapid*. I repeat that, in threatening circumstances, the operation should be done *early*; but that it should not be withheld, through despair, at *any* period of the disease.

The effect produced upon the condition of the patient by the timely formation of an artificial glottis, is very striking. The moment that the scalpel penetrates the rings of the trachea, air begins to hiss through the incision; and when a fair opening is established, and a full inspiration is made through the wound, several forcible expirations generally succeed, whereby a considerable quantity of mucus is expelled, which could not pass the contracted aperture of the natural glottis. Then the breathing soon becomes easy, the anxiety and distress are followed by a perfect calm, and usually the exhausted sufferer sinks into a tranquil slumber. This sleep is apt to be from time to time interrupted by the clogging up of the orifice with frothy mucus. It is requisite that some intelligent person should remain by the patient, to assist him in these emergencies, or he may still be throttled, notwithstanding the apparent prosperity of the operation.

When a sufficient hole has been made in the instrument of the voice, below the glottis, the voice of course becomes extinct, or nearly so; and the patient is as unable to utter a cough, as he is to use vocal language. Now this it is of some importance to notice, for he often wants to cough; in order to clear the air-passages of mucus, or of blood, by which they may be embarrassed; and he may be helped to do so, or taught to help himself. First he should draw in a full breath, and then stop the orifice for a moment with his finger, while he makes the expiratory effort. And as the parts within the larynx recover, the patient, by a similar manœuvre, may enable himself to speak aloud.

As actual examples are more interesting and often more instructive than an abstract of results, I will tell you in a summary manner the history of a case of laryngitis, which occurred in one of my hospital patients, in the latter part of the year 1832. He was an old man, about sixty. His name was Kent. He was brought to the hospital bloated with anasarca, which was most conspicuous in his legs and thighs. His breathing was laborious and difficult, and attended with a wheezing noise, audible at some distance. He could not lie down: he had a hard, but not full pulse. The dropsical swelling had come on suddenly five or six days before; and in the outset his face (he said) was so puffed up that he could scarcely see. He had been bled to the amount of a pint and a half, according to his own account, on the previous evening. I had a vein opened immediately, and twenty-four ounces of blood were drawn; and eight ounces more were taken from the chest by cupping. He was thoroughly purged with calomel and senna. The bleeding gave him very little relief, so far as the respiration was concerned; but the next morning the anasarca had totally disappeared. I found him sitting up in bed, breathing with much effort, and with a loud stridulous noise, which accompanied both inspiration and expiration. He referred all his uneasiness to two points; one of these was the larynx, the other the ensiform cartilage. He swallowed with great pain and difficulty; and every attempt to swallow excited a fit of choking cough. There was no morbid appearance visible in the fauces; every part of his chest sounded well on percussion, and the murmur of healthy respiration could everywhere be heard in his lungs, almost drowned, however, in the louder laryngeal noise. As his strength was entire I had him again cupped to twelve ounces, at the back of the neck; and prescribed three grains of calomel every three hours. He also inhaled the steam of hot water.

Upon visiting him again the same evening, I found the dyspnœa increased. Each act of respiration was attended with a loud croupy noise. His countenance was beginning to be anxious and ghastly. He was restless; and his pulse was less firm. Being now firmly convinced that the operation of tracheotomy was the only thing that could save him, and that it could not safely be delayed, I sent to request that Sir Charles Bell would come and perform it. By the time he arrived the restlessness had increased. The patient was shifting perpetually from one side of the bed to the other, as if seeking some new point of support; his face had become pale; and his lips were turning livid. He spoke with sudden, and as it were convulsive efforts; stating earnestly how thankful he should be to have the obstacle to his breathing removed; and pointing to the larynx as the seat of his distress.

The ordinary operation, under such circumstances, is by no means an easy one to perform. Its difficulties were well exemplified in this patient. In the first place he was sitting up; he could not bear to be placed in the recumbent position. Then the dyspnoea caused him instinctively to elevate his shoulders, and sternum, and clavicle, to the utmost, so that the trachea was sunk deeply into the thorax; and the larynx was in constant and rapid movement up and down with a plunging motion, like that of the piston of a steam engine. Sir Charles, after some trouble, succeeded in cutting out a piece of the cartilage: for a mere slit did not suffice: it closed tightly during every inspiration, although it was open enough during expiration. At length, when the air was freely admitted, the breathing became gradually easy. I shall never forget the whole spectacle: there sat the poor man gasping and fighting for breath; his face covered with sweat, and wearing the most anxious expression. By and by what I have called an artificial glottis is opened for him; and presently afterwards, though half a dozen candles (as Sir Charles has himself painted the scene) are held close to his face, to throw light upon the wound, and though the surgeons, their hands smeared with blood, are still busy about his throat, making arrangements to ensure the patency of the orifice, the patient falls fast asleep. It was necessary to place an assistant behind him to prevent his head from nodding forwards, and deranging the apparatus in the wound. Nothing can express more strongly than this fact, the great distress and fatigue which had previously existed, and the perfect relief afforded by the operation.

This man ultimately got quite well: and he has since shown himself occasionally at the hospital, in excellent health. There were two or three points about the case which I am unwilling to pass over without notice. It was evident, after the opening was made in his windpipe, that he still breathed in part through the rima glottidis also; for the stridulous sound did not wholly cease. The aperture was formed as low as the circumstances of the case appeared to permit: the tube was perforated in the membranous space between the thyroïd and the cricoid cartilages. Strictly speaking, *laryngotomy* was the operation performed. I do not enter into the consideration of the best place for making the opening: that point you will be taught by the professor of surgery: but it was observed in the case in question, that the slightest touch of the irritable mucous membrane, with a hook or a probe—especially if the touching instrument were turned *upwards* towards the glottis—produced a fit of coughing, and a paroxysm of still more laborious breathing. For some days after the operation, it was noticed that a part of whatever liquid he swallowed *appeared immediately at the wound*. Now this proved as plainly as if we could have seen the parts, that the epiglottis was thickened, and erected, and incapable of performing its protective function to the larynx: and it accounted for the paroxysm of choking cough produced by each act of deglutition. At first the lining membrane of the larynx and trachea was so irritable, that the patient could not bear to have a metallic tube inserted; and an ingenious contrivance was adopted for keeping the orifice from being covered over by the lips of the wound. They were held apart by two bent wires, which were tied together at the back of his neck. After twenty-four hours had elapsed, the irritability of the mucous membrane had so far abated that he was able to breathe through a canula.

There cannot be a doubt that this man was snatched from the very jaws of death by the intervention of the surgeon. A function indispensable to life was nearly suspended; and a substitute for the faulty organ was provided by art, until the interrupting cause was removed. Scarcely a year passes over our heads without the occurrence of one or two such events in the hospital. When lecturing upon this subject last season, I was able to show you a female patient whose life had been saved in a similar way. And there is now also (December 11, 1839), in Pepys' ward, a rescued man, with the tube still in his windpipe. The operation was done on the spur of necessity by Mr. Tones, the present house-surgeon, with a trocar. The patient, who was previously in a state of extreme distress, said, in a faint whisper, as soon as the opening was effected, "It's all right now."

He had been exposed to rain and cold about a week before; and had suffered pain and tenderness of the larynx. Prior to his admission he had been bled, and *sali-vated*, and had a *blister on the throat, which embarrassed the operation*.

Within the last eight years the operation of tracheotomy has been performed in

the Middlesex Hospital fourteen times. Seven of the patients recovered; seven died. In two of the seven fatal cases, the condition of the patients was hopeless at the time of the operation. In four at least of the five others much relief from suffering was afforded by it, and life apparently prolonged. One of the patients was a young child; the opening was made by a trocar: much blood got into the air-passages, and the child, which seemed to be sinking previously, died within the hour. Life might, I think, have been preserved in this case, by a *timely* operation, properly done. For a trachea so small, the scalpel is preferable to the trocar. But in the adult subject I have seen the larynx penetrated so neatly, easily, and speedily, both by Mr. Arnott and by Mr. Shaw, with a small curved trocar, that I am persuaded of the general superiority of that method over the common operation with the knife or scissors. If this be granted, the fit place for the opening must be the membranous space between the thyreoid and the cricoid cartilages. The superjacent skin is first nicked with a scalpel: the larynx is then fixed for a moment by the operator's left hand, while with his right he thrusts the instrument steadily inwards and downwards.

I have said nothing hitherto about the use of mercury in this acute disorder, because I hold it to be of very secondary importance, and because I have been anxious not to divert your attention from the two great practical points, *bleeding* and *tracheotomy*. Mercury may very fitly be given in those cases and circumstances in which blood-letting appears proper; but we cannot depend upon it: we cannot reckon upon its influencing the system *in time*; nor upon any marked improvement of the symptoms when it does produce its specific effects. After the operation it is for the most part unnecessary.

Nor do I recommend the employment of tartar emetic, powerful as that drug is known to be in subduing inflammation of the mucous tissues. In the swollen and unpliant state of the epiglottis it would not be prudent to excite, or to hazard, vomiting. The contents of the stomach passing upwards would be apt to enter the unprotected larynx, and to cause hurtful, distressing, and even perilous attacks of suffocative cough.

In the examination of fatal cases, sometimes the thickened membrane forming the edges of the rima glottidis is found covered with viscid mucus, which had formed an additional impediment to the passage of air towards and from the lungs: sometimes pus is discovered, lying in the sacculi laryngis, or scattered among the cartilages and surrounding muscles: and sometimes the chief morbid condition is the infiltration of the submucous areolar tissue. The effect in all cases is the same, that of closing up, wholly or partially, the narrow fissure between the arytenoid cartilages. The state of the epiglottis I have several times described.

This very serious disease is a disease of adult age: it is not often known to occur in children. They again are almost exclusively liable to *croup*: and *cynanche laryngea* has been called the croup of adults. But as the part occupied by croup, and the event of the inflammation, are both different from those of laryngitis, this name, croup of adults, is objectionable. I may remark, however, that sometimes in true croup, the inflammation, besides specially affecting the membrane of the trachea, extends to that of the larynx also.

The main exciting *cause* of laryngitis is exposure to cold, or to cold and wet. My hospital patient, Kent, was a seller of small wares in the streets, and must therefore have been habitually in the way of such causes. The first attack of the disease in Sir J. M. Hayes was brought on by exposure at an open window to the night air for some time, while he was undressed, and in a state of profuse perspiration, with a strong breeze blowing upon him. Dr. Craigie states that young persons from tropical climates, from the West Indies for example, are apt to be attacked by laryngitis soon after their arrival in Europe.

The disease is liable to be produced also by mechanical violence, or chemical injury done to the larynx. It has been caused, on many occasions, in children, by their attempting to swallow boiling water from the spout of a tea-kettle; and life has been saved in such cases by the performance of tracheotomy. The mineral acids, taken as poisons, have excited the disease. Fatal laryngitis has followed the incautious application of ammonia to the nostrils, in cases of hysteria, and of suspended animation: and I once knew a man nearly killed by the inhalation of the fumes evolved from cinnabar thrown upon a hot iron, in what is called *fumigation* of the throat for vene-

real ulceration of that part. I am afraid that I must confess also to have once seen acute laryngitis produced by the bungling attempt of a young surgeon to introduce the stomach pump, in a case of poisoning.

In all these cases the laryngitis is primitive. But laryngeal inflammation, and especially laryngeal *œdema*, not unfrequently take place, and prove suddenly fatal, in the course of other diseases. I have apprised you that in cynanche tonsillaris, the inflammation sometimes steals onward to the larynx. I have seen two or three cases of erysipelas of the head, attended, as it almost always is, with sore throat, wherein death took place suddenly and unexpectedly, and where the epiglottis, and the edges of the fissure of the glottis, were found to be *œdematous*: the inflammation of the throat had extended to the areolar tissue beneath the mucous membrane of those parts, and had led to the effusion of serous fluid there. The very same thing is apt to happen in other forms of exanthematous disease attended with sore throat, and especially in small-pox, measles, and scarlet fever. I have known a similar condition of inflammatory *œdema* arise from a *mercurial* sore throat, in a broken-down constitution. In these cases the laryngeal affection is consecutive, and secondary; and in all of them the great remedy is the formation of a sufficient aperture beneath the obstructed glottis. In all of them also the essential symptoms, warranting and demanding the operation of tracheotomy, are the same.

Since the foregoing remarks were delivered and published, this subject has been brought before the profession by Dr. Budd, in a paper read at one of the meetings of the Medical and Chirurgical Society, in 1847. He relates several cases, and refers to others, which justify the belief that acute *œdematous* inflammation of the larynx, is connected more frequently than had hitherto been supposed with erysipelas. That disorder, as I shall hereafter show you, is apt to be propagated from person to person by infection; and the effects of the poison, which thus excites the disease by contaminating the blood, are sometimes displayed first of all in the fauces and their vicinity. Now these effects, redness and thickening of the epiglottis, and of the lips of the glottis, with the effusion of sero-purulent fluid in the submucous areolar tissue—sometimes prove fatal, producing death by apnoea, before the erysipelatous inflammation has time to spread far, or to declare its true character by developing itself upon any part of the external skin.

FIG. 34.

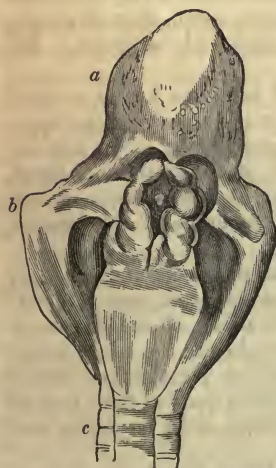


FIG. 35.



FIG. 36.

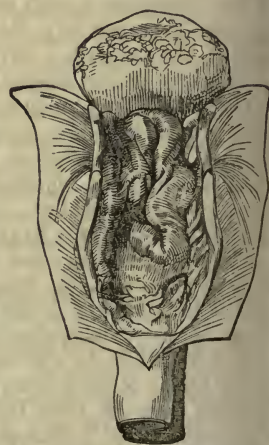


FIG. 34.—*œdema* of the glottis. *a*. Tongue; *b*. Mouth of the larynx; *c*. Trachea. From a specimen in the cabinet of Dr. Gross.

FIG. 35.—*œdema* Glottidis.

FIG. 36.—Acute *œdema* Glottidis; exposed from behind.

A distinction has been made between *laryngitis* and *œdema of the glottis*; and it is a just and real distinction. *œdema* of the loose areolar tissue subjacent to the mucous membrane of the glottis is indeed one common consequence of inflammation of

that membrane: but it may occur independently of inflammation. The lips of the glottis become tumid and *dropsical*, sometimes (as I have just pointed out) in consequence of a low inflammatory action in the throat, but sometimes also from obstruction of the veins leading from that part. When laryngeal dyspnoea accompanies aneurism of the thoracic aorta, it may, in some instances, result from local dropsy thus produced; and then tracheotomy is fully justifiable, and indeed demanded.

[In a very able paper on œdematous laryngitis, and its treatment, by Dr. Gurdon Buck, of New York, contained in the 1st vol. of the Transactions of the American Medical Association,¹ it is remarked that "the question of diagnosis in this disease is one of vital importance. Without stopping to notice the distinctive symptoms which have been generally regarded as characteristic of this disease, or those of other diseases that are most likely to be mistaken for it, I beg leave to insist upon one sign which is strictly pathognomonic, and does not appear to have been sufficiently appreciated. I refer to the swelling of the epiglottis as ascertained by the touch. The discovery of it, according to Bayle (*Dict. des Scien. Med.*, t. xviii, p. 507), is due to M. Thuillier, who proposed it in a thesis sustained before the Faculty of Medicine in Paris, in 1815. The value of this sign will be admitted if we consider how frequent the swelling of the epiglottis coexists with that of the glottis. Bayle (*loc. cit.*), who dissected more than seventeen cases of this disease, says: 'the epiglottis is rarely intact, often it is very much swollen at its edges.' Ryland says (*Diseases of Larynx and Trachea, Philada. Ed.*, p. 48): 'The œdema is seldom confined to these localities, but extends to the base and lateral edges of the epiglottis.'

"Among seventeen cases collected from different sources, and in which the condition of the epiglottis was ascertained, either by dissection after death, or by the touch or inspection during life, swelling was found in fifteen. Of the eight cases reported in this paper, the epiglottis was found swollen in seven, and in the remaining one there was no evidence that it was not swollen.

"The swelling takes place either at the margin, on one or both sides of the median line, or on the lingual surface of the epiglottis at its base, filling up one or both depressions between it and the tongue, and obliterating the central glosso-epiglottic frænum. It conveys to the touch the sensation of a soft pulpy body, easily recognised and distinguished from the stiff rigid swelling of these parts in membranous laryngitis.

"The facility of ascertaining the condition of the epiglottis with the end of the forefinger, not only by placing it in contact with its anterior surface, but by passing over its upper edge, and applying it upon its posterior surface, has been already noticed. To test this question still further, the experiment has been repeated in at least twelve individuals, and in all with success, though not with equal facility. In some, these parts were easier of access than in others, but in none did the experiment fail. In the exceptional cases, where the epiglottis is not found swollen, the edges of the glottis may be brought more within reach by pressing up the os hyoides with one hand, applied externally over it, and acting from below upwards, while the forefinger of the other hand is introduced as directed into the mouth. Should this not accomplish the object, the fore and middle fingers may be thrust far back into the pharynx, as is required for the removal of a foreign body lodged in the throat."—C.]

The main practical difference between mere œdema glottidis and acute laryngitis, is this: that in the former, there being no fever or inflammation, blood-letting is not requisite: and the operation of tracheotomy becomes almost the sole resource to which, in the extremity of danger, we can look for help. Mere œdema glottidis is seldomer attended with dysphagia too, than is laryngitis; yet if the epiglottis be involved in the œdematous swelling, and unable to shut over the glottis, the act of swallowing will be followed by strangling cough, and increased dyspnoea.

Mr. Busk has recommended a peculiar mode of treatment in these cases; whereby he believes the lives of two patients to have been saved, in the Seaman's Hospital on board the Dreadnought. The treatment consisted in making numerous minute punc-

¹ See, also, a further paper by Dr. Buck, on the same subject, in the 4th volume of the Transactions.

tures, with a sharp-pointed bistoury, in the back of the tongue, in the uvula, and in the pharynx. The operation was repeated every half-hour for two or three hours, and the parts were gargled in the interim with warm water. A great discharge of serum took place, and the relief was sudden and decided. Mr. Busk is of opinion that this proceeding would often preclude the necessity of laryngotomy.

[This plan of treatment was first proposed in cases of œdematous laryngitis by M. Lisfranc, but was entirely lost sight of until recently. Dr. G. Buck, of New York, in the paper already referred to, has called attention to the importance of free scarifications of the œdematous edges of the glottis, as well as of the epiglottis, as a means of affording effectual relief from the distress and danger attendant upon the serous infiltration of these parts, and has devised a knife for the more ready performance of such scarifications. For an account of his manner of performing the operation—the difficulties to be encountered in certain cases, and the history of a series of cases in which it was performed by him, we must refer to the papers indicated.—C.]

Besides the affections which I have now described or referred to, the larynx is liable to *chronic* disease: to chronic *inflammation*; chronic *thickening* of the membrane; slow *ulceration*; necrosis of its cartilages. Chronic inflammation and ulceration of that part is very common in *consumptive* patients. It is attended first with hoarseness, then with aphonia, a barking or stridulous cough, and all the melancholy accompaniments of tubercular phthisis. There has accordingly been a species of phthisis spoken of as *phthisis laryngea*. But in most, if not in all cases, this laryngeal affection is only a part of the complaint under which the patient labours; and what I have further to observe respecting it, I shall postpone until we come to the consideration of tubercular consumption.

Again, the membrane lining the laryngeal cartilages is not unfrequently thickened and ulcerated in *secondary syphilis*: giving rise to a hoarse croaking voice, and a noisy and painful breathing. In such cases, or in chronic thickening of the same parts from common inflammation, you may do great good by *gently* introducing mercury into the system, until the gums rise. I have again and again seen the uneasiness about the throat, the noisy respiration, the rough or whispering voice, all cease, as if by enchantment, so soon as the specific influence of the mercury became manifest. There was a woman who used to apply at the Middlesex Hospital for an affection of this kind: whether it was syphilitic or not I could not well determine, but she lost it under the employment of mercury, two or three times: the complaint returning again after the interval of a few months, upon the reapplication of some irritating cause. In another female patient, who was long under my care in the hospital with similar symptoms, everything failed to give permanent relief, till I began to leech the neighbourhood of the larynx repeatedly. She had four leeches applied, I think, every night, and then every other night, for a fortnight or three weeks; the hoarseness and difficulty of respiration gradually diminishing all the time, until at length the perfect use of the instrument of the voice was restored. In these cases, while using local depletion, or mercury, it is often necessary to uphold the strength of the patient by nourishing but unstimulating diet: and it is always expedient that the organ should be kept, as much as possible, in a state of repose.

It is said that a little practice will enable a person to pass his finger into a patient's throat, and to familiarize his sense of touch with the ordinary condition of the upper part of the respiratory apparatus, so as to enable him to detect swelling, or irregularity, or thickening about the chink of the glottis. And great advantage is sometimes obtained from applying remedies directly to the diseased or irritable part. This practice was much followed by the late Mr. Vance, who had been for many years a naval surgeon; and he called it, in naval phrase, *swabbing* the affected organ. A small piece of sponge, secured with a string, or fastened to the end of a finger of a glove, is dipped in a strong solution of nitrate of silver, and then carried down into the throat, as far as that spasmodic state of the muscles which the attempt induces will permit, and pressed downwards against the superior surface of the larynx. I believe other stimulating applications are sometimes employed in place of the nitrate of silver. Now of this method of cure I do not know much, except by report. I have heard that many cases of chronic hoarseness and cough have speedily been

cured by it. But I have more than once had what seemed satisfactory evidence of the beneficial effect of this expedient. The man Kent, whose case I have related, gradually regained the power of easy breathing through the natural passage; and the opening, which Sir Charles Bell had made, closed up perfectly. About a week after this took place, he began again to respire with a wheeze almost as audible as that which existed at the time of his admission: and to speak in a hoarse voice; and a night or two after the return of the wheezing, he had a paroxysm of extreme dyspnoea. I began to be afraid that the whole process of laryngotomy and the metallic tube would be again requisite. However, I got Sir Charles Bell to examine the interior of the throat, and we agreed that it would be advisable to swab the epiglottis and upper part of the air-passages with a strong solution of lunar caustic. For he had no fever, and we thought it probable that the membrane might have been left lax, and in a state to be benefited by astringents. Sir Charles applied the sponge with very little difficulty; and the next day the breathing was greatly improved, and the hoarseness almost gone: and he never had, from that time, any recurrence of troublesome dyspnoea.

Mr. Arnott has on some occasions, at my request, swabbed the upper part of the larynx for intractable hoarseness and aphonia; but with no good, nor any bad consequence.

The lining membrane of the larynx is liable also to warty growths, which impede the entrance and exit of air, and ultimately destroy life. There are several examples of that kind on record. I extract the following from my note book:—

George Tenon la Font, aged 11, admitted March 4, 1828. He speaks in a whisper; complains of difficult breathing, and of cough. Inspires with a loud wheeze. Coughs with a sort of whistling sound, as through a narrow tube. The cough is most troublesome at night. Expectoration mucous, and inconsiderable in quantity.

Has been ill, in this way, all the winter—having had whooping-cough in the preceding autumn. There are marks of cupping on his throat. Little can be heard in the chest, the loud wheeze of his respiration obscuring all other sounds. In about a fortnight his gums were brought under the influence of mercury. No perceptible improvement ensued. A careful examination was again made of the thorax, and the conclusion arrived at was, that the obstacle to his respiration lay in the larynx, or upper part of the trachea, and that the lungs themselves were not concerned. After this, a blister to the throat, a seton near the thyreoid cartilage, small doses of ipecacuanha, emetics, and iodine were successively tried—but in vain. Towards the end of the month he began to suffer, occasionally, very violent and apparently spasmodic attacks of extreme dyspnoea. He died, during the night, two months after his admission. For some days before, he had been manifestly worse than usual, was more feeble, wandered somewhat, and complained that his vision was imperfect. No noticeable increase had taken place in the difficulty of breathing, except during the paroxysms of aggravation already mentioned. His death was sudden, and probably took place in one of these paroxysms.

When the body was examined, the lungs were found sound as to structure, but copiously infiltrated, especially on the left side of the thorax, with serous fluid. At the very top of the larynx, involving the base of the epiglottis and the vocal cords, was a considerable warty growth, closing the rima glottidis almost entirely. The excrescences sprang chiefly from one continuous base, and branched out precisely after the manner of what is vulgarly called a seedy wart. There were, however, several distinct smaller growths or warts. The main excrescence, having several heads, passed upwards from and through the rima, and so came to act partly as a valve during inspiration, which was always sensibly more difficult than expiration.

Ought tracheotomy to have been performed in this case? I now think so. But supposing it to have been done, and to have been successful, the boy would have been under the necessity of breathing through an artificial tube for the remainder of his life. A private patient of mine wore such a tube for three years and four months; till he died of consumption.

In the 38th volume of the *Medico-Chirurgical Transactions*, Dr. Barker has recorded a case very closely resembling that which I have just detailed to you.

There are two excellent specimens of warty growths in the larynx, upon the table before you.

I might have referred, when speaking of chronic enlargement of the tonsils, in the last lecture, to the case of a little boy at present under the care of one of my colleagues. He was brought to the hospital on account of great dyspnœa, and a hissing respiration, produced apparently by two enormous tonsils. With some difficulty, (arising from his unmanageable age) a large part of one of the tonsils was cut off with scissors: but after the operation (either from some fresh swelling of the parts, or from pressure made by the remaining tonsil, which grew downwards, I understand, into the throat), his difficulty of breathing became extreme; and it was thought necessary to perform tracheotomy, which afforded him signal relief. He breathed for some time through the artificial opening in his windpipe. At length the other tonsil was partly removed: and the child is now well, and about to be discharged.

LECTURE XLVI.

Cynanche Trachealis; Symptoms; Pathology, Prognosis; Treatment. Diphtheritis. Child-crowing, or spurious croup.

I PROCEED this afternoon to another of Dr. Cullen's species of cynanche; the last that I propose to consider in this part of the course: viz., *cynanche trachealis* — *tracheitis* — *croup*.

The essence of this complaint is violent inflammation, affecting the mucous membrane of that portion of the air passages which lies between the laryngeal cartilages and the primary bronchi: in one word, of the trachea, or *windpipe*. This is the genuine seat of the disease: but the inflammation sometimes ascends into the larynx; and not unfrequently it dives into the bronchi and into their ramifications.

[In perhaps the majority of cases, the inflammation in croup commences in the mucous membrane of the larynx, and from thence extends into the trachea — the disease is therefore, strictly speaking, a *laryngeo-tracheitis*. In many instances, however, we have reason to believe that the inflammation commences in the bronchi, and from thence extends to the trachea; but there are we suspect, very few cases indeed, if any, in which the disease is confined to the trachea. In cases of croup that have terminated rapidly in death, the inflammation and pseudo-membranous exudation which form so striking a feature in this disease are found only in the larynx and upper portion of the trachea — when death occurs at a later period the exudation is often present in the trachea alone, or in the trachea and bronchi; it is never found to exist in the latter tubes alone. It has been attempted to be shown by Jurin and others, that, in the ordinary form of croup, the disease is, in its first stages at least, simply a tracheitis, and that in the more violent and rapid form, (suffocating croup), the inflammation is confined to the larynx. Although this is not strictly true, as is proved by the result of numerous dissections, yet our observations have shown us, that in cases marked by symptoms of great violence, which are sudden in their onset and rapid in their progress, the indications of inflammation are to a much greater extent, and the pseudo-membranous exudation more copious about the larynx, glottis, and upper portion of the trachea, than in cases in which the disease succeeds to bronchitis, runs a more protracted course, and is attended by symptoms of less violence. For a more detailed account of the pathology of croup, the reader is referred to the Editor's Treatise on Diseases of Children. — C.]

Cullen makes no distinction between cynanche trachealis and cynanche laryngea. Yet they are separated from each other by very definite boundaries. They differ in anatomical position: they differ in gravity. Both indeed are serious diseases; but croup is the more serious, because it seldom admits of that mechanical relief which, when rendered in time, deprives cynanche laryngea of its dangerous character. The

two disorders differ also in respect to the period of life at which they occur. Idiopathic laryngitis is seldom met with except in adults; croup seldom after the age of puberty. Cynanche trachealis is indeed a very remarkable disease, for it exhibits an event of inflammation which does not usually belong to that process when it affects the *mucous* tissues. In this too it is unlike laryngitis.

[In a very excellent monograph on *The Pathology of Infantile Laryngo-Tracheitis, or Croup*, Dr. E. R. Peaslee, of New York, has very clearly shown that the idea of the inflammation in croup having a peculiar or specific character is one altogether erroneous. That the disease is simply a laryngo-tracheitis, and as such, stands in intimate relationship to, or rather, is identical with, this form of inflammation, whether it occur in the adult or infant—whether it run its course to a favourable or unfavourable termination, or with or without the production of a false membrane. On this particular point the general conclusions of the author are as follows:—

“1st. An inflammation of the larynx, extending into the trachea, occurs, offering in its essential nature nothing different from any other case of inflammation of the same parts, either in the infant or the adult. It is generally preceded, in both infants and adults, by congestion and irritation, and therefore, by catarrh.

“2d. An exudation of plasma occurs on the inflamed surface, as in the adult; this being most abundant in the trachea on the posterior wall, for reasons already given.

“3d. This exudation may be disposed of, in at least two ways, provided it is not at once removed, as it generally is, in adults but not in infants, by coughing; reabsorption probably very seldom occurring in this disease, though it is not impossible. *a.* It may become degenerated into pus—purulent matter—and thus, of course, at once be detached, which is the most common result. *b.* It may become organized into false membrane. This is more probable if the blood is rich in fibrin, as in a plethoric child; if there is but little cough, an adult generally expelling it thus, and if time is allowed for its development, less being required in the child than the adult.

“4th. Croup is, therefore, merely a laryngo-tracheitis in infants and children, and offers nothing essentially different from the same inflammation in adults. The exudation in the case of adults is, however, usually at once ejected by coughing, or in the form of purulent matter, while the liability to its organization in infants is greater, though, after all, a comparatively rare result, considering the whole number of cases, for the reasons before mentioned.

“5th. Practically, therefore, as well as pathologically, we cannot say with Bouchut, ‘*without a false membrane, croup does not exist.*’ This membrane never exists till the inflammation—the essential element of the disease as we believe—has preceded, and has produced the exudation of plasma, as before shown. No sooner does the catarrhal irritation merge into inflammation, than the plastic lymph is thrown out, and this inflammation and its accompanying exudation are the elements always present in croup.

“We, therefore, need not for any practical purpose, admit an ‘inflammatory and membranous’ croup, as some writers have done, any more than we should make the same distinction in regard to pleuritis or peritonitis. All croup is inflammatory, at any rate, and a few cases are accompanied by the formation of a false membrane. But the latter should not affect the treatment of the disease as an inflammation, but merely from its mechanical effects, and cannot be predicated in any case till it is actually seen, and this is not possible in most cases in which it is developed at the very onset of the disease. Finally, we would drop the word croup entirely, and use the term *laryngo-tracheitis* instead. In a work on diseases of children, we would call particular attention to the fact that a false membrane is formed in about one-sixth of all the cases of this disease, while in adults, this is of very rare occurrence. But we would not make an accident the distinguishing feature of this disease, any more, than we do in the case of others, nor allow it to enter into either our name or our definition of it.

“As in all other inflammations, so in this, the distinction of ‘*sthenic*,’ and ‘*asthenic*’ is important, both in a pathological and a therapeutical point of view. So far, also, as laryngismus enters into any particular case—and it does into all cases of true laryngitis to some extent—the case is, of course, *spasmodic*, but this term must not be applied to the exclusion of the idea of inflammation. *Genuine spasmodic* croup is a mere laryngismus. There is more or less spasm in all cases of bronchitis, and still

more in whooping-cough, in the latter case in the larynx also, so that infantile laryngitis does not present any peculiarity in this respect."

"Diphtherite, or the 'croup of adults,' differs not *pathologically* from infantile laryngo-tracheitis. Generally, however, it is an asthenic form of laryngo-tracheitis, since it usually attacks persons already debilitated by other diseases."—C.]

I say that croup is peculiarly a disease of early life; and, wherefore I know not, it affects more male children than female. The interval that lies between the two periods of *weaning* and *puberty*, is the time during which its visitation is chiefly to be apprehended. Comparatively few cases of it occur during the first year of infantile life. There are more in the second year than in any other. This, in all probability, is connected with the change that ensues in regard to diet, upon the child's being weaned. Dr. Cheyne, whose experience of croup was very extensive, says, the younger children are when weaned, the more liable are they, *cæteris paribus*, to this malady. From the second year onwards the number of children affected with croup gradually decreases. Of ninety-one instances referred to by Jurin, one only was after the tenth year. But it does occasionally happen subsequently to the period of puberty, and up to the twenty-first year; and even later. A girl of nineteen, one of my hospital patients, died of it. Some of the cases recorded of croup in the adult were probably, in reality, cases of laryngitis.

[We have met with the disease most frequently in children, between ten months and five years old. By numerous writers the occurrence of croup previous to the seventh months has been denied; others, however, declare that they have seen it to occur, as a primary disease, repeatedly, at an earlier period. — C.]

It is curious that inflammation should thus, at different epochs of life, fix itself upon limited portions of the same continuous surface, and lead to consequences so diverse. We are unable to give any account of this.

The distinctions are real and usual: yet it should be stated that the inflammatory disease of the parts thus contiguously proximate, are apt to transgress their ordinary bounds, to run the one into the other, and thereby to lose their distinctive characters.

Cynanche trachealis is frequently preceded by a slighter and more diffused affection of the membrane lining the air-passages. The child has what is popularly called a cold; sneezes, coughs, and is *hoarse*. Now with respect to this last symptom, Dr. Cheyne makes the following practical remark. Hoarseness (he says) in very young children, does not usually attend common catarrh. When noticed in a district where croup is not unfrequent, it ought to put the parents or the medical attendant of the child upon their guard; especially as much depends upon the early treatment of the disorder. With these symptoms the child is feverish and fretful, and does not sleep well. In the course of a day or two the signs peculiar to croup begin to show themselves: they are well stated by Cullen, in his definition of the complaint. "*Cynanche, respiratione difficili, inspiratione strepente, voce rauca, tussi clangosâ, tumore fere nullo in faucibus apparente, deglutitione parum difficili, cum febre synochâ.*" These are the phenomena that characterize croup. Difficulty of breathing; and sonorous inspiration. The last is often almost enough, of itself, to identify the disease. Hoarseness; a gruff voice; sometimes a total loss of the power of vocal speech. A very peculiar and distinctive cough, to which the epithet "brassy" has been justly applied; the noise resembling that which would be occasioned by coughing through a brazen trumpet. This remarkable sound is always easily recognised when it has once been heard. It is a *ringing* cough; and the expiration has a ringing character; and either of these, the cough or expiration, is followed by a loud *crowing* inspiration. Then there is the negative symptom; the absence of any difficulty of swallowing: and with all this, inflammatory fever; a flushed face, a hot skin, a frequent hard pulse, thirst.

However, it is by taking the symptoms collectively, that we judge of the existence of croup, and by the rapid progress of the disease; rather than by any particular or pathognomonic sign. Some of the symptoms may occur, separately, when there is no croup. The brassy or metallic cough, for instance, has been known to accompany some chronic affections of the larynx. Dr. Gregory—the late Edinburgh Professor of Physic—knew a man with a venereal disease of the throat, who coughed so exactly

the cough of croup, that he was admitted into the clinical wards of the infirmary every session for some years, that the students might have the opportunity of *hearing* this peculiar sound. So also the remarkable crowing inspiration may take place, as we shall soon see, without croup.

In the outset, the fever generally runs high; and it is of importance, as respects the diagnosis, to mark the presence or the absence of pyrexia. As the obstruction to the passage of air increases, the blood ceases to be duly arterialized: and then, of course, the skin grows dusky, the pulse feeble and irregular, the extremities cold. The cough, also, as the malady thus goes on from bad to worse, ceases to be loud and clanging, becomes husky, and inaudible at a short distance, and the voice sinks into a whisper; the head is thrown back; the nostrils, in perpetual motion, dilate widely; the face is pale and livid, and sometimes bloated; the pupils often expand. When these indications of sinking have come on, the case usually terminates ill: the bottoms of the feet turn black and hard; drowsiness supervenes; some tossing of the arms perhaps; the breathing becomes gasping and interrupted, and the child dies after an *inspiration*.

In other cases the croupy symptoms make their attack very suddenly. A child shall go to bed apparently well, and in the course of the night have all the worst signs of the disease.

[These will be generally found, however, to be cases of spasmodic laryngitis, and to be unattended with the formation of false membrane in the larynx or trachea.—C.]

And it is observable, that whether the attack be altogether unexpected, or whether it have been preceded by hoarseness, sore throat, and catarrh, it usually comes on *in the night*.

Croup resembles laryngitis in this respect, that it runs its course rapidly: proving fatal sometimes within twenty-four, and often within forty-eight hours. It may, however, continue for five or six days before it terminates, whether death or recovery be the result. Dr. Craigie affirms that it is never protracted beyond the *eleventh* day; the fatal or the favourable issue having always taken place by that time. Life is destroyed, in pure circumscribed tracheitis, by the accumulation in the windpipe of a concrete membrane-like substance, which so frequently attends this disease, and is so peculiar to it, that it is called *the membrane of croup*. In cases of recovery this substance has been expectorated in the form of a nearly perfect tube, representing a cast of the trachea; at other times it is coughed up in flat or tubular fragments. In fatal cases it is found sometimes lying in close contact with the mucous membrane, and sometimes quite detached from it: so that it might have been expelled without much forcing or difficulty, if the child could have sufficiently inflated its lungs, and the requisite muscular power had remained, and spasmodic irritability of the glottis had not opposed.

But, in many instances, this albuminous exudation is not confined to the trachea. It often stretches down to, and enters, the ramifications of the bronchi, and reaches even to their termination in the pulmonary vesicles. Sometimes also it is found clothing the mucous membrane belonging to the laryngeal cartilages. This Dr. Craigie denies. But I show you two specimens in which the false membrane, besides filling the trachea, evidently extends into the larynx. One of these comes from the museum up stairs: the other I have borrowed from the Middlesex Hospital Museum: it is the larynx and trachea of the young woman whom I mentioned just now as having died there of croup, at the age of nineteen. The false membrane reached from the tip of the epiglottis to the bifurcation of the trachea. You see the same thing represented in this excellent plate of Sir R. Carswell's, pictured from nature. Usually the adventitious membrane commences just below the larynx, where it is thin and soft: about the middle of the wind-pipe it is more dense and firm: lower down in the trachea, and in the bronchi, it is generally looser again, pulpy, and broken: it sometimes, I repeat, penetrates to the very air-cells. What are called (absurdly enough) bronchial polypi, branch-like casts of the smaller ramifications of the air-tubes, are then apt to be coughed up. And even when this concrete substance is not formed, we have other evidence, often, of the extension of the inflammation throughout the whole downward course of the membrane.

On the other hand, there are a few cases in which this adventitious membrane is not formed at all; the inner surface of the windpipe is seen to be merely reddened

FIG. 37.



FIG. 38.



FIG. 39.

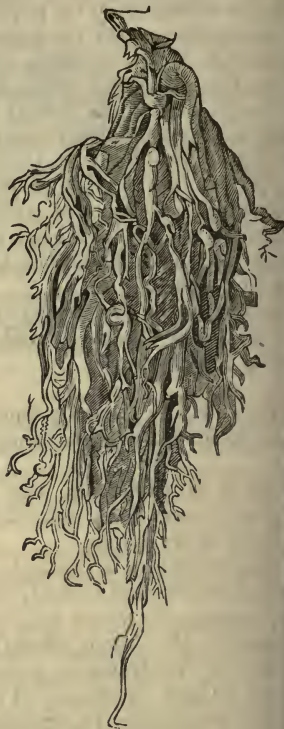


Fig. 37. — False membrane of croup. From a specimen in Dr. Gross' cabinet.

Fig. 38. — Example of false membrane of croup.

Fig. 39. — False membrane of the bronchial tubes.

and tumid, and covered with viscid mucus; or perhaps with a shred or two of concrete albumen here and there.

The difficulty of breathing, and the characteristic sounds that accompany it, depend, in part, no doubt, upon spasmodic contractions of the small muscles of the larynx: for remarkable aggravations of the dyspnoea are apt to occur, and to subside again; and these aggravations are sometimes brought on by sudden causes — by the movements of deglutition, for example.

I shall have to recur to this spasmodic constriction of the glottis; but I may here remark, that because it has not been (and cannot be) *seen*, doubts have been expressed by some about its having anything to do with the dyspnoea. Such doubts seem scarcely reasonable. It is easy, at any time, by an effort of the will, to close the glottis, and to prevent the passage of air to and from the lungs. This is mainly effected by the action of the little muscles that bring together the arytenoid cartilages. But those muscles, like the other muscles concerned in respiration, act also independently of the will, spasmodically therefore, through the reflex function of the spinal cord. And it is by a providential and conservative appointment that they do so act, as janitors, admitting, in the healthy state, the vivifying air, but barring the door against certain hurtful gases, and against solids and liquids which would be injurious to the respiratory apparatus. We know that if a drop of water, or a crumb of bread, or a whiff of carbonic acid gas, gets past the outer defence, the epiglottis, and into the larynx, spasmodic action of the little muscles in question is instantly excited. We cannot see these intruders, and voluntarily resist their entrance, but the unsleeping sentinel is there to guard the passage. We may well conceive, therefore (and I know

not how the supposition can be disproved), that the noisy and difficult respiration of croup may be caused, in part, by spasm.

[As further evidence of the correctness of this remark may be adduced those cases in which all the diagnostic symptoms of croup are suddenly induced by an irritation seated in the stomach, as from indigestible food, &c., and are as suddenly removed the moment the cause of irritation is expelled. A medical friend has informed us, that in one of his own children, symptoms resembling croup are produced by eating fish, and are immediately removed upon the operation of an emetic. — C.]

The presence of the adventitious tubular membrane in the trachea affords a plausible explanation of one of the symptoms observed in these little patients; the tendency they show to throw the head back. The cylinder of membrane is kept open in that position; whereas, if the head were inclined at all towards the chest, the membrane would be bent upon itself, and the passage through it obstructed.

This concrete exudation is often adduced to prove that the mucous membranes may exhibit, under certain circumstances, the phenomena of adhesive inflammation. Similar films sometimes form upon, or are thrown off by, the mucous surfaces of the intestines, and of the uterus. Whether they are to be regarded as essentially identical with the layers of coagulable lymph poured forth in inflammation of the serous and areolar tissues, may be made a question. There are certainly some strong points of distinction between them. The concrete membrane of croup is more brittle, less fibrous, more decidedly albuminous, than the false membranes that cover the inflamed pleura, pericardium, or peritoneum. A still more remarkable difference is this, that it is not *plastic*, in the sense in which that term was formerly explained; it never becomes organized, never connects itself by blood-vessels with the surface from which it proceeds. On the contrary, it is partially detached; and by degrees, if the patient live long enough, it is completely separated from the adjacent parts.

Hypotheses have been framed to account for the limitation of this product of tracheal inflammation to the early periods of life; and for its variation from the usual products of inflammation of the same part. Dr. Stokes thinks that the predominance of the white tissues in young subjects may explain the greater frequency of croup, with its peculiar membrane, in infants. Dr. C. B. Williams starts the very reasonable supposition that the inflammation involves the submucous areolar tissue, which is abundant during youth; and that the natural product of this phlegmonous inflammation transudes readily through the thin, simple, and delicate mucous membrane proper to that age.

The formation of this adventitious membrane, and even its renewal, appears to be sometimes very rapid. I have here a preparation made by the late Dr. Sweatman, illustrative of this. It now belongs to the Middlesex Hospital Museum. Upon a child on the very brink of suffocation from croup, the operation of tracheotomy was performed, at one o'clock in the morning, by Mr. Chevalier. A tubular portion of membrane, of the shape and size of the trachea, was presently forced through the artificial opening. Immediately, the child's respiration became easy, and it fell asleep. In the course of the same morning, Dr. Sweatman was hastily summoned, and arriving at eight o'clock, found the child dead. It had slept six hours, and upwards; and then the distress of the breathing had returned, and was soon fatal. The trachea was found to contain a new tube of lymph, or of concrete albumen. The preparation shows that such a membrane may re-form in that short space of time; namely, in from six to seven hours.

The croup is not contagious; although, like cynanche tonsillaris, and for the same reasons, it is found sometimes existing at the same time, or in quick succession, in more than one child of the same family. Thus two twin children of Dr. Gregory's were seized with croup on the same night. They had both been walking in the evening on the sea-shore during a cold wind. This is in accordance with what Dr. Cheyne has stated, that the attack is almost always nocturnal, and often when the child has during the preceding day been exposed to the weather. It frequently occurs sporadically; but there are places in which the disease appears to be endemic. Dr. Cheyne found it so on the coast of the Frith of Forth. Indeed, the first distinct account of it that we possess was drawn up by Dr. Home, of Edinburgh, in 1765,

from much personal observation of its ravages in Leith and Musselburgh. Cold situations—and damp places, more than such as are merely cold,—are subject to the prevalence of this disease. It is accordingly frequent in the seasons of winter and spring. It is said to be most common near the sea-shore, and in the neighbourhood of large bodies of water generally. It occurs in low, moist, what are called in Scotland *carse* districts, oftener than in upland situations that are more exposed to cold winds. It is more common at Leith than it is in Edinburgh; and in Edinburgh, it is most frequent in the lowest parts of the town. This I learned from Dr. Alison, who, having long been physician to a dispensary there, had had ample means of observing the disease. Canal Street and the Cowgate, both low spots, as some of you may know, have long been famous, or rather infamous, for cases of croup. Towns situated on the banks of rivers have more than the average share of it; and it has been observed to be particularly frequent among the children of washerwomen in such places; and thus evidently connected with exposure to moisture. In towns so situated, it has been known to prevail epidemically after an inundation.

[Of the frequent prevalence of croup as an epidemic, Berge, Canstatt, Fleury, Val-leix, Wunderlich, and others, furnish incontestible evidence. An epidemic of the disease is recorded as having extended over the greater portion of Central Europe during the period between 1805 and 1807, and one of more circumscribed limits by Terrand in his Thesis on Membranous Angina, published in 1827, during which, in a district of very small extent, there occurred no less than sixty cases of croup, all terminating fatally.—C.]

Dr. Allison has made a very curious remark respecting croup. He says that it seems to be often produced by the child's sitting, or sleeping, in a room newly washed: and that he has noticed its frequent occurrence on a Saturday night—the only day in the week on which it is customary for the lower orders in Edinburgh to wash their houses.

[The remarks in the foregoing paragraph are applicable rather to spasmodic laryngitis than to cases in which there is a formation of false membrane.—C.]

Like cynanche tonsillaris, and unlike cynanche parotidæa, the croup is exceedingly apt to recur. Relapses may happen within a few days after apparent recovery; and these are very perilous. But besides this tendency to a renewal of the severer symptoms, the little patients are often affected with cough, and hoarseness, and even with aphonia, for a long time. And while these relics of the acute attack continue, it is easily brought back again. The first seizure is generally, I believe, the worst: but to this rule there are numerous exceptions.

Croup is a disorder which justly excites extreme alarm in the friends and parents of the patient: for the prognosis can never be better than doubtful. It is said that four children out of five attacked by it used to die: but that now, the treatment being better understood than formerly, the number of deaths and the number of recoveries are nearly equal. We judge of the probable issue, in a given case, by the apparent circumstances and progress of the malady. If we could see the interior of the air-tubes, we should know that the chance of escape was small, in proportion as the inflammation, and its albuminous product, descended along the ramifications of the bronchi. But in these little patients, and amid the tracheal noises, it is difficult to ascertain the physical state of the lungs. The prognosis is chiefly to be collected from the general condition of the child. If the distress of breathing seem to remit, and free expectoration to come on, while the strength is yet entire, we venture to hope. On the other hand, we begin to despair when the lips are becoming blue, the skin is losing its heat, the pulse is already feeble and intermitting, and the little patient is drowsy or comatose: in other words, when we perceive the final symptoms of death in the way of apnœa. Some few patients die suddenly and unexpectedly without any previous coma.

The mortality will differ according as the disease is detected early, and treated vigorously,—or otherwise. And with respect to treatment, there is no specific remedy for this, any more than for any other inflammation. We must put in force the general

principles upon which the treatment of inflammation is founded; adapting them, however, to the malady in question by those particular rules which the experience of the best observers has collected for our guidance.

I need scarcely say that when cough and catarrh, and especially hoarseness or loss of voice, are noticed in a young child, he should be narrowly watched, and protected against all circumstances likely to excite or to aggravate inflammation: he should be kept in the house, and put upon farinaceous diet; and the functions of the bowels and of the skin should be attended to.

The three remedies that most require consideration are blood-letting, tartarized antimony, and calomel.

Bleeding is to be unhesitatingly employed when the patient is strong, and plethoric, and seen in the outset of the disease. In judging of its mode, and of its amount, we must recollect that what is no more than a topical bleeding for an adult, becomes equivalent, in its effects upon the system, to general bleeding, when it is used for a very young child. Abstraction of blood by venæsection or cupping in the case of older children, and by leeches in the case of infants, should be practised whenever the symptoms are violent, and there is much fever, and the patient is seen within a few hours after the commencement of the symptoms. The relief that is given by this measure, under such circumstances, is often so decided, that no doubt can remain of its usefulness and propriety.

It is impossible to lay down any fixed rules for the quantity of blood that should be taken in this complaint. Under two years of age, it should not, says Dr. Cheyne, exceed five ounces. I should esteem that a *large* bleeding, at that age. Upon an average, a moderate bleeding will be produced by the application of a couple of leeches to an infant in its first year: and an additional leech may be employed for every additional year: so that six may be put to a child five years old, or eight if he be stout. Dr. Copland estimates the amount of blood which these patients may, with safety, bear, to be somewhat more than an ounce, or as much as an ounce and a half, for every year of their age. Much, however, must depend upon the special circumstances of the case: the quantity of blood extracted by a given number of leeches is less in one instance than in another; and then, of course, the number must be increased. I have been in the habit of recommending that they should be applied at the upper part of the sternum, rather than in front of the windpipe itself: for this reason:—that the pressure which may be necessary to stop the bleeding, or to regulate its quantity, cannot well be borne upon the throat in these cases. Yet if the process be conducted by the practitioner himself, and not left (as sometimes it inevitably must be) to the casual attendants of the sick child, this incidental difficulty may by care be obviated—and then it will be desirable, as Dr. West suggests, that the leeches should draw the blood from the nearest vicinity of the affected membrane.

After one sufficient evacuation of blood, whether by means of the lancet, or of leeches, or of cupping-glasses applied between the shoulders, it will always be right, before repeating it, to ascertain the effects of other measures; such as emetics and purgatives, the beneficial operation of which in this disorder is often very remarkable.

Full vomiting sometimes affords relief so sudden and complete, as to lead to the persuasion that the symptoms had been principally owing to spasm. And even when the disease is unequivocally inflammation—sometimes even late, but particularly in the early part of its course—the effect of a vomit is often very striking. It promotes expectoration; and is not unfrequently followed by the expulsion of shreds of the adventitious membrane. When blood-letting is employed, it should precede the emetic; or, at any rate, it should precede the act of vomiting. Dr. Cheyne recommends that the bleeding should be practised ten minutes after the emetic has been swallowed. The loss of blood assists the operation of the emetic, and lessens the risk (which is not a fanciful one) of injurious congestion of the vessels of the head during the straining efforts of vomiting.

It is better, in Dr. West's opinion, to re-excite from time to time, as circumstances may require, the act of vomiting, than to aim at prolonging a state of nausea and aintness, which might mask the progress of the disorder towards a fatal ending. In corroboration of the result of his own experience, as to the superiority of emetic over

nauseating doses of medicine, he quotes that of M. Valleix, who states, that in thirty-one out of fifty-three cases of true croup, ipecacuan and antimony were employed in full doses, and fifteen of the patients recovered; whereas, of the remaining twenty-two, in whose treatment these drugs were but sparingly resorted to, all died but one. Now the substance best adapted to our purpose is the tartarized antimony. This medicine, as I have often mentioned before, has great power over inflammation of the mucous tissues: and there is one very great advantage belonging to it in cases of croup; namely, that children may be induced to take it without their knowing that they are taking medicine; for the solution of it has little or no taste: whereas the struggling which is often occasioned by the administration of other emetics may be the cause of much inconvenience, and even of much injury, to the patient. It should be dissolved in boiling water, in the proportion of a grain to an ounce; and the cold solution given. A tea or a dessert spoonful may be repeated every quarter of an hour, till some effect is produced.

[By the majority of physicians the tartarized antimony is preferred as an emetic in croup. Some, however, ascribe a peculiar efficacy to the sulphate of copper, while others again prefer the sulphate of zinc. Many of the American practitioners esteem the *Sanguinaria Canadensis*, in infusion, as almost a specific, while a few prefer the *Lobelia Inflata*. Dr. Meigs considers alum in powder, from the speediness of its operation, to form the best emetic in cases of croup, while Dr. Hubbard, of Maine, recommends the turpeth mineral—the yellow sulphate of mercury—as an emetic in this disease, in consequence of its promptness and certainty, and its never producing catharsis, or being followed by the prostration caused by tartar emetic.]

It is upon these latter considerations that the sulphate of copper has been considered the article best adapted to produce emesis in croup. By many of the continental physicians it is held to be superior to all others. It is recommended by Berignier and Trousseau, in two or three grain doses, but by the latter in doses of five grains, to be repeated after a short interval, should the first fail to produce vomiting.

Dr. Hornerkopff, in a paper published in 1855 (*Journ. der Kinderkrankheiten*), states that he had employed the sulphate of copper in 99 cases of croup, of which 77 recovered. He gave the salt in solution, 6 to 8 grains to one ounce of water, of this from a tea to a table spoonful was given, more or less frequently, according to the age of the patient and the extent of emesis produced. He continues the use of the remedy so as to keep up vomiting or decided nausea until convalescence has been fully established. Dr. J. Sumter, of Posen (*Gönsburg Zeitschrift*, 1855), has employed the sulphate of copper very much in the manner recommended by Dr. Hornerkopff, and speaks of it in equally favourable terms.

Dr. Lazinsky (*Schmidt's Jahrbuch*, 1855) gives a decided preference to sulphate of copper in croup. He gives it in solution (two to four or more grains in two and a half ounces of fluid), of which the dose is a tea spoonful every fifteen minutes.—C.]

When vomiting is thus excited on the very first appearance of the symptoms, and before the disease seems thoroughly formed, it sometimes puts it off; so that no other treatment remains necessary beyond the exhibition of some purgative medicine. But when this perfect relief does not ensue upon the operation of the emetic, Dr. Cheyne advises (and this is in conformity with the practice of many other persons) that a powder, consisting of two, three, or four grains of calomel, with two or three grains of James's powder, should be given at short intervals; every two or three hours for example. A dose of castor-oil is to be administered occasionally, to clear the bowels. And another expedient, of great efficacy sometimes, and therefore never to be omitted, is the warm bath. This is often properly resorted to just after the act of vomiting, particularly if any tendency to perspiration be apparent. The temperature of the water should not be lower than 98° Fahrenheit; and the child should remain in the bath for ten minutes at least. When taken out, he should be wiped dry, and put immediately into bed again. The change for the better produced by the bath is sometimes so marked and so speedy, as to strengthen the conclusion that the most distressing of the symptoms had resulted from spasm.

The usual effect of calomel thus frequently repeated is that of causing (not, as in

adults, salivation, but) the discharge of a quantity of green fecal matter, resembling chopped spinach: and when stools of this kind begin to make their appearance, there is often a sensible mitigation of the symptoms. The green colour is a common consequence of mercury given to young children; and will occur, I believe, whatever be the disease, when the full effect of calomel as a purgative is obtained. The green matter has been found, after death, in all the intestines, small as well as large, up to the duodenum. I presume that the colour is owing to some chemical action that takes place between the calomel and the bile. We know that calomel does tinge bile green when mixed with it out of the body. It may be, however, that the calomel provokes a flow of altered bile.

Calomel thus administered, is the purgative that has received the strongest recommendations. Its usefulness appears to have been fully borne out by the test of experience: and the well-known virtue belonging to mercury, of preventing or arresting the effusion of coagulable lymph in other textures, has formed (I conceive) one cogent reason for its adoption in this disorder, of which the chief peril results from the pouring forth of the albuminous part of the blood. But whether mercury really has the same power of controlling adhesive inflammation, when that process is set up in mucous tissues, which are so commonly exempt from it, may be questioned. On the other hand, the effect of full doses of tartar emetic in restraining active inflammation of those tissues is well ascertained. The act of vomiting helps greatly, also, to dislodge from the air-passages, the phlegm and false membranes by which they are obstructed. For these reasons, the remedy is admirably adapted to the early stages of this dangerous malady. The system can be brought to feel its decided influence with much more certainty, and in a much shorter space of time, than that of calomel: and if it fail to make a beneficial impression, it need not long interfere with the mercurial treatment. Let me quote to you the statement of Dr. Cheyne (whose experience of this disease was far ampler than mine has been) respecting the efficacy of tartarized antimony in what he calls the second stage of croup. He recommends that half a grain, dissolved in a table spoonful of water, should be given to a child two or three years old, every half hour, till sickness and vomiting ensue. In two hours after the last effort of vomiting, the same process is to be recommenced; and so repeated while the symptoms require it, and the strength will permit. This mode of treatment was suggested to him by the accidental observation of a particular case, in which it was remarkably successful. From that time he placed his whole reliance on that remedy in the second stage of croup; especially as he had found that blood-letting in that stage only accelerated the death of the patient. He noticed that the cases were *few* in which he had known children survive the second stage, but in *all* of these few, they recovered while using a solution of tartarized antimony. He held that no other medicine was, at that stage of the disorder, entitled to confidence. In short, he declared "that tartar emetic so given as to produce continued nausea, had been his sheet anchor, in the treatment of croup, since the year 1799." This was written in 1801, in a separate work on the pathology of the larynx and bronchi; and Dr. Cheyne recently has affirmed, in the *Cyclopædia of Practical Medicine*, that he still found reason to adhere to the same opinions, and the same practice.

[Dr. Watson makes no mention of the application of nitrate of silver locally to the throat, in cases of croup, and yet the evidence in its favor is of the strongest character. The best means of effecting this application is by wetting a small piece of fine sponge, firmly attached to the end of a portion of whale-bone, properly curved, with a solution of the nitrate of silver in water, and carrying it to the larynx and on the glottis, and then quickly withdrawing it. — C.]

Now, what is good for the second stage, would, *à fortiori*, I think, be likely to save life, if employed during the earlier stage of the disease. I therefore should say, take blood in the very outset, as largely as may seem prudent: then give the tartar emetic solution in the way already described. As soon as it causes vomiting, and pallor, and a sinking of the pulse, stop: and suffer the heart to recover itself. And if, with the rallying circulation, the difficulty of breathing return, have recourse again to the same remedy. The faintness and collapse are sometimes so great as to threaten the extinction of life: the child, with a flying pulse, and a clay-cold surface, seems

gasping its last. When this happens, a few drops of sal volatile, or of brandy, mixed with water, will presently bring the little patient round again. If no ground be gained after two or three repetitions of the emetic treatment, then it may be well to make trial of the calomel plan.

Sometimes the antimony acts severely on the bowels: it may occasionally therefore be necessary to combine with it a small quantity of syrup of poppies, or of laudanum.

[We have been in the habit, for many years, of prescribing immediately after bleeding in those cases in which this was demanded, and when such was not the case, as our first prescription, a combination of one or two, or three grains of calomel, an eighth of a grain of tartar emetic, and from three to five grains of muriate of ammonia, every one, two, or three hours, according to the urgency of the symptoms present, and the age and vigour of the patient, and we have had much cause to be pleased with the effects of this combination. Under its use all the prominent symptoms often disappear with great promptness. — C.]

Blisters are often applied in this disease; but with very questionable propriety. In the outset they are likely to do harm; in the advanced periods they are not likely to do good. If used at all, they should be placed, not on the throat, but across the upper part of the sternum.

[We do not approve of the use of blisters in this disease. If applied, they should only be kept on for a few hours, so as merely to redden the skin. A much better application is a strip of flannel wet with spirits of turpentine, kept on for only a few minutes at a time, and repeated if considered necessary. — C.]

When signs of approaching death have come on—lividity of the lips, coldness of the skin, and a tendency to stupor—the question *will* obtrude itself, whether there may not still be a chance of saving the patient by performing tracheotomy. In the first place you will consider that the operation is much more difficult to execute upon children than upon adults; and is attended with more perplexing hæmorrhage. But there is a greater objection than this to tracheotomy in such cases—an objection which you will have anticipated—namely, the existence of the preternatural membrane; which often extends so far down, that air would not be admitted into the lungs, even if an aperture in the windpipe were made at the lowest possible point. Another consideration, forbidding much hope of success from this expedient at any period of the disease, is that the ramifications of the bronchi and the ultimate air-cells get filled up with serous, or mucous, or puriform matter, or even sometimes with a membranous exudation, whereby suffocation is effected *in the lungs themselves*. The membrane in the trachea, being tubular, does not entirely exclude the air from those organs; but it does not admit it in sufficient quantity. Tracheotomy has been many times practised in this complaint, and with just enough of success to warrant its repetition in cases otherwise hopeless. Two instances in which life was saved by its performance are recorded in the *Medico-Chirurgical Transactions*: the one achieved by Mr. André, and related by Dr. Farre, in the third volume: and the other by Mr. Chevalier, in the sixth volume. These were both apparently desperate cases. Immediate relief followed the operation in both, and the patients recovered perfectly. More recently five examples of tracheotomy in croup, performed in St. George's Hospital, have been brought before the society by Dr. Fuller. In two of these the operation succeeded; in three it failed.

There seem to be just two predicaments in which there is a fair chance that tracheotomy may be useful. They are perhaps rare: yet they have been noticed by several observers. The one is where the preternatural membrane extends but a very little way down the trachea, and is chiefly confined to the larynx: and the other is where there is no preternatural membrane at all, or only a very slight coating in some part of the trachea, the impediment to the breathing having arisen mainly from the thickening of the mucous membrane. And you will observe that an impediment from this cause will always be the greatest at the narrowest part of the canal: and therefore incision of the windpipe in such a case may be expected to bring relief. The effect produced by the tracheotomy in Mr. Chevalier's case was very instructive. Air was

fully inspired through the opening, then a strong cough took place, by which a large quantity of viscid reddish mucus was forced out by the natural channel, through the glottis. It was evident that the child could not expectorate before, simply because it could not sufficiently fill its lungs with air to drive the collected mucus out. Dr. Farre gives a circumstantial account of a case in which the adventitious membrane did not reach more than a finger's breadth below the cricoid cartilage; and the rest of the tube was so free that he was convinced the child's life might have been saved by a timely opening into the trachea. Unfortunately we cannot tell, before death, to what degree or extent the preternatural membrane exists. All that can be said, I think, is, that when dyspnoea and much croup come on suddenly or quickly, the disease is probably limited to the larynx and upper part of the windpipe: but that when the progress of the disorder is slower, and the croupy symptoms are not so well marked, it is more likely that a greater extent of the trachea, below the larynx, participates in the mischief. Our expectations of success from tracheotomy will vary accordingly. It affords a bad chance at the best; but it affords also, in many cases, the *only* chance. For some sound practical suggestions in regard to the proper management of the operation, and to the after treatment which is essential to its prospering, I must refer you to Dr. Fuller's paper.

[The subject of tracheotomy in croup is one of considerable interest. That in many cases it may, when timely performed, save the life of the patient, we have the most unquestionable evidence. In a statement by M. Trousseau of the result of the operation, as performed by himself, and by others, according to his method, in *one hundred and fifty* cases, the patients were saved in *thirty-nine*. In the case of his own child, three weeks old, recently published by Dr. Scoutetten, the operation was performed on the third day of the disease, under circumstances apparently the most desperate, with complete success—the infant recovering in a short time from the effects of the operation, as well as from every symptom of the croup. The class of cases, the period and particular circumstances of the disease in which the operation is most likely to afford relief, are questions of no little importance, and it is probable that much of the want of success, which many practitioners have experienced in performing the operation, is to be attributed to its having been resorted to under improper circumstances. M. Trousseau, whose experience on this subject has been somewhat extensive, presents the following as a summary of the prognosis of tracheotomy in croup:—

"1st. If the commencement of the attack dates several days back, if, consequently, the disease has advanced slowly, whatever may be the extent of the false membranes in the trachea and bronchi, the children either recover, or live at least several days after the operation.

"2d. But if the disease has been very rapid, even although at the time of the operation we ascertain that the false membranes do not extend beyond the larynx, the children die very quickly.

"3d. If previous to the operation, the false membranes have extended to the nares, or if they cover the blistered surfaces; if the child is pale and somewhat bloated, without having taken mercury or been bled, or if he has lost much blood, there is little chance of the operation succeeding.

"4th. If previous to the operation the pulse is moderately frequent, and if, after it, the pulse remains calm, hopes may be entertained.

"5th. It is a bad sign, if, immediately after the operation, the respiration becomes very frequent, without any cough or with very little.

"6th. More boys than girls are cured.

"7th. Children under two, and over six years of age, rarely recover.

"8th. The more deeply the false membranes have extended, the greater, *cæteris paribus*, the danger.

"9th. If the child has been subject to chronic catarrhs, and if he had been suffering from a cold for some time when he was attacked with the croup, tracheotomy is more successful.

"10th. Even when all is going on favourably, very great frequency of respiration is a bad sign.

"11th. The more rapid and energetic the inflammation which attacks the wound

in the trachea, the better are the chances of success—a sudden sinking of the wound is a mortal sign.

"12th. So long as the respiration is silent or the noise is only occasioned by the displacement of mucosity, there is nothing to fear; but when the respiration becomes saw-like (*serratique*), in other words, when it is attended with a sound like that produced by a saw cutting stone, death is certain.

"13th. There is no reason to despair of the patient, even if a pneumonic or pleuritic attack should supervene.

"14th. Agitation and sleeplessness are bad signs.

"15th. Should the wound become covered with false membranes; if, after withdrawing the canula, it remains gaping for a long time, or, if after having become entirely cicatrized it reopens largely, the child is in danger.

"16th. The sooner after the operation the larynx is disembarassed, the sooner may we remove the canula, and the more rapid and certain is the cure.

"17th. If the croup supervened upon rubeola, scarlatina, variola or pertussis, although there is not ordinarily any connection between the malignant angina and these pyrexia, tracheotomy does not succeed.

"18th. If the expectoration becomes mucous and catarrhal by the third day after the operation, the children will recover. If there is no expectoration, or it is serous, or like half-dried portions of gum Arabic, they will die.

"19th. If the patients react vigorously under the injections into the trachea of water or nitrate of silver, and under the sponging out of the trachea, we should not despair, however unfavourable may be the other signs.

"20th. Children attacked with convulsions after the operation die, and the younger the patients and the more blood they have lost before or during the operation, the more often convulsions supervene.

"21st. When, after the tenth day, the drinks pass almost entirely from the pharynx into the larynx and trachea, even if they are readily rejected, the children most generally die.

"22d. The increase of the fever after the fourth day, agitation, sinking of the wound, and dryness of the trachea, frequency of the respiratory movements, and attempts to cough, announce the invasion of pneumonia, which, at first lobular, becomes sometimes pseudo-lobular, and is to be treated by the same means as are usually employed in the pneumonia of children; we should, however, exclude blisters, because they too often become covered with false membranes." (See *Rilliet and Barthez, Traité des Mal. des Enfants*, t. i., 365-7. — C.]

A severe inflammatory disorder of the throat, much more common in some parts of France than it is in this country, and named by M. Bretonneau, of Tours, who first fully described it, *diphtheritis*—is regarded by Dr. West as a variety of croup. Some analogy with that disease it certainly has; but the points of difference are stronger and more essential. It resembles croup, inasmuch as it leads to the production of an adventitious membrane upon a mucous surface. It differs in the position of that membrane, which is seldom formed in the trachea. The affection of the windpipe, when it occurs at all, is secondary: so that the term cynanche trachealis would be quite inappropriate. The parts first and chiefly concerned are the fauces. A whitish or ash-coloured membrane forms upon the pharynx and tonsils, and extends forwards to the soft palate, and into the nostrils, and backwards into the œsophagus, sometimes into the larynx, but seldom into the trachea. Around it, between its fissures, and in spots from which it has been detached, the mucous membrane is seen to be of a deep red, and sometimes of a purplish or claret colour. The submaxillary and cervical glands are liable to swell, the neck in front becomes full and œdematous, and an acrid discharge from the nose is commonly present.

This very formidable complaint, of which I have not seen more than two or three examples, proves fatal generally by the extension of the inflammation into the air passages. It is attended by fever, commonly of a low type. In Picardy and Touraine it would seem to be endemic; occurring sometimes sporadically, sometimes with an epidemic prevalence, and not without suspicion of contagious properties. In 1855 and 1856 it was rife and deadly in Boulogne. Dr. West has met with the disorder occasionally as an idiopathic affection, but much more frequently as "a most dan-

gerous complication of some other disease, almost always of measles." It seldom begins until the eruption of measles is on the decline, or the process of desquamation has commenced. There is generally so great a depression of the vital powers, as to contra-indicate the employment of active antiphlogistic treatment. The two remedial measures upon which Dr. West mainly relies, are the careful and repeated sponging of the fauces with a strong solution of lunar caustic, (a scruple to the ounce of distilled water,) and the exhibition of tartar emetic in the same manner as in cynanche trachealis. Mercury, by the mouth if the state of the bowels permit, or by inunction—and an early support by nourishing broths and by bark, or wine—form also important parts of the treatment.

The comparative freedom of the windpipe would encourage a trial of tracheotomy in these cases, when life seemed in jeopardy from impeded respiration; but the morbid condition of the blood, and the resulting character of the attendant fever, forbid the hope of such success from that mechanical remedy as it might otherwise promise.

This disease has recently (November, 1855) proved fatal in the family of an esteemed member of the council of this College. The patient, a young man about 17 years of age, was carried off after less than four days of illness. His symptoms, as described to me by my friend Dr. Burrows, were precisely such as I just now enumerated. The respiration became at last laborious, and tracheotomy was performed by Mr. Stanley—but in vain. In the larynx and trachea there was no trace of that sort of coriaceous membrane which covered the tonsils, velum palati, and fauces generally. I mention the case for the purpose of recording that, besides and beyond the usual appearances already specified, the centre of the swollen left tonsil was occupied by a sloughy abscess, while the right contained about a drachm of genuine pus. At no time during the progress of the disorder was any kind of lowering treatment admissible.

[Dr. Willige states (*Schmidt's Jahrbücher*, 1847,) that he has derived marked success in the treatment of severe cases of croup by the external application of iodine to the upper part of the neck. He applied, by means of a feather, the tincture of iodine over the front part of the neck, corresponding to the region of the larynx and trachea, and repeated the application at intervals of about four hours, until redness and irritation of the skin are produced. This, he declares, was followed in most cases by a subsidence of the difficulty of respiration, of the spasmodic affection of the glottis, and of the other distressing symptoms.

The internal use of the iodide of potassium has been suggested as a means, in the first stage of croup, adapted to modify the disposition to membraniform deposition in the larynx. It was first used in a case of the disease by Dr. J. D. Griscom, of this city, with apparently the most favourable results; it has since been employed by others, who speak of it in terms that should encourage us to a trial of it, that its true value as an antiplastic in this disease may be ascertained.

As a means of reducing the plasticity of the blood, and thus diminishing the tendency to the formation of false membrane in croup, the vegetable alkalies have been strongly recommended by several of the continental physicians. Eggert considers the carbonate of potass, after an experience of its effects in about two hundred and fifty cases, as almost a specific in croup; and Luzinsky speaks of it in the most favourable terms, (*Schmidt's Jahrbücher*, 1855). The latter states that it may be given advantageously in doses of from half a drachm to two drachms, daily. In mild cases the carbonate of soda may be employed, but in the more severe, the carbonate of potass is alone to be relied on. — C.]

There is a sort of bastard croup, with which it is quite necessary that you should be acquainted, for it is not at all uncommon; nay, it is far more common, in this place at least, than the real disease. It has received a variety of names, which shows that it has been recognised, as a distinct malady, by various observers. Yet no doubt can be entertained that it has very often indeed been confounded,—and is still continually confounded,—with the true croup, with cynanche trachealis. In their most obvious symptoms the two affections are much alike. The broad and essential distinction between them, is the absence, in the spurious disorder, of inflammation and of fever—and consequently of any concrete or other effusion from the mucous membrane of the air passages. The child is seized all of a sudden, roused perhaps from its sleep, or

checked during the act of sucking, by a catch, or interruption of its breathing, more or less complete. It strives and struggles to inspire, but is apparently unable to do so; at length the effort is successful, and the breath is drawn in with a shrill whistling or crowing sound, like that which characterizes the inspirations of croup, or of hooping cough, and depending, no doubt, upon the same cause—a narrowing (in this complaint temporary) of the fissure of the glottis. *Spasmodic croup* is the most common of its names. It is the *thymic asthma* of the Germans. My late colleague, Dr. Ley, in a volume upon this curious disorder, published a short time before his death, adopts from Dr. Mason Good the appellation of *laryngismus stridulus*. Dr. Gooch called it *child-crowing*, a homespun term, which I much prefer to the somewhat pedantic and cacophonous title bestowed upon it by Dr. Good. The crowing noise, and its concomitant phenomena, take place in paroxysms, which vary in respect to frequency and severity, and which are separated by intervals of easy and natural breathing.

“When the closure of the chink of the glottis is not perfect, the child struggles for its breath: the respiration is hurried; the countenance generally bluish or livid; the eyes staring; and each inspiration is attended with a crowing noise. When the closure is more complete (and this state was found by Dr. Ley, whose words I am now quoting, to be much the most frequent, at the commencement of the paroxysm) the function of respiration is entirely suspended for a while; there is an effectual obstacle to the admission of air. The child makes vehement struggles, by some termed convulsive, to recover its breath. At varied intervals, from a few seconds up to a minute, or upon some occasions nearly two minutes, air is at length admitted through the glottis, now partially open; and this rush of air, passing through a very narrow chink, produces the peculiar sound. To these symptoms not unfrequently succeed a fit of coughing or crying, which terminates the scene: or, if the glottis be not thus partially open, the child, at the end of from two to three minutes at the utmost, will die suffocated. Pallid and exhausted, it falls lifeless upon its nurse’s arms; and it is then that the child is generally said to have died in a fit.”

Sometimes, but not always, with the symptoms now described there is a contracted state of the flexor muscles of the thumb, fingers, wrist, ankle, and toes; giving to the foot an appearance approaching to that of club-foot.

Now, till very lately, most of those persons who had learned not to confound this child-crowing with true croup, were of opinion that it depended upon *cerebral* disease, or disorder. This was the doctrine of Dr. John Clarke, who has left a very good description of the complaint, which he called “a peculiar species of convulsions.” Dr. Ley doubted the correctness of that notion, and fancied that the bending of the limbs resulted rather from feebleness or paralysis of the extensor muscles, than from spasmodic contraction of the flexors. It was obviously a great point to make out, whether the disorder depends upon pressure *within the head* or not. Our treatment will be regulated by what we know, or believe, in that respect. But what was chiefly original, and very interesting, in Dr. Ley’s views concerning this bastard sort of croup was this, that he attributed the temporary closure of the glottis to pressure made by enlarged glands in the neck or chest upon the recurrent nerve, or upon some part of the eighth pair: “subverting the exact antagonism by which the glottis is automatically and involuntarily kept open, and allowing its margins to come together, and to occasion the peculiar kind of inspiration so much like that of croup.” Dr. Ley looked upon the affection altogether as more allied to paralysis than to convulsive movement. This certainly was a very original, but, I conceive, a mistaken view of the matter; I must refer you, however, to his book for the facts and reasonings upon which it is founded. The important practical fact announced by him was the frequent connexion of child-crowing with tumefaction of the glands in the neck and chest, and with the entanglement of the pneumo-gastric nerve, or its branches, among these glands. “Scarcely an instance (says he) has occurred to me since my attention has been very much directed to the subject, in which there has not been the strongest foundation for the belief that either the glandulæ concatenatæ of the neck, or the thoracic absorbent glands, had become morbidly enlarged.”

Not long after the publication of Dr. Ley’s book, this curious and intricate knot was somewhat further untwisted. Its complete solution is of yet more recent date. Dr. J. Reid ascertained, by a well-contrived set of experiments, that the inferior

laryngeal (or recurrent) nerve, is an efferent or motor nerve, by which nearly all the movements of the larynx are regulated; and that the superior laryngeal is an afferent or incident nerve. We may easily conceive, therefore, how pressure upon, or irritation of either of these nerves, may affect the aperture of the glottis. If the superior laryngeal nerve be implicated, the impression is communicated to the spinal cord, and thence reflected, through the recurrent, upon the laryngeal muscles. Irritation of the *gastric* ramifications of the par vagum may have the same result. It is an observation of Dr. John Clarke's, that the attacks "very commonly take place after a full meal." Nay, it seems probable that not only *any* of the afferent fibrils of the eighth pair of nerves, but those of the fifth pair also may have a similar exciting power; for a transient crowing is readily produced in some children, by exposure of the surface of the face and chest to a breeze of wind, or by their being suddenly tossed in the arms of a nurse. We must even suppose that the effect produced upon the central cord may, by reflexion, influence other muscles, and cause the carpopedal contractions that are so frequently to be noticed. Again, if the recurrent nerve itself be pressed upon, or interfered with, undue contraction or paralysis, according to the kind and degree of interference, will be likely to ensue, of the muscles belonging to the glottis. Spasm of those muscles would close the chink, and stop the breath. And Dr. Reid has shown that their palsy, except while the breathing is perfectly quiet and tranquil, sensibly impedes inspiration, and alters its character.

It appears, therefore, that the ingenious view taken by Dr. Ley of the special pathology of this child-crowing disorder, or croup-like convulsion, merges in the more general principles of reflex function advanced by Dr. Marshall Hall.

At the same time it is interesting to observe how Dr. Ley's theory harmonizes with what has been noticed of the predisposing causes of this crowing inspiration. In the first place, it is often manifestly connected with *dentition*. Now one effect of dentition is the production of glandular swellings of the neck; which happen even in the absence of all strumous taint, but with still more likelihood if any such taint exist. And thus he explains the fact, that the disorder has appeared in the most robust as well as in the most delicate infants. Thus also he explains another well-known fact, viz., that, when child-crowing accompanies painful dentition, the symptoms do not vanish instantaneously, as if by magic, the moment that the tooth starts through the gum; but pass off by degrees. Dr. Ley remarks that, "after the gum and enveloping membrane of the tooth have been relieved from swelling and inflammation by the free use of the gum-lance, *some time* is still required for the irritation and tumid state of the cervical glands to subside." It is not improbable that the gingival irritation alone may sometimes suffice, through the channel of the trifacial nerve, to determine the reflex spasm.

Again, this child-crowing is found to occur in connexion with excoriations behind the ears, and with inflamed and irritable scalp; and these complaints very frequently lead to enlargement of the absorbent glands of the neck, which enlargement may thus form an intermediate link in the chain of events. And upon some occasions Dr. Ley has had strong reason to suspect that bronchitis, or other *disease of the lungs*, has occasioned enlargement of the bronchial glands, and so given rise to the crowing inspiration.

I think that Dr. Ley has made out fair grounds for his view of the pathology of what is called spasmodic croup. But it is too partial and limited a view. Again, the alleged connexion between child-crowing and *cerebral* disturbance is not a mere fancy, although its bearing may have been sometimes misapprehended. The cerebral disturbance is mostly the consequence, more rarely the cause, of the impediment to the function of respiration.

[We doubt the correctness of Dr. Ley's views, as well as the views of those who refer the disease to enlargement of the thymus gland. For an examination of the facts connected with the pathology of *laryngismus stridulus*, the reader is referred to the Editor's Treatise on the Diseases of Children. — C.]

The practical *fact* which you have to remember is, that croupy breathing may occur, and return in paroxysms, when there is no croup. And the practical *lesson* which you have to learn is, how to discriminate between these two similar, yet diffe-

rent disorders. I have already specified the distinctive characters of cynanche trachealis. The complaint that copies it may be known by its sudden accession, and its sudden departure; by the freedom of breathing in the intervals between the paroxysms; by the absence of fever, of preceding or present catarrh, of hoarseness, and of any abiding cough. The diagnosis, easily enough reached when these points are sufficiently attended to, will be still more sure, if you discover enlarged glands in the neck: or hot, tense, and tender gums.

According to Dr. Clark, "convulsions of this description seldom, if ever, occur after the expiration of the third year of a child's life."

It must be unnecessary for me to urge the *importance* of the diagnosis. Those active measures which befit the outset of the inflammatory disease, would be misplaced and mischievous in the other. It was the result of Dr. Ley's experience that "those children have upon the whole a much better chance of being preserved, who are not subjected to very severe discipline, than those who, in compliance with the prevailing doctrine, have been treated by very copious bleedings, large doses of calomel, and such other remedies as the supposition of the invariable dependance of the disease upon cerebral turgescence, or excitement, has suggested."

Do not, however, imagine, from what I have just been saying, that this disorder, child-crowing, is a trifling disorder, and unattended with danger. It is really a perilous, as well as a terrifying condition. The respiration is sometimes so long suspended that death takes place in the paroxysm. And each paroxysm is accompanied by a tendency to stagnation and congestion of blood, in the brain, lungs, and heart: a tendency which, by its frequent repetition, may lay the foundation of serious and fatal disease in one or other of those vital organs.

The treatment of this affection must depend a good deal upon the nature of the predisposing and exciting causes. To go into it fully would require that I should speak of the mode of cure in painful dentition, in eruptive complaints about the head, in certain pulmonary and cardiac diseases, in disorders of the digestive organs, and in all those conditions which are apt to cause enlargement of the catenated or bronchial glands: or to fret, in some other way, the pneumogastric nerves.

In addition to the special methods of treatment which these complaints may severally need, great care must be taken, in all cases, to regulate the state of the bowels, and of the skin. *Fresh air* also is a powerful adjuvant; and sometimes of itself a sufficient remedy. Change of place, therefore, and especially a removal from the air of a city or town to the purer atmosphere of the country, should be urged whenever it is practicable.

Dr. Wm. Budd has recently announced that in ten or twelve instances, he has observed a speedy subsidence of this disorder under from five to ten grains of the phosphate of lime, administered three times a day, in chalk mixture. Whether the success of this remedy was due to its antacid properties, deserves further inquiry.

In the paroxysm itself the warm bath might be useful, if it could be got ready in time. The application to the throat of a large sponge from which hot water has just been squeezed, is a more accessible, and often a very effectual expedient. Sprinkling the face and chest freely with *cold* water will sometimes unlock the spasm, and set the little sufferer free.

I should have stated, that Dr. Ley's peculiar views were amply supported by the researches that he had made into the morbid anatomy of this affection. Had he lived, I should have had it in my power to show you some very beautiful and interesting preparations, illustrative of the connexion of the disorder with enlarged glands, which had come to press upon, or stretch, the par vagum, or its branches. As it is, I can only show you the published engravings from some of them, appended to his book. But for a full view of the pathology of laryngismus generally, I recommend you to study Dr. Marshall Hall's first essay *On the Theory of Convulsive Diseases*.

[SPASMODIC LARYNGITIS—Under the head of "child-crowing or spurious croup," Dr. Watson has confounded two very distinct diseases. One occurs, most commonly, entirely independent of inflammation in any portion of the air-passages, and consists simply in a spasm of the glottis, the result of a reflex action, through the excito-motory property of the nervous system, of various irritations of the gums or digestive apparatus, or of the action of external agents, or mental emotions. This is the affec-

tion known as *laryngismus stridulus*. The other disease differs in nothing from genuine croup, excepting that the inflammation is less intense, and seldom extends into the trachea; the exudation is of a more muco-form character, seldom adhering long enough to the inflamed mucous surface of the larynx to become converted into false membrane. This form of laryngeal disease has been termed *catarrhal croup*, *false croup*, and by some *spasmodic croup*: we prefer the name *spasmodic laryngitis*, given to it by Rilliet and Barthez.

The attack of spasmodic laryngitis is usually preceded by slight catarrhal symptoms; coryza, watering of the eyes, a sense of chilliness followed by flushes of heat; slight hoarseness and some cough. These symptoms may continue for twenty-four or forty-eight hours before a paroxysm of the disease is observed. In general this occurs at night; the child being suddenly awake from its sleep by a sense of impending suffocation. He starts up in a sitting posture, or throws himself forwards on his knees. He cries out that he is choking, seems anxious for air, tears off every covering from his throat, and pushes away all who surround or offer to assist him. The face becomes swollen and of a dark red or violet hue, the eyes projecting and humid, and the expression of the countenance extremely anxious. The inspiration is long and hissing; and so loud as occasionally to be heard in an adjoining apartment. There is also, at intervals, a frequent hoarse cough. The voice is constrained, but seldom whispering or abolished. The pulse is quick, and the skin hot. In a short time the paroxysm ceases, and the patient falls asleep. The disease may be confined to a single paroxysm. More commonly, however, the child continues during the ensuing day tolerably well, excepting perhaps that he is somewhat hoarse and has an occasional short barking cough, with or without expectoration; and on the succeeding night is again attacked with a paroxysm as on the night previous. The disease may continue thus, with one or two paroxysms each night, for several days, when the paroxysms will cease to recur, and the cough and other symptoms speedily disappear. In some cases, however, the paroxysms continue to recur at shorter intervals, and gradually augmenting in intensity, the patient dies from asphyxia, or, in other cases, after one or more paroxysms, he becomes affected with the utmost inquietude, with almost constant nausea and repeated vomiting, with symptoms of great exhaustion, cold extremities, and a small frequent pulse, and soon dies. Usually, however, the disease is one very readily controlled, and has seldom a fatal termination.

Spasmodic laryngitis is very liable to recur after three, six, nine months or a year, or whenever the child is exposed to cold and damp, or during seasons marked by sudden vicissitudes of temperature.

Barthez and Rilliet have pointed out the dependence of the disease upon actual but slight inflammation of the larynx. This opinion is based upon the fact that its attacks generally follow sudden exposure to cold, and by its being preceded by catarrhal symptoms, and being attended by some degree of febrile excitement. In connection with this mild bronchitis there occurs also a spasmodic affection of the glottis, to which they attribute the paroxysms of suffocation. That the disease is always attended with some degree of laryngeal inflammation also is evident from the result of post-mortem examinations. In the few instances in which an opportunity has been afforded for an examination after death, traces more or less decided of inflammation of the mucous membrane of the larynx have generally been detected—even when no traces of increased redness are present, the secretion of the membrane is usually found to be augmented, and of an opaque yellowish or reddish colour. When, as sometimes happens, death takes place suddenly during a paroxysm, it is probably the result of asphyxia, caused by a complete spasmodic closure of the glottis.

The disease usually occurs in children from ten months to eight years of age, but more frequently between two and five years. It occurs more often in boys than in girls.

There is in some families a peculiar predisposition to the attacks of spasmodic laryngitis—nearly all the children becoming affected with it as they attain the age of between two and three years. Though generally sporadic, it occasionally prevails as an epidemic.

Its occasional cause is almost exclusively exposure to cold and damp, or sudden alternations of temperature. After an attack has occurred, the paroxysms may be excited by any sudden or violent mental emotion.

The prognosis in this disease is generally favourable—but when the occurrence of the paroxysms is prolonged beyond the third day, and they constantly augment in intensity—especially when they are succeeded by great restlessness, nausea and vomiting, when the pulse becomes soft and feeble, when a frequent suffocative cough remains, and the patient's strength gradually diminishes, an unfavourable termination is to be apprehended.

The disease with which spasmodic laryngitis is most liable to be confounded, is pseudo-membranous or genuine croup.

The following comparative diagnostic peculiarities, borrowed partly from M. Valleix and partly from MM. Rilliet and Barthez, will enable us very readily to distinguish the two diseases —

In Croup.

There is fever of variable intensity — *pseudo-membranous angina*, and slight hoarseness.

There is a gradual increase of the hoarseness, and sooner or later a hoarse cough.

The fever does not remit; the cough becomes hollow and feeble, and the voice faint or extinct.

The disease is sometimes attended with an expectoration of false membrane.

The dyspnoea constantly increases, the croupal sound continues during the paroxysms—finally the voice and cough become extinct.

In Spasmodic Laryngitis.

The symptoms of invasion are slight — there are a slight catarrh, and a cough somewhat hoarse — the throat is unaffected — sometimes there are no prodroma.

The attack is sudden — usually at night.

Between the paroxysms the patient appears well — the fever disappears or declines — the voice is rarely extinct.

There is an expectoration of mucus.

The paroxysms of the disease decrease in violence.

The proper treatment of a case of spasmodic laryngitis will depend in a great measure upon the violence of the attack and the age of the patient.

When there are decided indications of laryngeal inflammation, and the spasmodic paroxysms are intense and prolonged, the application of leeches to the throat, in numbers proportioned to the violence of the symptoms, and the age and strength of the child, will be proper. In robust children of five years and upwards, bleeding from the arm will often be productive of the best effects. In no case, however, will it be proper to draw much blood either from the arm or by leeches; nor should the operation be repeated at short intervals, or too late in the disease — in the milder cases the abstraction of blood will not be demanded.

On the first accession of the disease it has been advised by Lehmann to apply a sponge, dipped in very hot water, and then carefully squeezed out, upon the anterior part of the neck, and leave it a moment in contact with the skin — the application being repeated at short intervals. This will produce a redness of the surface, and a general perspiration, which is to be promoted by giving some mild tepid diluent. By this simple procedure the paroxysm, we are told, together with all the leading symptoms of the disease, may be cut short.

An emetic given at the commencement of the attack, when followed by the warm bath, and sinapisms to the extremities, will generally abate the violence of the paroxysm, and shorten its duration. The exhibition of an emetic, or of small nauseating doses of ipecacuanha, in the intervals of the paroxysms, when considerable hoarseness of the voice, and a frequent short barking cough continue, will generally be found to remove very promptly these symptoms, and prevent their recurrence.

The bowels should always be attended to, and if not freely and regularly open, a dose of calomel, followed by castor oil or magnesia, will be proper.

Blisters to the neck or chest, or between the shoulders, are recommended by some writers. We have seldom seen much benefit to result from their application. When employed they should be allowed to remain on only for an hour or two; and when removed the part occupied by them should be covered with a soft emollient poultice.

In violent cases, subsequent to depletion and the use of the warm bath, much benefit has been derived from the administration of assafoetida, either by the mouth or by enema, a few hours after the paroxysm has terminated. Covering the breast with an assafoetida plaster we have, also, found to be decidedly beneficial.

Several of the German physicians recommend in the highest terms the use of musk in this disease. Wendt gave it in the dose of a grain every hour.

When the paroxysm, from its length and violence, threatens the destruction of the child by asphyxia, the operation of tracheotomy should be performed without delay.

During the entire continuance of the disease the child should be warmly clad, and kept in a warm, well ventilated room. His diet should be light, and of easy digestion, and his drinks should consist of weak, lukewarm lemonade, barley or toast water. The utmost care should be taken to preserve the patient in a state of perfect tranquillity. During the paroxysms he should be supported in an erect position; all covering or ligatures being removed from about the neck.

There is another affection of not unfrequent occurrence, to which attention has been recently directed by Dr. Behrend (*Journ. der Kinderkrankheiten*), that has no doubt been frequently mistaken for spasmodic laryngitis. It is a spasmodic cough of a peculiar character, to which children are liable, and which occurs only during the night.

This affection is most generally observed in spring and winter; it, however, also frequently occurs during the autumn, and but rarely in summer. It is seldom, if ever, observed in infants at the breast, but occurs at every subsequent period of childhood, and more frequently in boys than girls. There is no cough whatever during the day, not the slightest catarrhal symptoms, and, when put to bed, the child falls into a sound quiet sleep at the usual hour. In the course of a few minutes, however, it becomes restless, and before awaking, coughs violently. It now commences to cry, and the cough becomes more and more severe, until, in some cases, it produces vomiting; after one or two hours of suffering, the child again falls asleep, and passes the remainder of the night quietly. The cough returns at the same hour the following night, and continues sometimes for weeks and even months; finally, it begins to diminish in violence, the fits become of shorter duration, and occur at a more advanced period of the night, so that the length of the sleep enjoyed previously to its coming on, is gradually prolonged:—at length the cough disappears entirely and spontaneously. The subjects of this affection are pale, readily fatigued, and troubled with a coldness of the feet towards evening; they eat and play, however, as usual, and appear otherwise to enjoy good health. The nocturnal cough has frequently a catarrhal character, and is generally accompanied with a mucous rattle; sometimes, the cough is dry, croupy, and whistling; the kinks are short, isolated, and uniform, and may recur every five minutes, or extend only to one or two.

This nocturnal periodic cough cannot be mistaken for genuine croup—which has well marked symptoms; and it may be distinguished from spasmodic laryngitis by the complete intermission which follows the paroxysms, by its not being preceded by any catarrhal symptoms, and by the entire freedom from fever throughout the attack. It may be distinguished from hooping-cough from the latter being almost always epidemic, and occurring as well during the day as during the night, and always accompanied with a peculiar dyspnoea. In the nocturnal periodic cough there is no true appearance of suffocation; the children do not awake suddenly, as in a fit of asthma or spasm of the glottis, but they awake slowly, after having coughed several times during sleep. It most nearly resembles the cough of catarrh or bronchitis, but it is distinguished from those by the entire absence of the physical signs, and by the nocturnal periodicity of the fits, which are followed by complete remission.

Dr. Behrend considers this cough to depend on an affection of the nerves, and bases this opinion chiefly on the symptoms, and on the circumstance of its having frequently appeared after an epidemic of hooping-cough, and whilst intermittent fevers were very prevalent among adults. This latter remark has been made by several physicians whose attention was drawn to this cough by Dr. Behrend.

The prognosis, we are told, is always favourable. In one case the periodic cough was followed by bronchitis, but this was probably merely accidental. In another case there was repeated epistaxis.

The disease, upon the whole, is generally more troublesome than serious, and demands no active treatment. Gentle purgatives, as manna with the tincture of rhubarb, given in the evening, with attention to diet and regimen, are recommended. A drachm or two of the liq. acetatis ammoniæ, in a little sugar, may also be given, before putting the child to bed. One physician found small doses of quinia, given at bed-time, of great service; another found slight irritation of the skin, chiefly by means of pediluvia, and friction of the soles of the feet with roasted onions, and even enveloping the limbs, after the manner of Preissnitz, beneficial. — C.]

LECTURE XLVII.

Diseases of the Thorax. General observations. Dyspnoea. Cough. Methods of exploring the physical conditions of the chest, by the senses of sight, touch, and hearing.

FROM the throat—and especially from that part of it with which we were last occupied—the transition is natural and immediate to the *thorax*. Now the interior of the chest is the theatre of numerous and most important morbid changes. Within that cavity are lodged two of the three organs most essential to life. The heart, lungs, and brain, have been said, by a bold figure of speech, to constitute the tripod of life: and the two former are planted in the thorax. In the same region of the body lie also the greater blood-vessels, and many other parts of scarcely less consequence. With respect to the heart, its alternate swinging movement cannot long stop, and the patient continue to live: and a pause of three minutes in the play of the lungs would in most cases be irremediably fatal. And lesser impediments to the free working of either of these two vital organs are productive of much danger and distress; and lead often to consecutive changes of a very serious kind, in various other portions of the body.

These parts, of which the function is so indispensable, and of which the disorders are so grave and perilous, are encased in such a manner by the ribs and other boundaries of the chest, that they can neither be seen nor handled. And until a very late period in the history of medicine—until our own times, in fact—the diagnosis of the formidable maladies that befall the viscera of the thorax was exceedingly uncertain and imperfect. Physicians were able, indeed, by the observation of indirect symptoms, manifested through the general system—by remarking the presence of inflammatory fever, I mean—to infer that inflammation had been somewhere lighted up: and symptoms that denoted disturbed function of the respiratory apparatus—cough difficult breathing, or local pain—might suffice to apprize them that the inflammation was situated within the chest. But what tissue it affected, where was its exact place, what was its extent, or what were its physical consequences—these were points concerning which they had no means of obtaining any precise knowledge. “Under the title of pneumonia or pneumonic inflammation,” says Cullen, “I mean to comprehend the whole of the inflammations affecting either the viscera of the thorax, or the membrane lining the interior surface of that cavity: for neither do our diagnostics serve to ascertain exactly the seat of the disease, nor does the difference in the seat of the disease exhibit any considerable variation in the state of the symptoms, nor lead to any difference in the method of cure.” You will see, as we proceed, how very inaccurate this last statement would be, if it were made under our present mode of investigating these diseases. “Pneumonic inflammation (he continues) however various in its seat, seems to me to be always known and distinguished by the following symptoms:—Pyrexia, difficult breathing, cough, and pain in some part of the thorax.”

I state these things to you—who do not recollect the time, as I do, when no medical man in this country could, with truth and candour, say more of his knowledge of diseases of the chest than Cullen said—that you may the better estimate the exceeding value of the discovery of what is called the method of auscultation, for the detection and discrimination of disease; and most particularly of thoracic disease. In the present day we are able to obtain more exact information respecting the disorders of the parts contained within the chest, than of any other internal and therefore invisible parts of the body. Indeed, in a vast number of instances, we can tell, as accurately as if we saw them, the actual condition of the thoracic viscera: can follow, step by step, the successive processes of disease or of repair, in which they are involved. We can penetrate beyond the symptoms which denote deranged function, and comprehend those much less fallible signs which arise from alterations of structure. And this vast addition to our pathological knowledge has been given us by the simple application of one of our five senses to the investigation of disease, the sense of hearing, which

for so many centuries had been (strangely as it now seems) neglected, or but little used. Through the aid and use of the ear it has come to pass that those diseases which, besides being the most common and the most destructive, were also the most obscure, are now better understood than the diseases of any other internal part whatever.

The direct symptoms which arise out of the changed conditions of the parts affected in thoracic complaints, are so mixed up with all that we know or can learn of such complaints, that what in other cases is called the morbid anatomy of a disease, becomes, here, a part of its descriptive history. I shall not say, therefore,—as in regard to many other maladies I am obliged or I find it convenient to say—so and so are the symptoms; and afterwards, so and so are the morbid appearances: but I shall describe the morbid conditions in the outset, as the only way of rendering the symptoms which result from them intelligible.

But before I enter upon the subject of auscultation, it may be useful to make a few remarks upon those symptoms of thoracic disease which were previously known, and which depend upon, or rather which express, derangement of the pulmonary functions.

One of the most constant, and obvious, and distressing, and instructive of these symptoms, is embarrassed or laborious breathing: what is technically called *dyspnœa*. You know that by a healthy adult, under ordinary circumstances, the act of respiration is performed, unconsciously almost, about eighteen times in a minute. There is one act of respiration for about every four beats of the heart. In various diseases this proportion is materially altered. The reciprocal movements, by means of which, in measured succession, air is drawn into and again let out of the lungs, are performed with hurry, or effort, or unusual slowness. *Dyspnœa* implies some deviation from the natural manner or rate of alternately expanding the thorax, and suffering it to collapse again: of inspiring and expiring: in one word, of breathing. The patient himself may, or may not, be conscious of this deviation. In most of the cases in which *dyspnœa* claims to be regarded as a symptom, he is conscious of it. Now upon what does this symptom depend? It may ultimately be referred to an altered proportion between the quantity of atmospheric air that reaches the lungs, and the quantity of blood that is sent into them, from the right side of the heart, to be converted from venous to arterial. That, doubtless, is at the bottom of almost every case of *dyspnœa*. Let me remind you that respiration is an automatic movement; subject, nevertheless, to the occasional control of the will. The pulmonary branches of the par vagum constitute the principal and constant *excitor*, as the nerves that supply the muscles of respiration are the *motor* links of the nervous chain by which the automatic movements are governed. It is believed that the presence of venous blood in the capillary vessels of the lungs forms the natural stimulus to the pulmonary part of the par vagum. In the ordinary breathing of a healthy person, this stimulus or impression is not felt: perhaps because, being slight and habitual, and exactly apportioned to the need of the individual, it is not attended to: or, it is at once appeased by the admission of air, and the corresponding change in the blood. But when that change is not immediately or perfectly accomplished, then arises the distressful sensation which everybody has felt, but which our own language has no one word to express. The French call it the *besoin de respirer*. The English phrase, *want of breath*, denotes the peculiar sensation equally well. It calls into exercise, frequently, the voluntary power of performing the mechanical acts of breathing—a power which is superadded to the automatic process.

Various are the ways in which the natural manner and frequency of the respiratory movements may be disturbed. They were fully considered when I was on the subject of death by apnœa. They all operate ultimately, by destroying the just equilibrium between the blood and air which meet to undergo mutual and chemical changes in the lungs. We have lately been considering certain diseases in which the difficulty and distress of breathing are often extreme. In croup, and in laryngitis, the only inlet for the air is narrowed at its very entrance: there is more blood passing through the lungs than can be arterialized under the ordinary motions of respiration: instinctive efforts take place to increase the quantity of air; to make up by more numerous acts of inspiration for the diminished amount of air introduced by each single act. For a time these compensatory efforts may suffice. But if the access of air be still

impeded, blood but half decarbonized begins to circulate through the arteries; and to linger and stagnate in the lungs: the lips become livid, and the skin grows dusky. Make, however, a free and timely opening in the pipe that should conduct air to the lungs, and the balance being restored between the blood in those organs and the air that reaches them, the dyspnoea is soon at an end. The quantity of blood being the *same* then, but the air inspired *too little*, there will be dyspnoea. The very same thing occurs whenever a portion of lung, from being spongy, is rapidly rendered solid. No air can then penetrate it: nor *perhaps* any blood: but the same quantity of blood as before arrives at the right side of the heart, and is transmitted thence through the pulmonary artery: and consequently those portions of the lungs which are pervious to blood and air, are supplied with blood in excess, and require air in excess: *i. e.* dyspnoea is necessitated. And you will perceive that similar consequences may arise from any pressure made upon the lung, obliterating in a certain degree its cellular structure; as by fluid collected in the pleura; by enlargement of the heart; by aneurism of the great vessels; by tumours of whatever kind, within the chest; or by pressure upwards against the diaphragm by reason of a distended abdomen, whether the distension be occasioned by disease, such as ascites, or by obesity, or by a full stomach, or by a gravid uterus. A like disproportion will ensue, if the free expansion of the thoracic cavity be prevented by pain, by disease or rigidity of its boundaries, or by palsy of its muscles through interruption of the nervous circle whereon their contractions depend.

But on the other hand the balance may be destroyed from the opposite quarter: the air admitted during a single ordinary inspiration being the same, the quantity of blood requiring to be converted from purple to scarlet may be augmented; and in that case also, in order to maintain the due equilibrium, more numerous acts of respiration must be performed: in other words, dyspnoea will arise. This is the case under brisk exercise: the pressure of the muscles upon the veins propels their contents with greater velocity towards the right side of the heart; the heart contracts more frequently in proportion as it is more rapidly filled with blood; a greater quantity than usual is sent through the pulmonary artery to the lungs; and the individual breathes more quickly, to supply this augmented quantity of blood with air: he is out of breath, in a state of dyspnoea. But this is not disease. Disease, however, will often have the same effect. The quickened circulation in fevers, any obstacle to the free passage of the blood from the heart into the arteries, will tend to gorge the lungs with blood, to destroy the requisite equilibrium between the air and the blood in those organs, and so give rise to dyspnoea.

Other conditions still may be mentioned, as predisposing to hurry of the breathing—a peculiar state of the nervous system; certain qualities of the blood;—but I need not dwell on these at present.

There are two important collaterals deducible from what I have now been stating. In the first place, you must perceive how intimately the functions of the heart and lungs are dependent upon each other; and that disease originating in either of these vital organs may readily be the cause of consecutive disease in the other. We shall have many examples of this before us as we proceed. It would afford materials for an interesting essay, this mutual relation between cardiac and pulmonary disease. At present I merely glance at it in passing.

In the second place, what I have said of dyspnoea must have sufficed to show you that, taken by itself, it has not much value as a diagnostic symptom. All that it tells us is, that the healthy and natural relation between the quantities of blood and of air in the lungs is disturbed: but to determine the cause of that disturbance—to decide whether the heart be in fault, or the lungs, or both, or neither—we must have recourse to other sources of information.

Cough is another of the symptoms mentioned by Cullen as denoting disordered function of the breathing apparatus. I need scarcely tell you that it is produced by closing the glottis, and then making a sudden and strong expiration. Its purpose is the dislodgement of mucus which may have collected in excess in the air-passages, or of any other source of irritation to the membrane lining those parts. To be effectual it requires the admission of a certain quantity of air, and the possession of a certain degree of muscular strength. I pointed out to you, in the last lecture, a remarkable exemplification of this: the boy whose case is described by Mr. Chevalier in the

Medico-Chirurgical Transactions, was dying of croup; was on the brink of being suffocated by the collection of mucus in his windpipe and bronchi, which mucus he was unable to expel; and he was unable, not from defect of muscular strength, but because he could not sufficiently inflate his lungs *beyond* the collected mucus. When an ample opening was made in his trachea, he drew in a strong breath, and coughed the mucus up *through the rima glottidis*. In old and feeble persons labouring under chronic bronchitis with profuse secretion from the mucous surface, strength is often wanting to cough the phlegm up; and they die suffocated.

But the sensation which prompts to the act of coughing may arise from various other causes besides the accumulation of mucus in the air-passages. Any slight irritation about the glottis; a long, trailing and tickling uvula; the inspiration of irritating vapours; pressure of any kind upon the respiratory organs; may any of them produce cough. Nay, it sometimes is provoked by sympathy with other parts; an instance of which we have in what is called a stomach-cough. Some morbid condition, some irritation, of the stomach exists, which being appeased, the cough ceases. You will recollect the name and the functions of the *pneumogastric* nerves. We have, in the fact just mentioned, another example, in addition to those which I glanced at in a former lecture, of irritation of the sentient extremities of one branch of a nerve, declaring itself by uneasy sensations referred to other branches of the same nerve. For these reasons, therefore, cough is not more diagnostic of particular diseases situated in the thorax, than is dyspnoea. There are, indeed, certain varieties of cough, as there are certain modifications of the breathing, from which we may obtain very useful information even in respect to the nature and seat of *some* diseases: and these varieties and modifications I will point out as I go along.

Let me admonish you, also, before we come to auscultation, not to fall into an error which has been too common; that of trusting entirely to the ear in the investigation of thoracic disease, to the exclusion or neglect of those phenomena which are perceptible by the eye, or by the hand; or of those indirect revelations which are furnished by the condition of other parts and functions, or by the previous history of the patient. Even before the discoveries of Avenbrugger and Laennec, physicians were too remiss (if we may judge from their writings) in what may be called the mechanical exploration and notice of the actions of respiration. A good deal may be learned, sometimes, by merely placing one's hand upon the chest, or belly, as I shall explain more particularly by and by: and a great deal, also, may be made out, in some cases, by the simple inspection of those parts, when they are uncovered. You may see, for example, that the ribs, in respiration, scarcely move at all, while the belly rises and falls alternately with the descent and ascent of the diaphragm. This is called *abdominal respiration*. It may arise from a painful condition of the intercostal muscles, or of the pleuræ, rendering the patient *unwilling* to elevate his ribs; or it may arise from disease of the spinal cord, between the origins of the phrenic nerve and of the intercostal nerves, rendering the patient *unable* to raise them; or the same *inability* may result from disease of the lungs themselves. The symptom may guide us at once to the seat of the malady. Again, the breathing may be entirely *thoracic*, no motion of the abdomen taking place; and this may depend upon an affection of the diaphragm, or of the pleura which is reflected over it; or upon disease, accompanied with tenderness, within the abdomen—upon peritonitis for example; or upon mere distension of the abdomen. Or, by looking at the naked chest, you may see that one side of it moves, and that the other moves less, or does not move at all: and the motionless side may be of the natural size as compared with the other, or it may be flattened and contracted, or it may be round and bulging; and most important conclusions, and most important indications of treatment, will flow from a knowledge of these circumstances. The general form of the chest is also instructive. Never neglect, then, to examine the thorax, in cases where it is supposable that the disease may be seated in that part of the body, by your *eye*, as well as by your *ear*. The eye needs but little training to enable it to perceive and comprehend those signs which are within its sphere: the ear, unfortunately, requires to be carefully educated. I will just remark, further, that in the case of females no indelicate exposure of the person need be made. In most instances the morbid conditions I have been adverting to may be recognised through a thin linen covering.

Auscultation signifies the investigation of internal diseases through the sense of

hearing : and it is especially applicable, for reasons which I either have stated or will state, to the study of *thoracic* diseases. In its full meaning it includes all that we learn by listening to a cough, and all that we gather by striking the chest, and attending to the resulting sound. But in general, the term *percussion* is used to express this last mode of eliciting information, although the information is conveyed through the medium of the ear : while the term *auscultation* denotes the art of distinguishing diseases by hearkening to internal sounds, by means of the ear applied to the naked or thinly covered surface of the body ; or by means of some conductor of sound interposed between the ear of the listener and the person of the patient. In the first of these two modes the auscultation is said to be *immediate* ; in the second, *mediate*. By percussion we ascertain the degree of resonance, or want of resonance, of the part struck : by auscultation we learn the qualities and modifications of the voice, as reflected through the chest ; and of the breathing ; and of the sounds of the heart. The invention of the method of percussion we owe to a German, Avenbrugger, who wrote an excellent treatise upon it, which was brought into notice by Corvisart, who translated it. For the more brilliant discovery of auscultation we are indebted, as everybody knows, to Laennec.

Now it will save us much trouble, and conduce, I hope, to your future progress as practical auscultators, if, before I speak of any of the diseases of the chest, I premise some general observations respecting these modern methods of examining the human body, with the view of detecting and discriminating its diseases. Indeed, I could not make myself intelligible unless I did so.

And first, with respect to percussion, which you will please to recollect is nothing else than auscultation of, or listening to, sounds which we ourselves artificially and purposely produce.

You know, every child knows, by daily experience, that different substances, when struck, give out very different modifications of sound. If you strike a drum, you get one kind of sound ; if you strike a brick wall, you get another. The one is loud, trembling as it were, and prolonged ; the other dull, short, and flat. But why should I attempt to describe in words things which are familiar to you already ? Bodies that are solid, or inelastic, give the dull flat sound in proportion to their solid thickness, or their want of elasticity. On the other hand, hollow vessels, *i. e.* vessels containing air, with thin, firm, elastic boundaries, give out a sound more or less approaching in its qualities to that of a drum : the sound is called a *hollow* sound from that circumstance. If you have a wooden cask containing air only, it is resonant when struck : fill it half full of water, and the lower part will render a flat sound, the upper empty portion a hollow sound ; less hollow, however, than when the vessel contained no water : fill it up with water, and the *whole* is dull on percussion : pour out the water, and fill it loosely with wool—it will still be resonant, though in a different and less clear note than when it held air alone.

Now this experiment may be transferred to the human chest, which is a cavity, bounded by firm, thin, tense, and elastic walls, and containing, in its natural state, the spongy lungs, which are full of air ; and other parts that are solid, whereof the heart is the chief. If you strike the surface of the chest (it requires a little knack to do it properly), and if the blow fall over a portion of healthy lung, you will produce a resonant or hollow sound. If the lung be not there, if it be pushed aside, and its place supplied by some more solid or inelastic substance, by fluid for example, you will hear a dead sound. So you will if the lung *be* there, but has lost its spongy character, is void of air, and somehow or other solidified. But you may have a resonant sound, though the lung is in a state of disease ; nay, though the lung is not there : so that percussion alone cannot always be depended upon. I shall tell you, hereafter, how to guard against being misled by it in such cases. Again, if you strike over the region of the heart, you will get a positively dull sound, or at any rate a much duller sound than in most other parts of the chest.

It is really a singular thing, that this method of searching for indications of disease, and of health, should have been so long neglected or overlooked in our profession. I am sure that I had a practical acquaintance with the principle of percussion long before I knew anything of physick ; and so, I make no doubt, have most of you. Many a time, when wishing to know whereabouts I might drive a nail firmly into a wall, I have tried with the hammer to find which was brickwork, and which

was wooden joist; and percussion is an art in daily use for similar purposes, with carpenters and bricklayers. Yet it does not appear to have been thought of by physicians till the middle of the last century, when Avenbrugger, after studying its results, "*inter tædia et labores*," for seven years, published at Vienna his "*Inventum novum, ex percussione thoracis humani, ut signo, abstrusos interni pectoris morbos detegendi*." This was almost totally neglected, however, until, as I stated before, Corvisart's work on diseases of the heart brought it into general notice.

Avenbrugger and Corvisart, and indeed everybody who used percussion at all, until a very few years ago, employed *direct* percussion: that is, they struck the chest with the extremities of their fingers. More recently, *mediate* percussion has been introduced into practice, by M. Piorry. In mediate percussion some solid substance is placed upon the spot, the resonance of which is about to be explored, and the blow is made upon that substance, which is called a *pleximeter*—a stroke-measurer. A round thin plate of ivory, laid flat upon the surface, is the most common sort of pleximeter; or metal, or wood, or leather, or india rubber, may be employed. Many persons, and I am one of them, use no other pleximeter than the fingers of the left hand.

I shall explain, as briefly as I can, the method of employing percussion, and the cautions requisite to render it an effectual and a true interpreter of the state of the parts beneath the stricken surface.

The position of the patient is of some consequence. It should be one that is convenient to the examiner, and not inconvenient to himself; and it should be one calculated to render the part struck as firm and tense as possible. The best position of all is a sitting position, on a firm chair. But you may percuss a patient very effectually as he sits up in bed, or while he stands, or some parts even when he is lying down. A good deal is said,—more, in my opinion, than is necessary,—about the effect of curtains and so forth, in deadening the sound. I do not believe they will ever interfere with your conclusions, especially as we learn more from comparing the sounds given out upon percussing the corresponding parts of the opposite sides of the chest, than from the absolute resonance or want of resonance of any single part. But there are some exceptions to this; and if your patient can be made to sit on a chair in the middle of the room, so much the better.

Then, if you are about to percuss the front of his chest, let him suffer his arms to hang loosely down, and let him throw his head back. If you desire to explore in this way the lateral portion of the thorax, he may place the hand of that side upon his head, and lean a little to the opposite side. If you would know how the posterior part of the chest sounds, he may lean forwards, let his arms hang loosely between his knees, and bend down his head.

Next, as to the mode of percussing. For direct percussion, the ends of the fingers of the right hand should be brought together, and into a line with each other, so that no one of them may project beyond the rest; and care should be taken, first, to compare the sound produced by striking any part of the chest on one side, with that produced by striking the corresponding part on the other side. It follows from this rule that we should not examine all the points on one side before passing to the other, because we should thus lose the remembrance of, and the power of accurately comparing, the sounds obtained from corresponding points. It is best to strike first on one side of the body, and then on the corresponding spot of the other side. It follows also that we are not to compare the result of percussion on one of the *ribs*, with the result of percussion on one of the *intercostal spaces*. The blow should fall *upon* the rib, and *parallel* to it.

A second point requiring attention, is the state of the chest in respect to the act of breathing. If one side be percussed after the movement of inspiration, and the other after that of expiration, some little difference in the resulting sounds will be manifest, even in the healthy condition of the thorax. And this might mislead. Let corresponding spots on the two sides be therefore both struck, either while the chest is expanded, or while it is collapsed, or while the patient holds his breath.

Thirdly, you must take care to strike the corresponding parts at the same angle, and not with the fingers perpendicular to the surface on one side, and inclined obliquely to it on the other: also to strike corresponding parts with the same degree of force. And the blow should not be hard enough to give the patient pain; indeed

such a blow would not produce a good sound. It should be smart and quick; the ends of the fingers should not *remain* on the chest. Under some circumstances, however, the patient cannot bear to be percussed at all.

These latter cautions are most necessary when *direct* percussion is employed; over which *mediate* percussion has, however, many advantages. Some of these are obvious. In the first place, the space examined by mediate percussion is very exactly defined and limited. Secondly, you may strike the pleximeter much more forcibly than you could strike the unprotected body, and so produce a more decided sound. Even when the surface is morbidly sensitive, or the patient unusually irritable, so that percussion in the ordinary way cannot be performed at all, it may generally be done through the pleximeter. A third and very great advantage is, that mediate percussion is available when made over certain parts where, even although there may be no pain occasioned by it, ordinary *immediate* percussion is attended with no useful result. Parts, I mean, where there is much fat, and parts which are fleshy, or œdematous. If the pleximeter be pressed firmly upon these parts, even upon the mamma in females, the hollow sound is attainable; whereas, if they were struck by the fingers, the sound would be perfectly dull. Mediate percussion may be applied also, with effect, through the clothes.

I say that a convenient way, and one which I find quite sufficient, of employing mediate percussion, is by making a pleximeter of the finger, or fingers, of the left hand; taking care that they are closely in contact with the subjacent parts, and *similarly* applied to corresponding spots, and that the backs of the fingers be outwards. Piorry declares, indeed, that the resonance produced by this mode is scarcely one-tenth part so great as that elicited by using a thin, solid, and elastic plate. For all practical purposes, however, I am certain that the finger, as it is the readiest, so also is it a very satisfactory and sufficient pleximeter. It has, moreover, this positive advantage, that the sound made by striking it is not loud, and does not obscure or interfere with that which depends upon the condition of the chest.

It is right that you should be aware of differences of sound which belong to the individual. *Cæteris paribus*, the sound given out on percussion is more resonant during *inspiration* than during *expiration*; in childhood and youth than in middle age; in middle age than in old age; in females than in males; in thin persons than in fat; and, *they say*, in nervous irritable people than in those of a contrary temperament.

And it is still more necessary that you should be aware of differences of sound given out, in health, by different parts of the surface of the thorax, *in the same individual*. And in order to explain this more distinctly, and for the sake of reference hereafter, let me here exhibit to you the *regions* of the thorax, as they have been artificially mapped out for the purposes of auscultation.

It is unnecessary for me to describe particularly the method followed in this arbitrary division of the thorax into regions. I will state the names that have been commonly applied to them, and the sounds which in health they respectively yield, according to the numbers in the diagram before you.

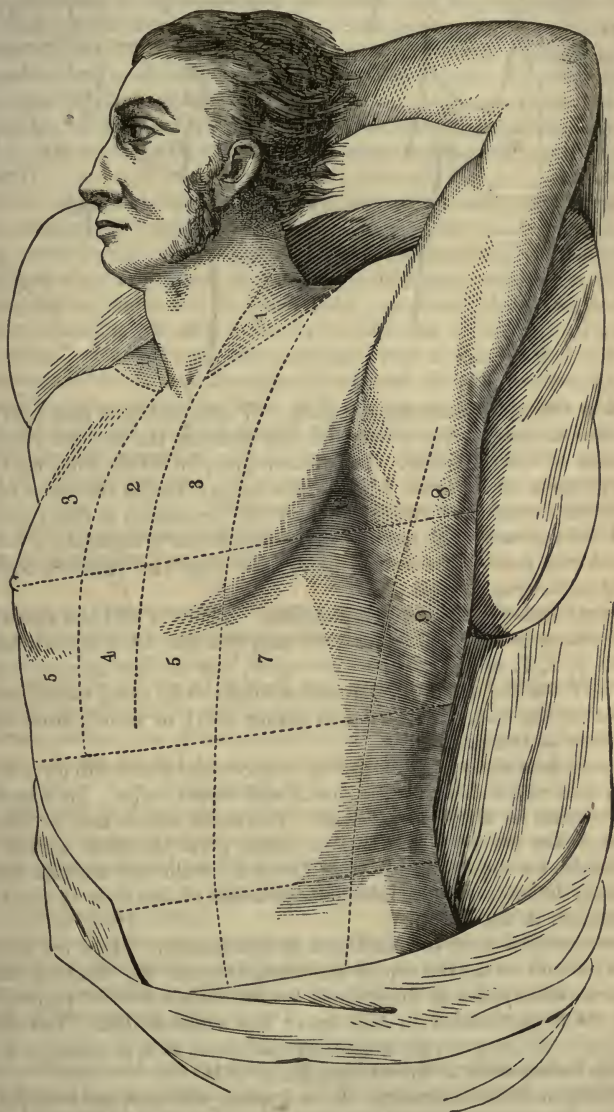
The *first* region is that of the *clavicles*; one, of course, on each side. Upon these bones it is immaterial whether direct or mediate percussion be made. The sound given out should be very clear at their sternal extremities, dull at their humeral extremities, and clear at their middles. The resonance diminishes from the sternal towards the acromial end of the clavicle. These parts correspond to the summits of the lungs.

The *second* region is the *subclavian*. It lies between the clavicle and the fourth rib on each side. Beneath this superficial region lies the upper lobe of the lung; and towards the sternum large bronchi are situated. You will understand, therefore, that the sound educed by striking this part ought to be very clear.

A little lower down, number *three*, is the *mammary* region, extending from the fourth to the seventh rib on each side, and answering to the middle lobe of the lungs. Here also the resonance afforded by percussion is clear; but in the lower part of this region on the left side, we find the heart, which is more or less covered by lung: and on the right side the liver begins to mount. The sound is somewhat modified and deadened by these deeper-seated viscera. In women we can determine the degree of resonance of this region through the *mammæ*, by *mediate* percussion only.

[The annexed figure, borrowed from Dr. Hughes' work on Auscultation, will give the student a more clear idea of the several regions of the chest than the figures furnished by Dr. Watson.—C.]

FIG. 40.



1. Acromial regions, corresponding with the *eleventh* region of Dr. Watson.
2. The superior sternal region, corresponding with the *fifth* region of Dr. Watson.
3. The infra-clavicular regions, corresponding with the *second* of Dr. Watson.
4. The inferior sternal, corresponding with the *sixth* and *seventh* of Watson.
5. The mammary regions, corresponding with the *third* and *fourth* of Watson.
6. The axillary region (left), corresponding with the *eighth* of Watson.
7. The lateral region (left), corresponding with the *ninth* and *tenth* of Dr. Watson.
8. The scapular region (left), corresponding with the *twelfth* of Dr. Watson.
9. The infra-scapular region (left), corresponding with the *thirteenth* of Dr. Watson.

FIG. 41.

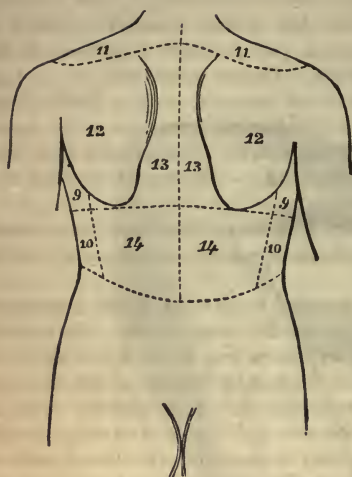
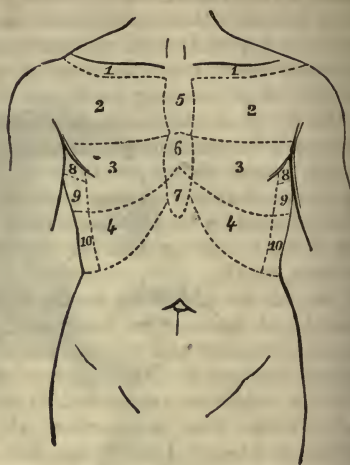


FIG. 42.



The *fourth* is the *infra-mammary* region. It comprehends that part of the bony compages of the thorax on each side which lies between the seventh rib and the edge of the cartilages of the false ribs. Into this region, on either side, may descend the thin anterior margin of the lower lobes of the lungs; but in the same region, on the right side, lies the liver, over which the sound of percussion is dull; and on the left side is placed the stomach. Hence when the stomach is tympanitic, a preternaturally resonant sound will proceed from this part; and when the stomach contains no gas, the sound will be irregularly dull.

All the regions hitherto described are double. There is still the mesial part of the front of the chest to be subdivided, and, we may call the three regions there situated, and numbered 5, 6, 7, the *upper sternal* region, the *middle sternal*, and the *lower sternal*. In all these the sound on percussion ought to be clear, except perhaps in the inferior portion of the last, which may be rather dull; or which, from its vicinity to the stomach, may be tympanitic.

The *eighth* region is the *axillary*; the axilla above the fourth rib on each side. The *ninth* is the *lateral* region, between the fourth and seventh ribs. In both these regions the resonance should be distinct and clear. The *tenth*, which may be called the *lower lateral* region, below the seventh rib at the sides, gives the same sounds as the *infra-mammary*, namely, on the right side, a dull sound, on the left a sound which at times is preternaturally hollow: on account of the presence of the liver in the former place, and of the stomach in the latter.

But we have yet to look at the hind part of the thorax. Here we have the space (region *eleven*) which forms the top of the shoulder, and lies between the clavicle in front, and the superior spine of the scapula behind: the *acromial* region. Very little can be made out by percussion in this spot; the sound is dull. But the depression immediately above the scapula, the *supra-supinal fossa*, as it is called, is a space which affords valuable information to the ear applied over it.

The *twelfth* region is the *scapular*. It comprises the part covered by the scapula on either side. It corresponds to the posterior part of the middle lobe of the lungs, but yields a dead sound, unless a pleximeter be used.

The *thirteenth* region is the *infra-scapular*. It lies between the inner edge of the scapula and the spine on each side. It corresponds to the roots of the lungs, and to the middle parts of their lower lobes. You may get a good clear sound here, if the patient's arms be crossed, and his head bent forwards, and his back arched, so as to stretch and tighten, and make as thin as possible, the superincumbent muscles; you will find also that the ridge of the spinal column in this part gives a hollow sound.

The last region to be mentioned, number *fourteen*, the *dorsal*, is the lowermost part

of the ribs behind. It answers to the base of the lungs, and gives a clear sound: but the liver on the right side is apt to render its inferior portion dull; and the stomach on the left to make it unduly resonant.

Now, knowing these circumstances, if you find that a dull sound is yielded on percussing a part that should render a hollow sound, you conclude that beneath the part struck there is not the natural quantity of air. But whether this arises from consolidation of the lung itself, and the obliteration of its cells, or from fluid effused into the cavity of the pleura, you cannot, by mere percussion, determine. So, again, if the sound be unnaturally tympanitic, you cannot say whether that results from what is called pneumo-thorax, or from emphysema of the lung. Percussion testifies that *air* is there; but further this deponent sayeth not.

The information which the exercise of percussion may leave thus imperfect, auscultation of the sounds produced by the acts of breathing, speaking, or coughing, will in most cases supply.

If the ear be laid close to the surface of a healthy thorax (or if the instrument called the stethoscope be interposed between that surface and the ear), the ear will hear the air enter and fill the lungs, and then withdraw and leave them again, in perpetual succession. The sound produced by this ingress and egress of air has been called the *respiratory murmur*. I might tell you that this sound, to my mind, is rather a rustle than a murmur: that it is like the sighing of the wind in the branches of a tree. I might say, with Laennec, that it resembles the sound made in the deep inspiration of a sleeping person; or, with Mr. Davies, that it reminds one of the soft murmur of a pair of bellows, of which the valve does not click. But one minute's appliance of your ear to the subclavian region in a child, or even in an adult, will give you a clearer notion of the nature and character of this sound than any verbal description could convey. Yet, respecting this natural respiratory murmur, there are some things of which it is desirable that you should be previously informed.

In the first place, the *entrance* of the air is much more noisy than the *exit*: which, sometimes, can scarcely be heard. You will see, by and by, the importance of noticing this fact.

In the next place, the murmur of respiration is not equally audible in all persons. It may differ much in intensity, though not in kind, in two persons, each of whom is in perfect health. Thus it is much more loud and distinct in children than in grown persons. So remarkably is this the case, that when we meet with an unusually noisy respiratory murmur in an adult, we say that his breathing is *puerile*; it has the character of the respiration of a child. Now, I tell you beforehand, that puerile respiration in the lung of an adult is generally a sign of disease; and it is mostly partial; heard in certain parts only of the chest.

Again, *cæteris paribus*, the natural murmur of respiration is more clearly audible in lean and spare persons than in such as are muscular and fat. Fat and muscle are bad conductors of sound, and act as dampers. Listening to the breathing through a thick layer of adipous tissue, is like listening (as Dr. Latham says) to the respiration of a person through his clothes.

But if you take two healthy men who are alike in respect to leanness and fatness, you may often find that the respiratory murmur is very loud in the one, and very feeble, or almost inaudible, in the other: but in this last it *becomes* audible when he makes a deep and forced inspiration.

The reason of this difference is not very well understood. The breathing may be very indistinct, though the thorax be capacious, and well formed, and healthy. Some persons seem to require less effort than others to maintain the due equilibrium between the air in the lungs and the blood in the lungs; as if they had not only pulmonary space enough, but to spare. So that the difference in the manner of breathing, and in the sound occasioned by the passage of the air in and out, depends, in all probability, upon individual peculiarities of the circulation. At any rate, it is very important that you should be aware of the existence of these differences.

But the sounds which reach the ear applied to the chest of a breathing person will differ in different parts, and under different circumstances. The sound given out by the air as it passes through the trachea and larger bronchi, differs from that which results from its passage into and out of the smaller bronchi and air-passages. I am anxious not to refine too much in these matters: and, therefore, I pass by

minuter points of difference. Place the stethoscope over the trachea, and you will hear just such a sound as you might expect to hear: the sound of air rushing through a tube of considerable size, a blowing noise. We will call that sound, which you will recognise again when you have once heard it, *bronchial* breathing. It accompanies the outward as well as the inward passage of the breath. Again, place your ear or your stethoscope upon the right mammary region: there you shall hear that rustling sound, which I propose to call *vesicular* breathing, and which is chiefly audible during inspiration. We shall find these distinctions of much use in the discrimination of disease. We shall find, for example, that the breathing sometimes is bronchial, where it *ought* to be vesicular.

Now if in any part of the chest where we should hear breathing, we can hear none, this may result from consolidation of the subjacent portion of lung; or from some obstacle in a large bronchus, preventing the air from entering that part; or from air being in any way shut up and stagnant in that part; or from an effusion of liquid into the pleura at that part. And here again percussion comes into play, and determines for us which of these possible circumstances is the one really present. If the part when struck emits a hollow sound, there is stagnant air beneath it, either in the healthy lung, or in the lung altered by disease, or in the cavity of the pleura. If a flat solid sound be given out, there is solid lung beneath, or liquid in the pleura, between the ear and the lung. All these points, and the conclusions to be deduced from them, will become clear to you, I trust, as we proceed. Minuter analysis of the diagnostic signs would be premature.

A word or two preliminary I must say about the *voice*.

The voice passes outwardly through the mouth and nostrils into the surrounding air; it passes also inwardly, through the trachea and bronchi into the lungs, and it may be heard through the lungs, by the ear laid flat against the chest. But it gives quite a different sensation to the ear in different places. If you place the stethoscope on the trachea, the voice will articulate itself into your ear as if it came from and through the instrument. This sound, which is natural here, would be unnatural, and a mark of disease, if heard beneath the clavicles. When heard beyond its natural situation, it is called *pectoriloquy*. A less degree of this, a sound like that of a person talking into a tube, and whose words, for that reason, are muffled and indistinct, is called bronchial voice, or *bronchophony*. When to this modification of the voice there is added a twanging vibration, a cracked discordant tremor, resembling the squeak of Punchinello, or (as some think) the bleating of a goat, *ægophony* is said to exist. All these unnatural modifications of the voice are indicative of most important changes within the thorax. I merely introduce them to you now: you will become better acquainted with them in due time.

It is a curious thing — of which I am unable to give you any satisfactory explanation, but which I had noticed a hundred times before I saw it mentioned in any book — that the resonance of the *listener's* voice, whose ear is applied to the patient's chest, is apt to be exalted or modified by certain conditions of the subjacent parts. I am not aware that any inference can be drawn from this phenomena beyond what other symptoms would still more readily and accurately supply.

What is true of the natural voice, is true, *mutatis mutandis*, of that unnatural vocal sound, *cough*. The cough may be so modified by the condition of the internal parts, as to reveal that condition.

There are yet other ways in which some information can be gathered respecting the interior of the chest. If you give certain patients a sudden smart jog while your ear is applied to their thorax, you may hear a splashing sound: like that produced by shaking a barrel or a bottle partly full of water. From this expedient you in fact determine that the cavity of the pleura, or perhaps a large excavation in the lung, does contain both air and a liquid. This is called the method of *succussion*. It was employed long before the other forms of auscultation were thought of. Hippocrates mentions it.

Again (but that is not auscultation), you may sometimes collect useful information respecting the state of the chest by simply laying your open hand upon it. In most persons there is a distinct vibration or thrill produced by their speaking, which thrill is very sensibly felt by the hand. Now this thrill may be felt on one side of the chest and not on the other. And such a difference is an unequivocal sign of disease.

The positions which I described before as being the most convenient for the performance of percussion, are the most convenient also for the purposes of auscultation. You may listen with the unassisted ear, or you may listen through the stethoscope. This, as you know, is a solid or perforated piece of wood, of which one end is adapted to the ear, and the other, which is to be applied to the chest, is hollowed out, or expanded, into the shape of a bell, or funnel. The object of the instrument is to collect and convey to the ear of the observer the vibrating impulse of the air, or of the solid walls of the thorax, occasioned by the motions within. The stethoscope is sometimes useful for examining a circumscribed spot in the thoracic cavity. With it we gauge, more nicely than we could do with the naked ear, the signs furnished by the patient's voice. We *must* use it also when we would investigate the breathing in parts to which the ear itself cannot easily be applied; and in cases where, from the filth of the patient, or because he has some infectious disorder, we desire to avoid any immediate contact with his clothes or person; and in cases of young or old ladies, to whose breasts it might not be seemly or delicate to be putting our faces. But, with these exceptions, the stethoscope, in my experience, is rather a hindrance than a help. Much, however, depends upon custom. I can best distinguish most of the sounds to be heard within the thorax by my unassisted—perhaps I should say my unimpeded—ear: and I therefore employ the stethoscope or cylinder in such cases only as I have just adverted to. I cannot do without the instrument, but I do without it as much as I can. Care should be taken that the end of the stethoscope next to the ear be closely and comfortably fitted to that organ; and that its other extremity be blunt, so as not to hurt the patient; and further, that it be uniformly and evenly applied to the surface of the chest. If these precautions be attended to, the shape and fashion of the instrument are very unimportant.

LECTURE XLVIII.

Catarrh; its varieties. Acute Bronchitis. Dry Sounds attending the Respiration; Rhonchus, and Sibilus: Moist Sounds; Large and Small Crepitation: how these are produced, and what they denote. Treatment of Acute Bronchitis. Collapse of the Lung—diffused, and lobular. Sudden Infarction of a large Bronchus. Peripneumonia Notha.

IN the last Lecture I described to you, in a general manner, the method of auscultation; or the employment of the sense of hearing in the investigation of disease; and particularly of *thoracic* disease. I explained, as well as I could, the different sounds which result from percussing various parts of the chest; and from the entrance and exit of air during the several acts of breathing, speaking, and coughing. I mentioned certain conditions in which those natural sounds are abolished: but I did *not* speak, except incidentally and very cursorily, of the altered and new sounds to which the presence of disease within the thorax has been found to give rise. Nor do I propose to enter now upon any *formal* account of these morbid sounds. I shall endeavour to make you acquainted with their characters, and causes, and signification, as they arise in the progress of the separate thoracic diseases which I am about to consider.

Before you can possibly appreciate the *morbid* sounds, you must make yourselves familiar with the *natural* and *healthy*. You must have a standard, by which you may measure the numerous deviations from the natural sound, that will reach your ear in disease. Listen to the voice and the breathing of healthy children—or of some of your friends and fellow-students—and you will soon recognise those respiratory sounds which accompany the perfect state and working of the breath-machine.

I intend to commence with those diseases of the lungs in which the *mucous membrane* lining the air-passages is primarily or principally involved. This membrane is

often affected *alone*. It is much exposed to known *causes* of disease: to alternations of temperature in the air which is constantly passing over it; to the irritation produced by acrid or noxious matters, solid or gaseous, which are mixed and inhaled *with* the air. And when disease commences in *other* parts of the lungs, it seldom fails to reach, sooner or later, the *mucous membrane*. In diseases also of the *heart* the same mucous membrane is very liable to be consecutively affected, by reason of those alterations in the condition of its capillary vessels which the disorder of the circulation produces.

Now I shall first point out the changes to which this mucous membrane is liable; and then describe the modifications of the natural sounds that result from such changes; and lastly, consider the different forms of disease which these changes constitute, and these altered sounds denote.

In a former part of the course I gave you some account of the peculiarities which differences of texture impress upon the phenomena and process of inflammation: and among the rest, I spoke of the behaviour of the *mucous* tissue when inflamed.

The mucous membranes, in the state of health, are perpetually *moist*. The exhalation of this moisture, to a certain amount, and *not beyond* a certain amount, constitutes an essential part of their healthy functions. Now their *inflammation* (for I am about to consider first the inflammatory affections of the membrane of the air-passages; some of them indeed I have already discussed), I say the inflammation of these mucous surfaces alters their ordinary secretion. An inflamed mucous membrane is in the first instance *dry*; its secretion is suspended. But this is not the only change that takes place in it; it becomes tumid also, swollen, thicker than before: it is redder than natural: and its sensibility undergoes a perceptible modification. *Pain*, in mucous membranes, is *not* a common phenomenon: for their texture enables them to expand or dilate freely, so that they escape much tension, and the pain which is produced by tension; but their natural sensations are blunted, and new and uneasy sensations arise in them: sensations of heat, fulness, itching. It happens that we can *see* a portion of the mucous membrane that belongs to the air-passages: and by noticing the changes produced in it by inflammation, we infer those which are apt to take place in the parts we *cannot* see. We have all often experienced in our own persons an inflammatory state of the membrane lining the nasal cavities; the schneiderian membrane. At first the nostril is preternaturally dry: yet, though it is dry, you cannot breathe through it: it is stuffed up; not with accumulated mucus, but by the mere swelling of the membrane: the sense of smell is perverted or lost; the part is evidently red; it is tender also and irritable; the contact of atmospheric air a little colder or a little less pure than common, provokes sneezing. The affection extends often into the frontal sinuses; and headache and oppression ensue: or it passes into and through the lacrymal sac, the conjunctiva participates in the inflammation, the puncta lacrymalia become impervious, and the tears flow over the cheek. And with all this there is sometimes shivering or chilliness; and the pulse, especially in the evening, becomes a little more frequent than common. There is slight fever. After the unusual dryness, the membrane begins to secrete a thin serous fluid, having acrid properties: for it reddens and frets the *alæ nasi* and upper lip over which it flows. By degrees, this thin serous fluid becomes thicker, and as it becomes thicker it becomes less irritating also, more viscid, opaque, and yellow: the swelling of the membrane diminishes; it is less raw and sensitive: at length the secretion resumes its natural *quality*, and is reduced to its natural *quantity* again; and the tumefaction of the membrane entirely disappears. This is the course of what is popularly called a *cold in the head*. When the defluxion from the nasal membrane is considerable, systematic writers call the complaint *coryza*; when it is attended with much pain and weight about the *frontal sinuses*, it is named *gravedo*. It is a variety of *catarrh*. In *catarrh*, sometimes one part, and sometimes the whole, of the mucous membrane of the air-passages suffers inflammation. If the disorder go down into the lungs, it is said to be a *cold in the chest*; or, from one of the most prominent of its symptoms, a *cough*; in medical language, *bronchitis*. It sometimes travels from one part of the membrane to another. Beginning, for example, in the nose, it gradually creeps down into the wind-pipe and lungs. Sometimes the inflammatory condition passes from the throat into the eustachian tubes, and produces deafness; or down the gullet and to the stomach,

causing qualmish or other uneasy sensations, and loss of appetite. And occasionally this order appears to be reversed. There are some persons who will tell you that whenever anything disagrees with their stomachs, whenever dyspepsia is produced by some error in diet, they are sure to have catarrh.

Now I have adverted to this *cold in the head*, or coryza, because the phenomena which are open to our inspection in the schneiderian membrane take place also, no doubt, in the bronchial. The membrane is first dry, and tumid, and irritable; the uneasy sensations of which it is the seat prompt to the action of coughing. The chest feels tight, stuffed, constricted. There is some hoarseness, and a sense of roughness and soreness in the wind-pipe; and a dry cough, which seems to arise from some irritation about the glottis. Sometimes, with these symptoms, pains in the limbs, like the pains of rheumatism, occur; the appetite is impaired; the patient is thirsty; and a general lassitude is felt all over the body.

But what effect has the altered state of the membrane upon the sounds elicited by percussion; or heard within the chest, by the ear, during respiration? Why it brings us acquainted at once with two remarkable modifications of the natural sound of breathing; and these I must describe and explain.

I will take this opportunity of again recommending you to read and study the lectures recently published by Dr. Latham. They contain a very plain and clear account of the auscultatory signs of disease within the chest; and he speaks of these signs in more easy and popular language, with less of over refinement, and a less subservient adoption of the French mode of thinking and writing on these subjects, than any other English author that I know of. I recommend his volumes the rather also, because he uses the nomenclature which is the most familiar to myself: in fact, as we some years ago saw and talked of these matters together in the wards of St. Bartholomew's, we were likely to employ the same terms.

When you listen, I repeat, to the breathing of a healthy person, you hear, as the breath goes in and out, but especially as it goes in, a smooth and gentle rustle—the *respiratory murmur*, or the *vesicular breathing*. But when the inner surface of the bronchial tubes, and of their ramifications, is preternaturally dry, and tumid, this sound is altered: you hear a hissing, or wheezing, or whistling, as the breath goes in and out; and this is technically called *sibilus*; or you hear a deeper note, a snoring noise, as the patient inspires or expires—a sound like the cooing of a pigeon, or the bass note of a violin, or the droning hum of an insect in its flight; and this is called *rhonchus*. These two, in their various modifications, constitute the *dry* sounds of respiration; and it will be worth while, once for all, to reflect upon their cause and nature, and the manner in which they are combined, and what they denote. You are aware that when air is driven through a cylindrical tube of a certain size, and when that tube is narrowed in a particular way at one or more points, a musical note is produced. Now this is what often happens in the larger bronchi; this is what *always* happens in them when *rhonchus* is present. Rhonchus belongs to the larger divisions of the bronchi exclusively; and as these are often, for a time, exclusively affected, so rhonchus may exist *alone*. It will be grave or deep in proportion to the length and diameter of the tube in which it is produced. When the sound is grave and deep, the hand placed upon the chest may frequently perceive a trembling or thrill communicated to its parietes. I believe that rhonchus is mostly occasioned by portions of viscid, half-solid mucus, which adhere to the membrane, and cause a virtual constriction of the air-tubes, and act as vibrating tongues while the air passes by them. I conclude such to be the case, because it seldom happens that the rhonchus cannot be got rid of by a vigorous cough. It will soon begin again, perhaps, or it will commence in some other part, but the effort of coughing, which detaches and removes the adhering tough mucus, dislodges also, for the time, the rhonchus. Yet, rhonchus in a given spot may be permanent: a tumour, or a tubercle, may flatten one of the air-tubes, and convert it into a musical instrument. For the most part, you will find what I have told you holds true: you may suspend the rhonchus by getting the patient to make a hearty cough. Now in the natural state of the chest, we do not, except in particular spots, hear the transit of the air through the larger bronchi. Whatever sound it makes is damped by the spongy lung, or covered by the vesicular breathing. But rhonchus, in its turn, may overpower the vesicular murmur, and render it inaudible. It does not prevent it, but it outroars it, as it were. Yet this

is seldom the case: you hear the rhonchus, and, if you listen attentively, you may in general hear, mingling with it, the vesicular murmur also. Recollect, then, that rhonchus belongs to the larger divisions of the air-tubes; that it denotes their partial narrowing; that it is a dry sound; and that the condition of which it is expressive implies usually no danger: there is no material obstacle to the passage of the air through these larger tubes into the vesicular structure beyond them.

I must further admonish you, that in your earlier essays in auscultation you will be apt to deceive yourselves in respect to the exact place in the lung in which the rhonchus which you hear is produced. It is so loud a sound, that when it proceeds from a single bronchial tube it may be plainly audible over the whole of that side of the chest; and sometimes, more obscurely, over the other side too.

When air is driven with a certain degree of velocity through a small pipe, it gives rise to a hissing noise. It is by forcing air through a cylinder perforated by a slender tube, that Professor Wheatstone obtains the sound of the letter S in the talking machine which he has constructed, after Kempelen's model. Precisely this condition we have in the smaller bronchial ramifications, when the inflammation in catarrh or bronchitis has reached *them*, and rendered the membrane lining *them* tumid. And sibilus is the result of this change. Now sibilus, like rhonchus, may exist alone; and, inasmuch as the sibilus proceeds from the smaller air-tubes, adjacent to the pulmonary vesicles, it *abolishes* the natural respiratory murmur. It does not, like simple rhonchus, merely drown it, but it takes its place. If you hear the respiratory murmur mingling with sibilus, you may be sure that some of the lesser air-tubes are narrowed, and some free: you cannot have both sounds at once from the same ramifications of the bronchi. Sibilus is a sound of more serious import, therefore, than rhonchus; it bespeaks a condition of greater danger. It belongs to the smaller air-tubes and vesicles, and denotes that they are in the first stage of inflammation, which has diminished their natural calibre, by rendering the membrane tumid. It is a dry sound, but you cannot cough it away.

I say rhonchus may occur alone, and sibilus may occur alone; but very often indeed they both occur together; and may be heard in various parts in different degrees: causing a strange medley of groaning, and cooing, and chirping, and whistling, and hissing, mixed, it may be, here and there, with the natural respiratory murmur. When you hear sibilus over the entire surface of the chest, the mucous membrane is universally affected, and the case is a severe one, and attended with considerable hazard.

It is just possible that a sibilant sound may proceed from a large air-tube, when its bore has been narrowed to a very minute slit or orifice; but this possibility does not interfere with the general distinctions that I have been endeavouring to point out.

Now in these cases we neither obtain nor require any information from percussion, except of a negative kind. Supposing the inflammation confined to the mucous membrane, the resonance on percussion will not be sensibly diminished; the lung is everywhere spongy still, and air reaches every part of it, though not with the usual freedom.

There is one exception to this. Occasionally, though rarely, a piece of tough phlegm may seal up, as it were, the very entrance of one of the principal bronchial tubes, and so prevent the air from passing to or from the portion of lung to which that tube conducts. When this happens, it is very likely to puzzle the auscultator for a time. There is air in the sealed-up portion of lung, therefore percussion gives a natural sound; but the air is at rest, therefore no sound of respiration is audible. An effort of coughing unstops, perhaps, the bronchial tube; and then the air is again heard to enter and to depart from that portion of lung. I shall advert to this sort of accident again.

Finally, I may remark, that these dry sounds, rhonchus and sibilus, are heard during the *breathing*; they have no relation to the voice or to the cough.

After a while, the inflamed membrane begins again to pour out fluid; but it is not the vaporous, bland, moderate exhalation of health; it is a glairy, saltish, transparent liquid, like white of egg somewhat; and if it be expelled only after much coughing, it will be frothy also, *i. e.*, it will contain many bubbles of air entangled in it. At first thin, and even watery, the fluid expectorated soon becomes stringy and tenacious, and the more so in proportion to the intensity of the inflammation. With this new condition of the membrane, we have new sounds—sounds which result from the

passage of air through a liquid; sounds which are occasioned by the formation and bursting, in rapid succession, of numerous little air-bubbles. These sounds are called *crepitations*. This process may take place in the larger air-tubes, or it may take place in the smaller, or in both. In the larger tubes the *bubbles* will be larger, and the ear can readily distinguish this; we have *large crepitation*. In the smaller air-tubes, we have, in the same way, *small crepitation*. There is no difference between these sounds, except in degree; and they graduate insensibly into each other. But there is a considerable difference in the nature of the intimations which their well-marked varieties convey. If there be merely large crepitation, without any other morbid sound, it is produced in the larger tubes. Air passes, notwithstanding, into the vesicular structure *beyond* the accumulated liquid; and vesicular breathing *exists*, though perhaps it *cannot be heard*, on account of the crepitation. But the state of the patient is not a state of peril. On the other hand, small crepitation has its seat in the smaller air-tubes and cells; it supercedes the vesicular breathing, and, if extensive, it bespeaks considerable danger.

Rhonchus and large crepitation are respectively the dry and moist sounds that belong to the larger bronchi; sibilus and small crepitation the dry and moist sounds of the smaller branches. When the latter sounds are heard over a considerable part of the chest, there is, I say, usually a good deal of distress, dyspnœa, and cough; and the fever which attends the local inflammation is at its height. By and by the expectoration becomes opaque, and more consistent, and of a greenish or yellowish colour; it is brought up with more ease; the crepitation, great and small, diminishes; perhaps rhonchus reappears: but at last the parts return to their original condition; and the natural, smooth, equable rustle of the breathing is again everywhere audible.

These are all the morbid sounds to which active and recent inflammation of the mucous membrane of the air-passages ever gives rise: rhonchus and sibilus; large and small crepitation. Having once described their nature and causes, I need not repeat the description if we find them accompanying other diseases; but their *import* may be different. I may mention here, that as crepitation results from the passage of air amongst and through a liquid, from the rupture of the little air-bubbles so produced, the *kind* of liquid may vary. If the air in going and returning, meet with serum, or with pus, or with blood, it will occasion exactly the same bubbling noise. Hence the French term for what I have been calling crepitation, viz., *mucous rattle*, is very objectionable. From the sound itself, we cannot tell whether it proceeds from *mucus* or from some other liquid present in the air-passages; and from this objection the word crepitation, whatever exception may be taken against it on other accounts, is free.

Having thus embraced the occasion of explaining these auscultatory signs, I will now resume the history of catarrh. It implies inflammation of the mucous membrane of the air-passages; and it receives different appellations, according to the district of that membrane which it chiefly plagues: *gravedo*, in the frontal sinuses; *coryza*, in the schneiderian membrane of the nose; *bronchitis* in the trachea and lungs.

Catarrh is the commonest of all disorders. Not one man in ten thousand passes a winter without having a *cold* of some sort. And this name points to its ordinary cause: cold somehow applied to the body. It does not always or often result, I apprehend, from cold air brought into contact with the membrane itself, in the process of breathing; but from cold, and especially from cold and wet, applied to the external integument. It is unnecessary for me again to go over the circumstances under which the application of cold is most likely to prove injurious. Catarrh is usually a trivial disorder, and runs its course in a few days, if abstinence be observed with respect to animal food and stimulating liquor, and if the patient remain in an equable temperature, and avoid re-exposure to the cause of his malady. I am now speaking of the milder forms of catarrh. We are not often consulted for this complaint. Every man, in regard to a cold, thinks himself qualified to be his own doctor. But if you are consulted, keep your patient in the house, or even in bed; let him live upon slops; give him a gentle aperient, and then some of those medicines which are esteemed to be diaphoretic: small doses of James's powder; three drachms of the liquor ammoniæ acetatis, with a drachm of the spiritus ætheris nitrici, and an ounce of camphor mixture; or a saline draught with an excess of alkali, and a few grains of nitre, or a little antimonial wine; give some such dose three or four times a day; and let him

take four or five grains of Dover's powder, and put his feet and legs into a warm bath, just before he goes to bed. In this way you may conduce to his *recovery*; and he may be simple enough to suppose that you have *cured* him.

Yet I believe catarrhs *may* sometimes be *cured*; and the natural recovery from them *may* be, *sometimes*, accelerated. If you practise the old maxim, which says, "*venienti occurrere morbo*," you may occasionally stop a cold on the threshold, as it were by an opiate. And to persons who are habitually troubled with slight catarrhs, this piece of practice may prove of the greatest value. A surgeon who resides in this neighbourhood, and with whom I was a fellow-student, is exceedingly subject to what he calls a snivelling cold. For many years he used to bear this as he best might; and that, to say the truth, was very ill and impatiently. On one occasion, almost by accident, he took twenty drops of laudanum just as one of his colds was beginning to torment him; and he found that the initiatory symptoms vanished. Since that time he has constantly had recourse to the opiate under similar circumstances; and whereas he used formerly to be very miserable for three or four days, he is now quite well and comfortable in the course of half an hour. And this is not a solitary case. It is worth trying, if you experience the feelings of an incipient catarrh, to go to bed, and to take a beaker of hot wine negus, with a tablespoonful of the syrup of poppies in it. This will not suit every person, but if it fail on the first trial, it need not be repeated, and no great harm, beyond an increase of headache, can be done by it. I would not recommend this plan, however, to a plethoric person; nor to any one having a tendency to inflammatory disease; for when it does not cure, it makes the complaint worse.

There is also a period in catarrh which has gone on unchecked, when you may accelerate its departure—"speed the going guest"—by a good dinner; and an extra glass or two of wine. But this pleasant method is scarcely to be advised for persons of delicate habit; or in whom any phthisical tendency is suspected to exist; or who are prone to inflammation. And it is not to be tried with any one till the fever is over, and expectoration thick and loose.

I must not omit to mention the *dry* plan of cure; although (I confess it with some shame) I have never yet tried it either upon myself or upon others. Dr. C. J. B. Williams, who invented it I believe, has a high opinion of its efficacy. It certainly has the merit of simplicity, for it consists merely in abstinence from every kind of drink. No liquid, or next to none, is to be swallowed until the disorder is gone. The principle here concerned is that of cutting off the supply of watery materials to the blood. The wants of the system exhaust, from the circulating fluid, all that can be spared for the natural evacuations; and there is nothing left to feed the unnatural secretion from the inflamed mucous membrane. Its capillary vessels cease to be congested; the morbid flux is diverted, and the inflammation starved away. Such is the theory. Habitual toppers might hold the remedy to be worse than the disease; but Dr. Williams assures us that the necessary privation is not very hard to bear: and that it achieves a cure, upon an average, in forty-eight hours. He allows, without recommending, a tablespoonful of tea or milk for the morning and evening meals, and a wine-glass of water at bed-time.

One great advantage of this plan is, that it does not require confinement to bed, or to the house. The man whose business calls him abroad, may, with appropriate clothing, pursue his customary employment, and his cure is all the while going on. In fact, exercise, inasmuch as it promotes perspiration, helps the recovery; whereas the system of warm drinks and diaphoretics renders the body more susceptible to atmospheric vicissitudes; and, to be effectual, implies restrictions which are oftentimes extremely inconvenient.

Dr. Williams observes, that while this dry treatment is serviceable in catarrhal bronchitis, it is *most* successful in coryza, snivelling cold in the head. It must be put in force in the very commencement of the disorder.

You may often do much by way of *prevention*, for persons who are unusually liable to take colds. I have remarked before upon the great value of the *shower-bath* for that purpose. I could mention several instances in which persons have got rid of the tendency to catch cold by the habitual use of this aspersion. It should be begun in the summer, and the water should at first be tepid; but in a short time quite cold water may be employed; and being once begun, the practice may be continued

throughout the winter. I stated formerly, that the effect of exposure to cold was, *cæteris paribus*, in proportion to the intensity and the duration of the sensation of cold that it produced. The intensity of the sensation of cold under the shower-bath is considerable, but the duration of it is momentary. It operates as a prophylactic in this way: it inures the surface to a lower temperature than it is likely to be subjected to at any other part of the day. The lesser degrees of cold have then no injurious effect, unless they are long protracted. For those who cannot procure a shower-bath, or who cannot bear its shock, cold sponging will be found exceedingly salutary.

But inflammation of the membrane lining the air-passages may be, and often is, a very acute and dangerous disorder, *i. e.*, the inflammation may be both intense and diffused; it may descend into the vesicular texture, and occupy the whole surface of the membrane on one side of the chest, and then it may prove a very grave disease; or it may involve the whole lining membrane of both lungs, and then it is always attended with considerable peril.

This inflammation will sometimes, when it is thus *general* over the whole membrane, linger for a considerable period in its first stage; and it may even, after so lingering, subside again without ever passing beyond the first stage. By the first stage, I mean the stage of dryness. Very little notice of this modification of bronchitis has been taken by authors. Dr. Latham has given a distinct and graphic description of it, to the accuracy of which I can testify from my own experience. You will find cases of it detailed in his book. Since they were published, some striking instances of this form of the disease have occurred to myself. One, which happened lately, I will relate by way of example. I was asked by an old pupil of the hospital to see a lady, his patient, in Gordon Square. I found her feverish, and in a state of extreme dyspnoea, sitting up in bed from inability to lie down, labouring for breath; her face turgid and rather livid, her nostrils working, her shoulders elevated. She could scarcely speak, but expressed, in what she did say, a dread of immediate suffocation. She had been in nearly the same state for a day or two. On listening at her back I could hear the air slowly wheezing and whistling into her lungs everywhere, and then leaving them still more slowly, with a prolonged growl, something like that of an angry cat. There was no true vesicular breathing; there was no crepitation: and there was no part into which the air did not, although with difficulty, find its way. The chest was everywhere resonant on percussion. There could be no doubt that the membrane throughout was tumid and dry, and in the earliest stage of inflammation. Depletion had already been employed in this case, and we had recourse to the tartar emetic. This was given in free and repeated doses, till it produced nausea and sickness. Whenever it did so, the pulse diminished in force, the face became blanched, and the breathing much easier; and the medicine was then suspended until these effects had gone off, when it was repeated in the same manner. The disease was not, however, brought at once to an end by this treatment; it was kept at bay for a day or two longer, and then a copious secretion from the membrane took place, with great relief to all the distressing symptoms. Then, of course, crepitation became universally audible. Except the debility which it left behind, the patient soon recovered of the bronchial inflammation.

But in the great majority of instances the inflammation does not thus linger in its first stage: the membrane soon begins to pour forth glairy mucus; so that we do not often meet with *sibilus*, without finding at the same time, in some part of the same lung, that there is also small and large crepitation. It is of some importance to attend to the characters of the mucus that is expectorated. It is transparent and adhesive. If you pour it from one vessel into another, it flows out in one mass of extreme tenacity; it will draw out sometimes like melted glass; and the degree of viscosity is a tolerably accurate measure of the degree of the existing inflammation. Upon the surface of the viscid mucus there is usually more or less froth, the *quantity* of it depending on the facility or the difficulty with which the sputa are brought up. If the patient do not expectorate till after a long fit of coughing, during which air has been many times inspired and expired, and has thus got intimately mingled with the mucus that fills the air-passages, the expectoration will contain numerous little air-bubbles; will be very frothy. Sometimes also, during this stage of the complaint, the sputa are marked with streaks of blood.

While the expectoration possesses the characters I have been describing, the in-

flammation is still active, and the fever and dyspnœa considerable. This correspondence between the general symptoms and the matters spat up was well known to the ancients, who said that such expectoration was still *crude*. But in proportion as the inflammation approaches to resolution, the appearance and qualities of the sputa are changed: the mucus loses by degrees its transparency, is mixed with masses or pellets that are opaque, and of a yellow, white, or greenish colour: and these masses, few at first, increase more and more in number, until they constitute the whole of the sputa. Such expectoration as this is commonly accompanied by a marked remission in the different symptoms of the bronchial inflammation: it announces that the inflammation is terminating in resolution. It is such as the ancients spoke of as being *concocted*, or *ripe*. However, the characters of the opaque sputa expectorated towards the end of an attack of acute bronchitis are subject to much variety.

It will often happen that the expectoration after having thus become opaque, and parti-coloured, will go back again to its former condition of transparency, and stickiness, and froth: and that is a very certain index of a return or increase or extension of the inflammation; so that the character of the matter expectorated exhibits, in a certain degree, the progress of the inflammation; and consequently constitutes one point of guidance to our treatment. The nature of the expectoration forms also an important particular in our means of distinguishing bronchitis from pneumonia; as I shall further explain when I speak of the latter disease.

I have described acute bronchitis as it appears when it terminates favourably: in such cases the inflammation generally begins to abate somewhere from the fourth to the eighth day of the disease. But acute bronchitis may terminate *unfavourably*. When the inflammation is universal and intense, the fever high, and the labour of respiration great—if the symptoms do not yield to the treatment employed, or if judicious treatment have not been adopted, or have been too long delayed, signs of impending suffocation begin to show themselves: the lips, and cheeks, and tongue, assume a purplish colour; a livid paleness takes the place of the former red flush; the expression becomes more and more anxious; delirium comes on, and rapid sinking. These indicate, you know, the circulation of blood that is in a great measure venous through the arteries; and the venous blood acts as a poison when it so circulates. Profuse, cold, clammy sweats ensue; and the patient dies of apnœa. His breathing is choked by the morbid secretion which occupies the bronchial tubes, small as well as large, and which he has not strength enough left to cough up.

Accordingly, when we examine the thorax after death so produced, we find, in the first place, that the lungs do not subside upon the admission of the pressure of the atmosphere to their external surface. We next find the trachea, and bronchi, and their ramifications, blocked up by a frothy adhesive mucus, resembling that which during life had been expectorated: and the membrane which lines them is red and thickened.

The treatment proper for these acute and dangerous forms of bronchitis is a matter of some nicety. Blood-letting, as I formerly stated to you, has not that decided power over inflammation of *mucous* tissues which it possesses over the adhesive inflammation to which serous membranes are liable. Nevertheless, if there be much fever, a hard pulse, and great oppression of the breathing, and particularly if these symptoms present themselves in a young, strong, and robust individual, it will be proper to take away blood. And you will always find blood-letting *relieve* the symptoms; even when its ultimate effect may



FIG. 43.
Injection and stasis in the vessels of the bronchial mucous membrane, in bronchitis, seen by a low power. The vessels were disposed in longitudinal clusters, united by transverse inosculations.

be injurious. The patient's distress arises from his inability to supply air enough to arterialize the venous blood which is carried to his lungs; and by diminishing the quantity of blood in those organs, you will, *pro tanto*, mitigate his uneasiness. But a great part of the danger to be apprehended in the advanced periods of the disease, is that the patient may not have muscular power enough to disembarass his air-

passages of the phlegm that overloads them; to draw a strong breath, and to achieve a vigorous cough. And this danger must be borne in mind in our earlier curative efforts. After much thought and observation, I have come to the conclusion that in these cases it is more safe, and not less effectual, to take blood from the surface of the chest by cupping, than to open a vein in the arm. The space between the scapulæ is the most eligible spot for the application of the cupping-glasses; but they may also be placed upon the front of the thorax. The amount of blood to be thus abstracted, and the question of repeating the cupping, must be determined by the condition of the patient's pulse, which supplies a better measure of the propriety of depletion than is furnished by the local symptoms.

After the bowels have been cleared by a mercurial purgative, calomel and jalap for example, you will find the tartar emetic a very valuable medicine in these acute cases of bronchitis. It should be given in such doses as will excite nausea. and if vomiting be occasioned, you may still go on with the medicine after the sickness has subsided. The depression which this substance produces is great, but it is temporary, and it is effected without expending blood. With the antimony—I mean during the same period—mercury may and ought to be given: to this combination I should be inclined to trust more than to any other internal treatment.

If symptoms of debility and sinking have begun to show themselves, it will be necessary to administer stimulating expectorants. I presume that the carbonate of ammonia, which is often extremely useful in such cases, acts as an expectorant, by giving a fillip to the muscular power. But it is supposed by some persons to exercise some specific influence upon the bronchial membrane. However this may be, five or six grains of it, given in solution every four or six hours, are often followed by free expectoration and a marked improvement.

One of the circumstances of which patients are much disposed to complain, is the violent or importunate cough; and another is, the want of sleep and of rest: indeed the one of these is often, in a great measure, the cause of the other:—the urgency or frequency of the cough prevents the patient from sleeping. Now there is nothing so well calculated to allay cough, and to procure sleep, as opium; and you will be strongly tempted to give these patients opiates, and you will probably be encouraged to do so by the success which will follow that practice in many cases. The good effects of a full narcotic at bed-time are sometimes very striking. Patients who for previous nights have been perpetually harassed by cough, and who are worn out by the disturbance of their rest, will sleep tranquilly, and in the morning expectorate largely and freely, and declare themselves wonderfully the better for their opiate. Yet opium is a ticklish remedy in these cases. Many a patient—some within my own knowledge—labouring under general or extensive bronchitis, have been put so soundly to sleep by a dose of opium on going to bed, that they have not waked again. I believe you may receive it as a golden rule, not to give opium—I mean in a full dose, so as to force sleep,—if you see any venous blood mingling in the general circulation, —if the complexion be dusky, and the lips in any degree blue. The circulation of half-artierialized blood through the brain is in itself a powerful cause of coma; and if you add the influence of an opiate, the coma may easily be made fatal. While the cheeks and lips remain florid, and when the first violence of the disease has abated, an opiate will do capital service. It is a common practice to combine it with antimony or some other expectorant. Twenty minims of laudanum, with the same quantity of the vinum antimonii potassio-tartratis; or a third of a grain of the acetate of morphia, with a drachm of oxymel of squills; are convenient forms.

Counter-irritation is frequently of great use, as an auxiliary measure, in the treatment of acute bronchitis. Sensible relief of the cough, and of the oppressed breathing, often follows the rising of a large blister laid across the front of the chest. When the dyspnœa is extreme, and a more speedy counter-irritant is required, you may have recourse to the mustard poultice. Inhalation of the steam of hot water is also very soothing and useful. It is one of the best expectorants I know of, when it answers at all; but to some persons it proves irritating, and they derive no comfort from it.

I have been speaking of acute bronchitis, uncombined with any other pulmonary disease; and it is curious how little disposed the inflammation often seems to be to extend itself from the mucous membrane to the neighbouring tissues. The reason, doubtless, is, that this membrane is furnished with a distinct set of blood-vessels, the

bronchial arteries, and veins: while the substance of the lungs is supplied by the pulmonary. We could not tell, merely by attending to the general symptoms, whether the inflammation was limited to the inner membrane or not; but by making use of the sense of hearing, we are able to determine this. If the inflammation should spread to the parenchymatous texture of the lungs—*i. e.* if the bronchitis should pass into pneumonia,—this circumstance would be disclosed by physical signs, which I shall in due time describe and explain; and it would demand certain modifications of our plan of treatment.

In the later stages of acute bronchitis, and in various disorders of the bronchial membrane, a peculiar condition of the lung is apt to result, mechanically, from obstruction of the air-tubes by inspissated mucus. This condition is one of great pathological interest; but it was not recognised, or if recognised it was not clearly understood, till of late years. It has been well described and explained by some modern French and German writers. The best English accounts of it to which I can refer you are those of Dr. William Gairdner, as it occurs in the bronchitis of adult life—of Dr. West, as it is modified somewhat in the lungs of children—and of Dr. Alderson, in its connexion with whooping-cough.

The condition of which I speak is that in which portions of the lungs are completely exhausted of air. Naturally, you know, the healthy lung contains a residual quantity of air even after the most forcible act of expiration. When taken from the body the healthy lung is moist, has a spongy elastic feel, and crackles slightly under pressure. But through the operation of disease, portions of the living pulmonary tissue may become as thoroughly devoid of air, as the whole of that tissue is in infants who have never breathed. Those empty portions are firm, tough, dry, of a dull red colour, and they sink when placed in water. The surface exposed by their section looks to the naked eye like a piece of muscle. Hence they are sometimes said to have become *carnified*. This state has been confounded with, but is very different from, a morbid state that I shall soon have to describe, in which the lung, from its resemblance in texture to liver, is said to be *hepatized*; in which there is the same dull red colour, and the same absence of crepitation under pressure, but the affected tissue is friable, and its cut surface moist and granular. In the one case the air cells are empty; in the other they are choked up with the exudations proper to inflammation: in neither case do they contain air.

This empty condition of the pulmonary substance may result from the expulsion of the air by pressure from without: as when the lung is pushed firmly against the ribs and vertebral column in pleurisy attended with liquid effusion—to be described hereafter. The lung thus compressed and looking like flesh has been not unaptly called *carnified lung*. But the cause of the absence of air is more often internal, and consists in the plugging up of one or more air tubes; and then *collapse* of the lung is said to have taken place: and this is the more common and the more correct term.

The mechanism of this collapse is very simple and intelligible. Small portions of tough and sticky mucus are driven to and fro in the larger air tubes during the alternate acts of respiration, or in the paroxysms of a cough. Mostly they are expelled by the cough which their presence provokes. But it may happen, and it often does happen, that during inspiration one of these pellets, forced strongly backwards in a tube which gradually becomes smaller and smaller, shuts up that tube, and all the smaller branches that proceed from it beyond the place of the obstruction. In expiration the plug moves a little outwards again, so as to allow a part of the imprisoned air to escape; but returning in inspiration, it does not permit any fresh air to enter. Repetitions of this process exhaust, or nearly exhaust the air from the portion of lung thus mechanically sealed up. The portion so exhausted suffers collapse.

Collapse of the lung may be *diffused*, or *lobular*.

In the first of these forms, which is also the most common of the two, the collapse extends over a considerable portion of one lung, or of both lungs. The collapse may not be complete, nor the absence of air total. The affected piece of lung may still therefore float in water. Its colour, which varies according to the quantity of blood contained in it, is usually a dark, brownish red, gradually shading off sometimes into the hue of the adjoining lung. This diffused collapse is generally met with in the posterior parts of the lungs.

The second, or lobular form of collapse, is more sharply defined, and its well-marked

outline strikes the eye at once. The affected lobules, or bunches of lobules, occupy the anterior edges of the lungs, as well as other situations. They are slightly depressed below the general surface of the lung. Seen through the pleura, they have a dark red or violet colour; and when cut into they present a brown or mahogany hue. This form of collapse occurs chiefly in the lungs of children, and it was formerly regarded as the consequence and evidence of lobular pneumonia. The condensation is not due, however, to present or to by-gone inflammation, but simply to the absence of air. When the change is of recent date, the collapsed portions may be restored to their natural volume and condition, by blowing air into them through their proper bronchial tubes.

When collapse has taken place to any considerable extent during an attack of bronchitis, the breathing is apt to become laborious. The act of inspiration is performed with effort, difficulty, and distress; while that of expiration is comparatively easy. The patient is unable to lie down. All the muscles accessory to the inspiratory movements are called into play, yet little air finds entrance. Dr. Gairdner speaks of this "long-drawn, exhausting, inadequate inspiration, as being probably peculiar to obstructive bronchitis." The paroxysms of severe dyspnoea incidental to bronchial disorders are doubtless often owing to casual changes in the position of portions of tenacious mucus in the air tubes.

You will bear in mind this condition of collapse, and the laboured respiration associated with it; for I shall have to point out to you a striking contrast, in respect to the manner of breathing, when we come to the consideration of pure pneumonia.

Collapse of the lung, in proportion to its extent and its proximity to the surface, must modify, and in some degree lessen, the resonance of the chest under percussion. When such modification of the stroke-sound springs up in the course of an attack of bronchitis, and the patient's respiration becomes at the same time laborious, these conjoint symptoms will generally suffice to reveal the presence and the character of what may be called an accident of the disease. From this accident, when it is recent and uncomplicated, both reason and observation teach us to believe that the lung may recover, upon the removal of the obstruction, and the consequent readmission of air. And it seems probable, as Dr. Gairdner has suggested, that the muscular contractions of the bronchial tubes themselves have frequently a greater share than the movements of respiration, in promoting the expulsion of the accumulated mucus.

A similar accident may sometimes convert an apparently slight attack of bronchitis into a most perilous and quickly fatal malady. A large plug of tenacious mucus may all at once enter and stop up the principal bronchus of one, or of the other lung. Sudden and urgent dyspnoea ensues, and unless the plug be dislodged, the patient may perish before any collapse can be effected. Instantly, in that portion of the lung to which the bronchus conducts, all sound of respiration ceases: yet over this same portion of lung, in which no sound, healthy or morbid, is heard by the ear applied to the thorax, percussion gives the natural hollow sound. If the obstructive mass be fortunately expelled, or displaced, in the desperate struggle for breath, the sound of respiration is re-established as suddenly as it had previously disappeared: and the dyspnoea also ceases. In some cases, however, the noise of the pulmonary expansion does not so return, the difficulty of breathing increases, suffocation becomes imminent, and death by apnoea takes place rapidly.

Andral relates two instances of death from this cause; one of which was the following. A coachman, fifty years old, had been several times a patient in La Charité, for obstinate pulmonary catarrh, with slight dyspnoea, and puriform expectoration. Every time he went away relieved, but not cured. On both sides of his chest could be heard all the varieties of rhonchus. In one spot the column of air which penetrated the bronchi imitated the snoring of one in a deep sleep; in another spot it was like a dull and prolonged groan; in a third, a sound resembling that made by bellows, and in a fourth the cooing of a turtle-dove, were exactly simulated. On the last occasion of his entering the hospital, his respiration was still tolerably free. One morning he was found in a state of unusual anxiety. In the middle of the night, after a violent paroxysm of cough, his breathing (he said) had suddenly become very much oppressed. It was discovered, on auscultation of his chest, that no air penetrated into the upper lobe of the right lung; yet that part sounded well on percus-

sion, even louder than the corresponding part of the other side, which was morbidly dull. The difficulty of breathing went on augmenting, and the man was soon dead.

Besides other marks of disease in the lungs, the primary bronchus leading to the upper lobe on the right side was closed up completely by tough mucus, and exhibited the appearance of a full cylinder.

In the other case, also, the obstructed bronchial tube supplied the upper lobe of the right lung.

It may seem strange that the interruption of the access of air to so small a portion of the lungs should be attended with such serious consequences, when we know that the greater part of each lung may be impermeable by air, and yet the patient live a long time, and often without any great dyspnoea. The explanation of the apparent difficulty seems to be, that in the one case the prevention of the arrival of air in the part affected is sudden, in the other gradual. Moreover, the remaining portions of the lungs are performing their functions imperfectly.

When once attention has been awakened to the kind of accident just mentioned, the diagnosis would not seem to be difficult. We may suspect obstruction of one of the bronchi when considerable dyspnoea comes on suddenly during the continuance of simple bronchitis: and our suspicion will be confirmed if at the same time respiration ceases to be audible in a certain portion of the lung, while the sound given by percussion over the same part remains unaltered. Emphysema of the lung (which I shall explain hereafter) is the only other condition which could give rise to similar physical signs.

Andral judiciously suggests the employment of emetics, and the inhalation of steam, in such cases.

I shall have to speak of some varieties of chronic bronchitis; but there is a mixed form of pulmonary disease that requires to be noticed, in which acute or subacute inflammation engrafts itself upon changes that are chronic and abiding. Sydenham calls the disorder to which I now refer *peripneumonia notha*—bastard peripneumony. *Catarrhus senilis* is another of its names. It may be considered as chronic bronchitis, occurring in old persons, and very apt to be converted into pneumonia, or to be greatly aggravated in degree during winter, or upon any accidental exposure. This is the common complaint of persons advanced in life. I mention it here in compliance with the usual custom, and because this is as convenient a place for its introduction as any. But it would be an error to regard it as exclusively a disease of the mucous membrane of the lungs. An habitually congested state of that membrane, marked by some shortness of breath, and some expectoration, and by the constant presence of some degree of crepitation in the lower parts of the lungs,—these are circumstances which are of daily occurrence as consequences of *disease of the heart*; and it is in persons whose habitual health is of this kind, that what is called *peripneumonia notha*, which implies a diffused inflammation of the pulmonary mucous membrane, with sometimes an enormous secretion from its surface, is most apt to supervene. Almost all such persons will tell you that there are periods at which they experience slight febrile attacks, and exacerbations of their complaints: they have pain in the breast or side, headache, heat, and thirst; and at these periods the cough and expectoration are always aggravated, and continue for some time to be more than commonly severe. “The disease (says Cullen, who, following Sydenham, has given a good description of its general symptoms) has often the appearance only of a more violent catarrh; and after the employment of some remedies, is entirely relieved by a free and copious expectoration. In other cases, however, the feverish and catarrhal symptoms are at first very moderate, and even slight; but after a few days these symptoms suddenly become considerable, and put an end to the patient's life, when the indications of danger were before very little evident.”

The truth is (and we learn the truth by the evidence of auscultation), that in these cases *pneumonic* inflammation is often suddenly set up. There is no security, as Dr. Latham observes, that the portions of lung which yield crepitation to-day may not be solid and impervious to-morrow. Dr. Latham is of opinion that in this disease the inflammation is apt to travel over the bronchial membrane from place to place, as erysipelas is seen sometimes to wander over the surface of the body. I know not how this may be; but certainly death is often produced in these persons by the sudden

spoiling of even a moderate portion of lung; or by its sudden *closure*. For it is more than probable that, in many of these cases, the distressful symptoms result (especially when they bear no inflammatory character) from the rapid accession of pulmonary collapse. In their ordinary condition, the patients have just enough, and no more, of the respiratory apparatus in an effective state, whereby to subsist; and when a fresh part of it is rapidly rendered solid and useless, they quickly perish. But they die also from another cause. The nicety of treatment which I spoke of as being required in certain stages of acute bronchitis, is still more apparent and necessary here. We are placed in this dilemma. If we do not take blood in the inflammatory attacks, we run a risk of losing our patient from the effects of the unchecked inflammation; and if we do bleed, we are in danger of losing him by producing a degree of weakness which will render him unable to expectorate the effused mucus, and so liable to perish by suffocation. Cupping and blisters are the remedies most suitable when there is evidence of recent inflammation — what are called expectorants and perhaps emetics, when we have reason to suspect collapse. Medicines which are at the same time diuretic are also serviceable — the spiritus ætheris nitrici, the preparations of squill, and of digitalis.

LECTURE XLIX.

Influenza. Symptoms and progress. Conjectures as to its cause. Treatment. Hay asthma. Chronic Bronchitis. Its varieties. Morbid anatomy of these affections. Dilatation of the Bronchi.

CATARRH, which was the principal subject of the last lecture, occasionally prevails far and wide as an epidemic disease. I speak, indeed (February 4, 1837), during the immediate presence of one such visitation, although the extreme violence of the complaint that has been raging among us is now fast subsiding. You can hardly be without curiosity to know what has been learned respecting an influence which has thus, on a sudden, before your eyes, disturbed and sickened a whole community. I have here used, without thinking of it, the very word by which, in a foreign version, the disorder is denominated. It has received, however, various names; for it has been known and noticed from remote antiquity. Cullen calls it *catarrhus e contagio*: and under that head, in his *Nosology*, you will find a copious reference to recorded accounts of epidemic catarrh, as it has been observed to spread over great portions of the world. In France the disorder thus prevailing, is styled the *grippe*. The Italians, putting the cause for the effect, call it *influenza*, the *influence*: and this last term influenza has now become naturalized in our language. Since Cullen wrote there have been four or five more of these epidemics. One in 1782, which extended over all Europe, visiting every country therein, affecting more than one-half of its inhabitants, and proving fatal to very many of them. You will find, in the third volume of the *Transactions of the College of Physicians*, a good account of the disease as it then showed itself in this country. In the spring of 1803 another instance of it occurred; and of this the history, as compiled from the testimony of a hundred and twenty-four observers, is preserved in the ninth and tenth volumes of the *London Medical and Physical Journal*. In the month of April, in the year 1833, the influenza again made its appearance, and prevailed extensively, both here and elsewhere. and of the influenza of 1837 you have had, and you still have, the opportunity of being observers. A very good and instructive sketch of this epidemic malady, compiled by Dr. Hancock, is to be found in the *Cyclopædia of Practical Medicine*. To that article, to the publications I just now mentioned, and to the works enumerated by Cullen, I may refer you for much which is curious and interesting in the history of the disease; but which would not be so well adapted to our immediate purpose in this place — namely, that of seizing upon the practical facts which have been ascertained respecting influenza.

One characteristic feature of this species of catarrh, as distinguished from the ordinary sporadic disorder, is the sudden occurrence, in the outset, of more decided febrile disturbance. The first two patients whom I saw in the epidemic of 1833 had just the symptoms which frequently mark the commencement of an attack of continued fever; and I did not know, at my first visit, what was about to happen to them. The symptoms, taking them altogether, are somewhat as follows. The patient is chilly, and perhaps shivers; presently headache occurs, and a sense of tightness across the forehead, in the situation of the frontal sinuses; the eyes become tender and watery; and sneezing and a copious acrid defluxion from the nose ensue, followed or accompanied by heat and uneasiness about the throat, hoarseness, a troublesome cough, and oppression of the breathing. In short, the symptoms are the symptoms of catarrh; including in that term, all the varieties thereof that are sometimes met with separately—*gravedo*, *coryza*, *bronchitis*: and with these symptoms, a sudden, early, and extraordinary subdual of the strength; and, most commonly, great depression of spirits. The debility which comes on at the very outset of the complaint is one of its most singular phenomena, taking place, in some cases, almost instantly, and in a much greater degree than would seem proportioned to the other symptoms of the malady which it thus ushers in. Indeed, this rapid and remarkable prostration of strength is more essentially a part of the disorder than the catarrhal affection, which sometimes (though rarely), is absent, or imperceptible. It is upon the mucous membranes, however, that the stress of the disease generally falls; especially upon the internal lining of the air-passages. Those of the alimentary canal seldom escape entirely; but they suffer in a less degree. The tongue is white and creamy, the palate loses its sensibility, the appetite fails, nausea and vomiting are not uncommon, and sometimes there is diarrhœa. The pulse, in the uncomplicated disease, is soft, and generally weak. The skin, at first hot and dry, soon becomes moist, and sometimes exhales a peculiar musty smell. The patients complain also of pains in the limbs and back, and of much soreness, a bruised, fatigued, or tender feel, along the edges of the ribs, and in various parts of the body.

In its simple form and ordinary course, the disease abates of its violence after two, three, or four days, and the patient is usually convalescent before the termination of the week: but cough and much debility are apt to survive the other symptoms, and while these continue, the complaint is very easily renewed. Pre-existing disease, and peculiar constitutional habits and tendencies, modify considerably the character of the influenza, as it affects different persons. It is apt to be complicated with *bronchitis*, with *pneumonia*, with *rheumatic* affections of the joints, with *neuralgic* pains. I do not attempt to represent in detail its various phases; they are fit subjects of study for yourselves.

I have remarked that Cullen makes this species of catarrh to proceed from contagion. But the visitation is a great deal too sudden and too widely spread to be capable of explanation in that way. I will not say that the disease may not be in some degree infectious; for there is reason to believe that other epidemic disorders, having many points of analogy with the influenza, are somehow imparted from one individual to another, although they are mainly produced by some influence which resides in the atmosphere. There are facts in the history of influenza which furnish a strong presumption that the exciting cause of the disorder is material, not a mere quality of the atmosphere; and that it is at least *portable*. The instances are very numerous, too numerous to be attributed to mere chance, in which the complaint has first broken out in those particular houses of a town at which travellers have recently arrived from infected places. But this great and important question of contagion I hope to examine with you more rigorously on a future occasion. What I wish to point out now is the fact that the influenza pervades large tracts of country in a manner much too sudden and simultaneous to be consistent with the notion that its prevalence depends exclusively upon any contagious properties that it may possess. You are aware that it has recently seized upon all parts of this metropolis—and I believe I may say of nearly the whole kingdom—within the space of a very few days. It has been observed to occur also, at the same time, on land, and on board different ships which have had no communication with the shore, nor with each other. Thus it is stated in the *Transactions of the College of Physicians*, that on the 2d of May, 1782, Admiral Kempenfelt sailed from Spithead with a squadron, of which the *Goliath* was

one. The crew of that vessel were attacked with influenza on the 29th of May; and the rest were at different times affected, and so many of the men were rendered incapable of duty by this prevailing sickness, that the whole squadron was obliged to return into port about the second week in June, not having had communication with any shore, but having cruised solely between Brest and the Lizard. This happened in one part of the fleet. In the beginning of the same month, another large squadron sailed, all in perfect health, under Lord Howe's command, for the Dutch coast. Towards the end of the month, just at the time therefore when the *Goliah* became full of the disease, it appeared in the *Rippon*, the *Princess Amelia*, and other ships of the last-mentioned fleet, although there had been no intercourse with the land. Similar events were noticed in the epidemic of 1833. One or two curious instances of the sudden sickening of considerable bodies of men in different places at the same time, were related to me on good authority. On the 3d of April in that year—the very day on which I saw the first two cases that I did see of the influenza, all London being smitten with it on that and the following day—on that same day the *Stag* was coming up the channel, and arrived at two o'clock off Berry Head, on the Devonshire coast, all on board being at that time well. In half an hour afterwards, the breeze being easterly and blowing off the land, 40 men were down with the influenza, by six o'clock the number was increased to 60, and by two o'clock the next day to 160. On the self-same evening a regiment on duty at Portsmouth was in a perfectly healthy state, but by the next morning so many of the soldiers of that regiment were affected by the influenza, that the garrison duty could not be performed by it. I make no doubt that facts of a like nature have occurred during the present epidemic, and will be made known in due time. They illustrate several important points in respect to the disease: viz., the impossibility of accounting for its prevalence upon the principle of mere contagion—the suddenness of its invasion—and the early and extreme prostration of strength with which it is attended.

[In 1733 the influenza commenced simultaneously in London and in Flanders during the first week of January; at Paris, about the middle of the same month; and in Ireland, towards its termination; at Leghorn, about the middle of February; and at Naples and Madrid, near the end of the month. The same year it made its appearance in America, about the middle of October, first in the New England States; and was soon afterwards prevalent in the islands of Barbadoes and Jamaica. In a few months it is said to have extended to Mexico and as far South as Peru. In 1789, the disease made its appearance first in New York, in the month of September, and was prevalent during the same month in Philadelphia. Soon after, it spread over the whole of the Eastern and Southern States, and to the army, in the northwestern territory, under the command of General Wayne. About one month subsequently, it made its appearance at Grenada.

In its several visitations in this country it has usually commenced in one of the eastern States, and extended southwards along the seaboard, with more or less rapidity. In 1807, however, it showed itself first in New York, spreading thence, as from a centre, in every direction. It reached Canada, on the north, and extended to the Southern and Western States, in the course of three months. The amazing rapidity with which it diffused itself over the greater portion of the American continent, resembled more the fleetness of the wind than the natural course of a disease. Almost the entire population of a city, town, or neighbourhood, became in a few days subjected to its influence; and as it seldom incapacitated the majority of those affected by it from pursuing their ordinary occupations, there was heard in every street and place of resort, so much coughing, hawking, and wheezing, as to interrupt conversation; while in public assemblies the voice of the speaker, itself scarcely audible from the hoarseness produced by disease, was completely drowned by the coughing concert kept up by his auditory. In the subsequent occurrence of the influenza in our midst, nearly the same facts have been observed—very shortly after its first appearance in Philadelphia the greater part of the inhabitants of the city and surrounding country, became affected with it to a greater or less extent, and within a very short period it had attacked the inhabitants of nearly every portion of the United States.—C.]

The occurrence of epidemic catarrh, as well as of most other epidemics, is unques

tionably connected with some particular state or contamination of the atmosphere. What that state is, or what may be the kind of contamination, no one knows. The present epidemic followed hard upon the sudden thaw that succeeded the remarkable snow storm of the final week of the last year. A similar coincidence between the breaking out of the same disorder, and a sudden elevation of the temperature of the atmosphere, happened at St. Petersburg in the epidemic of 1782. "On a cold night (Maertens says), the thermometer rose 30° of Fahrenheit; the next morning 40,000 people were taken ill with the influenza." Now if every epidemic had been preceded by similar changes in the weather, we might resolve the universal prevalence and sudden accession of the complaint, into the effect of the cold and damp state of the air, produced by the thaw. But it is not so; for, as Dr. Hancock observes, there has not been any uniform connexion between any one sensible quality of the atmosphere as to heat or cold, rain or drought, wind or calm—and the invasion of the epidemic. "Et tempore frigidiori et calidiori, et flante tam Austro quam Boreâ, et pluvioso et sereno cælo, peragravit hæc omnes Europæ regiones, et omnia loca indiscriminatim." Irregularities and great vicissitudes of weather have however gone before the disease in very many instances: but sometimes one condition of the atmosphere, and sometimes another, has been its immediate forerunner: and the epidemic has frequently been observed to fall partially and capriciously; as a blight falls upon a field or district. Short, in his chronological history of the weather, says that thick ill-smelling fogs preceded, some days, the epidemic catarrh of 1557. Jussieu states that the gripe of the spring of 1733 appeared in France immediately after offensive fogs, "more dense than the darkness of Egypt." So also in 1775, Petit informs us that in France the disease was ushered in by thick noisome fogs. In the same year it visited the shire of Galloway in Scotland, where, we are told, "a continual dark fog and particularly smoky smell prevailed in the atmosphere for five weeks, the sun being seldom seen." Dr. Darwin has recorded that, in 1782, "the sun was for many weeks obscured by a dry fog, and appeared red as through a common mist:" and he supposes that "the material which thus rendered the air muddy probably caused the epidemic catarrh which prevailed in that year." You will call to mind here the dark fog which brooded over this city in the midst of the raging of the distemper about ten days ago, and which was repeated in a less degree, on Wednesday last (Feb. 1).

[Notwithstanding the influenza has occurred at all seasons of the year, whether hot, cold, damp, or otherwise; in every state of the barometer and hygrometer, still, if we cast our eyes over the various histories of its different visitations in Europe and America, it will be found that the weather immediately preceding, or during its occurrence was extremely unseasonable, or, at least, was marked by sudden alternations from cold to warm, or from dryness to moisture, or the reverse. Thus in 1675, we are informed by Sydenham, that the disease made its appearance in the end of October, at which period, the weather which had before been unusually warm, became suddenly cold and damp. In the winter of 1729, Dr. Gilchrist states, that at the time when the influenza occurred, the weather was thick, warm, and rainy. In 1762, according to Monro, the disease appeared in April, after a sudden change of weather from cold to extreme heat. In 1789, the weather preceding the visit of the influenza, is stated by Dr. Currie (*Diseases of the United States*) to have been calm, misty, and warm; and in the month of August, just as the disease made its appearance, we are told by Dr. Rush (*Med. Essays and Observations*) it changed suddenly to a degree of cold uncommon at that season of the year. In 1790, according to the last mentioned writer, the influenza appeared during an extremely variable winter—the weather suddenly changing from cold to warm, and the reverse. In 1807, we are informed by Dr. Currie (*Op. citat.*) that the Influenza made its appearance during a season marked by sudden changes of temperature, but throughout more wet and cold than usual. The period of its visit in 1816 was marked by very variable weather, and a mild and open winter. In 1826 it occurred towards the close of winter, during a thick, damp, and unusually mild state of the atmosphere; and the very severe epidemic of 1831 was preceded by great and sudden variations of atmospheric temperature followed by an unusually early winter.

Van Swieten, in his Commentary on the 1407 aphorism of Boerhaave, mentions the dependence of influenza upon a thick, vapory state of the atmosphere, and so far as

we have been able to obtain access to accurate accounts of the weather during those years and seasons when the influenza has prevailed, we have found it to be marked by variableness and moisture, or by sudden transitions from a degree of unusual heat to the opposite extreme.

It has been supposed that the occult morbid constitution of the atmosphere gives the predisposition to the disease, and impresses upon it its peculiar character of early and extreme depression, while the catarrhal symptoms are occasioned by the sensible properties of the atmosphere, especially by frequent and rapid changes in its temperature. However this may be, it is at least very certain that the latter increase the number of those attacked by influenza, and augment very materially its intensity. That during a season in which the influenza prevails, particular winds, and atmospheric vicissitudes are alone capable of producing the disease cannot, however, be doubted. In the account of the influenza, which prevailed in England in 1803, by Dr. Carrich, we learn that the inhabitants of that side of Richmond Terrace, on Clifton Hill, near Bath, which fronted the east, were universally attacked with the disease, while the south side, the great majority both of persons and families, in all other respects similarly circumstanced, escaped it entirely. Here the east wind was evidently the direct exciting cause of the influenza. — C.]

It has been observed also, that shortly before, or during, or soon after, the prevalence of these epidemic catarrhs—*epizootic* diseases have raged; various species of brutes, and of birds, have been extensively affected with sickness: while on some occasions prodigious swarms of insects have made their appearance. In short, a great variety of facts concur to render it probable that some peculiar condition of the air existed, which, though it might be favourable to the multiplication of some species of living creatures, such as the insects just referred to, operated as a poison upon the human body, and upon the bodies of many of the brute creation.

It is a very curious circumstance in the history of epidemic catarrh, and worthy of your reflection, that they *travel*; migrate as it were from one place to another; and moreover, that they hold, for the most part, to certain courses, in spite even of opposite winds, and of variations of temperature. It has been noticed that the influenza generally follows a westerly direction, or one from the south towards the north-west. In this remarkable property it resembles, as you may perhaps be aware, the epidemic cholera.

Although the general descent of the malady is, as I have said, very sudden and diffused, scattered cases of it, like the first droppings of a thunder shower, have usually been remembered as having preceded it. The disorder is most violent at the commencement of the visitation; then its severity abates; and the epidemic is mostly over in about six weeks. Yet the morbid influence would seem to have a longer duration. In a given place nearly all the inhabitants who are susceptible of the distemper suffer it within that period, or become proof against its power. But strangers who, after that period, arrive from uninfected places, have not, apparently, the same immunity.

The locality does not appear to be thoroughly cleared of the poison for some time: or perhaps a more cautious statement of the fact would be, that the disorder generally shows itself again in succeeding years, but in a milder and less general form. This must depend either upon some remaining dregs, or possibly some revival, of the injurious influence; or else upon some abiding predisposition impressed upon the bodies of men by its former visit. You may hear, every year, of Mr. *So-and-so* having the influenza. In many instances, no doubt, common sporadic catarrh is dignified by that name; but it is certain also, that many of the colds, and bronchial disorders, of the seasons which immediately follow a period of genuine influenza, are attended with much more languor, debility, muscular aching, and distress, than belong to an ordinary attack of catarrh.

All this is very curious and very mysterious. All this, or much of it, is true also of all the diseases which are known to prevail occasionally as epidemics. The facts that have now been mentioned respecting the influenza, warrant, I think, the conclusion that it does *not* depend upon any mutations in the ordinary and obvious qualities of the atmosphere: upon any degrees or variations, I mean, of its temperature, its motions, or its moisture; upon what is expressed in the single word *weather*. Con-

cerning a calamity so generally felt, and so obscure in its origin, conjecture, you may well believe, has not been idle. One hypothesis assigns the complaint to some change in the electrical condition of the air: to its becoming negatively electric: or to its being such as to cause an excessive accumulation of electricity in the animal economy. The facts adduced in support of these views are of this kind. Meat, sent up by means of a kite, high into the atmosphere, during the prevalence of the disease, has returned putrid. Large heavy separate clouds, in a state of negative electricity, have been observed just before the setting in of an epidemic. Thunder-storms, and tumults of the atmosphere, have occurred at the same periods. During the raging of one epidemic, 300 women engaged in coal-dredging at Newcastle, and wading all day in the sea, escaped the complaint. It has been thought that this exemption might be accounted for by supposing that the almost constant immersion of the body in a conducting medium prevented any undue collection of electricity.

Again, it has been conceived that the tolerably definite course of the epidemic, in its migrations, might be somehow connected with magnetic currents.

One of the most recent and most plausible conjectures respecting the exciting cause of influenza, is that which refers it to the presence in the atmosphere of an excessive quantity of *ozone*. The attention of physicians was first directed to this substance by M. Schönbein of Basle, in a paper which you will find in the *Medico-Chirurgical Transactions* for 1851. Pure or atmospheric oxygen, when exposed to the action of electrical sparks, is transformed into an odoriferous matter, which is therefore called ozone, and which is believed to be merely an allotropic form of oxygen. Most persons who have stood near an electrical battery at the time of its discharge must have been sometimes aware of the peculiar smell. The same odour pervades the air in thunder-storms. Now this ozone has remarkable purifying properties, which I need not stop to describe. It has also the effect, when breathed in large quantities, of irritating the mucous membrane of the air passages. While M. Schönbein was engaged in examining its chemical relations, he found that the inhalation of strongly ozonized air produced a painful affection of his chest—a sort of asthma, with a violent cough, which obliged him to discontinue for a time his investigations. Reflecting on this circumstance, he began to suspect that certain catarrhal disorders might be caused by atmospheric ozone. He got several physicians at Basle to compare their lists of catarrhal patients with his tables of atmosphero-ozonometric observations; and he and they were struck by the occurrence of an unusual number of catarrhal cases, on the days, or during the periods, when M. Schönbein's test papers showed that ozone was unusually abundant in the air.

This presumable explanation of epidemic catarrhs deserves, and doubtless will receive, a searching scrutiny, whenever the community may again be afflicted with influenza.

Another hypothesis, more fanciful perhaps, at first sight, than these, yet quite as easily accommodated to the known phenomena of the distemper, attributes it to the presence of innumerable minute substances, endowed with vegetable or with animal life, and developed in unusual abundance under specific states of the atmosphere, in which they float, and by which they are carried hither and thither. Myriads of these animalcules, or of these vegetable germs, coming in contact with the mucous membranes, and especially with that of the air-passages, irritate (it is imagined) those surfaces, and exercise a poisonous influence upon the system. Now the sporules of certain fungi, which ruin the health, and destroy the vitality of larger plants, on which they prey, are inconceivably small. I shall prove to you presently, that vegetable effluvia are capable of producing, in the human body, symptoms not very dissimilar from those of influenza. Again, that the waters of this globe swarm with living creatures, which are invisible by our unaided eyes, the microscope has taught us. Others, too minute to be estimated even by that wonder-showing instrument, in all probability exist. We cannot doubt that the gaseous fluid which surrounds this planet, equally teems with living atoms. We know that multitudes of insects, and of cryptogamous plants, infinite in number with respect to our finite powers of computation, are sometimes suddenly hatched or developed, in places which were previously free from them. It is easy to conceive that atmospheric infusoria (so to speak) may rapidly congregate, or vivify, in masses sufficient to render deleterious the very air we breathe. If this be so, we can understand how such a cause of disease may first

act here and there, and presently overspread large districts; how it may move, or be wafted from place to place, or be carried about by persons; how its course and operation may be circumscribed and definite; and how some germs or ova may remain after the visit, retaining their vitality, and ready in future seasons again to start into life and activity under favouring circumstances. Taking the insect hypothesis, and knowing, as we do, that some animal poisons, (that of small-pox, for example,) have the singular property of multiplying themselves in the human body, like yeast in beer, we may conceive that diseases, produced by animalcules, may thus infect the fluids of the body, and become contagious in the fullest sense of that term. Lastly, the uniform duration of these epidemics has been supposed to add probability to the notion that they result from the operation of some organic principle, which has its definite periods of growth and decay. All this is sheer hypothesis: but I have nothing better than hypotheses to offer you. You may choose from among them, or you may reject them all, as the bent of your minds may incline. That which most commends itself to my own acceptance, and which may also be most easily put to the test when the opportunity shall arise, is the *ozone* hypothesis.

The character of debility, which is so conspicuously impressed upon this disease, bears closely upon the treatment required for its cure, or its safe conduct. As in all other epidemics, the severity of the complaint is extremely variable in different persons. In some it proves a very trifling malady, which soon passes off, and requires little or no assistance from medicine. In others it is a very distressing affection, and lays the foundation for other and still more serious, though more chronic diseases: and in some, and more especially in the old and the unsound, it shows itself a very fatal disorder. The absolute mortality under the recent epidemic has been immense: the daily newspaper obituaries have been unusually long; and you may have remarked, that the ages of the persons whose deaths they announced were in almost all cases great. The funerals have been so frequent, that difficulty has been found in performing them without indecent hurry and confusion. One undertaker, of whom I was informed, had at one time 75 dead bodies to inter—*above ground*, as he expressed it; and mourning coaches, and black horses, could not be procured in sufficient numbers to meet the demand for them. The absolute mortality, therefore, I say, has been enormous; yet the relative mortality has been small. You will hear people comparing the ravages of the influenza with those of the cholera, and inferring that the latter is the less dangerous complaint of the two; but this is plainly a great misapprehension. Less dangerous to the community at large (in this country at least) it certainly has been; but infinitely more dangerous to the individuals attacked by it. More persons have died of the influenza in the present year than died of the cholera when it raged in 1832; but then a vastly greater number have been affected with the one disease than with the other. I suppose that nearly one-half of those who were seized with the cholera perished: while but a very small fraction indeed, not more probably than 2 per cent., of those who suffered influenza have sunk under it. The only fatal cases that I have seen have been in persons advanced in life, or in persons whose lungs were previously known to be unsound.

Now the *treatment* of the influenza is pretty well understood. The chief risk of mistake is that of being too busy with the lancet. Certainly those affected by this disorder do not well bear active depletion. Of course no one would think of blood-letting except the symptoms were severe, and the distress great; but even in such cases, much caution is requisite in adopting that remedy. If you find that the inflammation has extended to the pleura, or to the substance of the lungs, it may be necessary to open a vein, or to apply cupping-glasses over the chest: but this is a very unpleasant necessity. Such is the result of all that I have seen, and heard from others, of the present epidemic; and such is the result of the recorded experience of nearly all previous epidemics. You will find abundant evidence of this collected into a summary view by Dr. Hancock. In 1510, Dr. Short says bleeding and purging did harm. In 1557, bleeding was said to be so fatal, that in a small town near Madrid, two thousand persons died after it in the month of September. In 1580, Sennertus, after stating that where blood-letting was omitted, the mortality was not greater than one in one thousand, adds, "*Experientia enim hoc comprobavit, omnes fere mortuos esse, quibus vena aperiebatur.*" Dr. Ash observes that, in 1775, it was never necessary to bleed at Birmingham; and that, in a neighbouring town, three died who were

bled, and all recovered who were not bled. And a great deal more evidence to the same purpose you may find in the article I have referred to.

I believe the best plan of management—as far as any general plan can be laid down—is to keep the patient in bed, and after clearing the bowels by two or three grains of calomel, followed by a mild aperient, to give a couple of grains of James's powder every six hours, with a saline draught, and slops, till the first brunt of the disorder is over; and then, if the cough be troublesome and the breathing laborious, and much rhonchus, or sibilus, or crepitation be audible in the chest, to apply a blister, and to give expectorants and diuretics. What I prescribed a great many times was something of this kind: half a drachm of oxymel of squills, a drachm of the sweet spirit of nitre, and sometimes another drachm of paregoric, in almond emulsion. With respect to full doses of opium, when the feverishness is abated and the headache gone, I should recommend the same practice which I described in the last lecture. If there be any lividity of the skin, or of the mucous membranes, it is hazardous to give a full dose of opium. On the other hand, if there be no visible indication in the complexion that venous blood is circulating in the arteries, opium given at bed-time will have sometimes a magical effect in relieving distress, and (by giving rest and refreshing sleep) in recruiting the strength also. In cases in which the powers of the system are prostrate, and the face and lips are livid, and the patient is tugging to expectorate the mucus that is filling up his air-passages, you should have recourse to ammonia, to nourishing broths, and it may be to wine and water: and when all danger from the disease is over, but the patient remains feeble, languid, and out of spirits, then is the fit time to administer tonic medicines; and although snake-root and cascarrilla are well spoken of by many practitioners, I know no tonics so good as the sulphate of quina, or of iron, for such patients.

As to external applications, mustard poultices, blisters, and the like—and to the inhalation of the steam of hot water,—these may each and all be very useful; but I have nothing to add concerning the time and manner of their employment to what I said upon the same subject in the last lecture.

There is one point in the treatment which I must not omit to notice, although I cannot tell you much about it from my own experience. Dr. Thomas Davies, an accurate observer, and one well qualified to form a judgment in the matter, states that he found a *mercurial* treatment answer well in severe cases, in the epidemic of 1833. He perceived that active depletion was not well borne, and discovering that in the bad cases there was always crepitation in the lower lobes of the lungs, he thought mercury was one of the most proper remedies to subdue the inflammation, and to occasion absorption of the fluid effused into the air-cells. He had severe cases to deal with. He says that it happened to be his duty to admit the patients into the London Hospital during the week the epidemic was at its height, and that thirty-two beds which were placed at his disposal were all soon filled with individuals labouring under the severe forms of influenza; so severe, that he believed the greater number of them would have perished if they had been allowed to wander about the streets, or even to have remained at their own homes, with the insufficient attention they could there have obtained. Only one or two of these patients were bled, but they were all put under the influence of mercury. This treatment commenced on Thursday, and all who, by Saturday night, were affected in the usual way by the remedy, safely and gradually recovered, with the exception of two; and one of these had hypertrophy of the heart, and diseased aortic valves. His object was not to salivate, but merely to make the gums tender. It was of course necessary that the action of the mercury should be prompt; and he found that the most quick and efficacious way of obtaining it was by rubbing in the linimentum hydrargyri.¹

There is a singular variety of catarrh produced by a peculiar local cause, and therefore requiring to be briefly noticed. I have now seen several unequivocal instances of it; and it has been observed and described by many writers. Dr. Bostock, in the *Medico-Chirurgical Transactions*, gives an account of this complaint as it was apt to attack himself. It is called the *catarrhus æstivus*, and by some the *hay-fever*, or the

¹ Influenza again prevailed in this country towards the end of 1847, and extended into the beginning of the next year. According to Dr. Farr it killed, directly or indirectly, not less than 5000 persons in six weeks.

hay-asthma. In Dr. Elliotson's lectures also, as published in the *Medical Gazette*, there is a good deal of curious information upon this malady, contained in letters addressed to him from practitioners in various parts of the country, in consequence of some previous remarks made upon it by him in a clinical lecture, which had also been printed. Dr. Elliotson speaks of it as a combination of catarrh and asthma. It consists in excessive irritation of the eyes, nose, and the whole of the air-passages; producing, in succession, itching of the eyes and nose, much sneezing occurring in paroxysms, with a copious defluxion from the nostrils; pricking sensations in the throat; cough, tightness of the chest, and difficulty of breathing, with or without considerable mucous expectoration. This complaint affects certain persons only, and in them it always takes place at the same period of the year, in the latter end of May, or in June, when the grass comes into blossom, or when the vernal hay-making is going on. It seems, in fact, to be produced by some kind of emanation from certain of the grasses that are in flower at that season, of the irritating qualities of which emanation some persons only,—and a very few persons in comparison with the entire community,—are susceptible. The disorder happens only at that one particular season; and it then attacks persons who are not remarkably subject to catarrh at other times, nor from the ordinary causes of catarrh; and if they avoid meadows and hay-fields, and the neighbourhood of hay-stacks, they escape the malady. Hence going to the sea-coast,—and especially to those parts of the coast that are barren of grass,—offers a means of protection: and when this cannot be done, such persons obtain refuge, in some measure, from the cause of the irritation, by remaining within doors, and shutting out as much as possible, the external air, during the hay-crop. One lady, who suffered annually from this strange affection, states that a paroxysm has been brought on by the approach of her children, who had been in a hay-field; and once this happened when the hay-harvest had been for some time over, upon their joining her at tea, after playing in a barn in which the hay of that year had been deposited. She was in the habit of flying to Harwich, or some other part of the coast, as the dangerous season came on. On one occasion, while walking on the shore at Harwich, she was suddenly attacked by the complaint, to her great surprise, as she was not aware of any grass being in the neighbourhood; but the next day she discovered that hay-making was in progress upon the top of the cliff at the time when she was walking under it. In another year, she being at Cromer, and an attack that she had suffered having quite subsided, and all the hay-making thereabouts being over, she was suddenly visited by the well-known symptoms, and on going into her bed-chamber perceived that they were building a large stack of hay in a yard near the house, having transferred it from a field five miles distant.

I was asked by Mr. Cheyne to see with him the wife of a stable-keeper near Regent Street. I found her suffering under what is popularly called “a crying cold:” pain in the situation of the frontal sinuses, streaming eyes, sneezing and defluxion from the nostrils, and very urgent dyspnoea, which was accompanied by loud wheezing. Symptoms of this kind had come on, suddenly, some days before: and her distress was then so great, that her husband proposed to drive her in a gig to consult a medical friend of his who lived at Islington. On their way thither, every symptom disappeared, and she felt at once quite well. She subsequently stayed a night or two, in comfort, with some relations in the city. Immediately upon her return home, the same symptoms recurred, with all their former severity, and resisted the means adopted for their relief by Mr. Cheyne, who had now been called in. He was soon led to suspect the cause of the attack, and of its obstinacy. There was a strong odour of hay in the house. The husband told him that his lofts were filled with a lot of hay which had recently arrived, and which had an unusually powerful smell. We learned that our patient was always worse at night, when the house was shut up; and better in the morning, when a free current of air blew through the open windows. We advised a temporary change of residence; but our advice was not followed until two days afterwards, the disorder meanwhile continuing, and increasing in intensity. Then the patient removed to lodgings not one hundred yards distant; and immediately all the catarrh and distress again ceased, and she passed a perfectly tranquil night. Afterwards she went into the country, and did not return till the odouriferous parcel of hay had been consumed, and a new stock laid in. She was

however revisited by some slight cough, and occasional dyspnœa — neither of which troubled her much or long.

Avoidance, then, of the ascertained source of the complaint is the best thing that can be recommended to these persons. You may read almost every year in the news papers that one of our English dukes has gone to Brighton to escape the hay-fever. But it is not in the power of every one to leave home for that purpose; and it has been found that the system is capable of being fortified in some degree against the pernicious effects of these vegetable effluvia. Mr. Gordon, of Welton, in Yorkshire, had communicated some interesting observations to the profession on this subject, before those of Dr. Elliotson were published. You may find Mr. Gordon's paper in the fourth volume of the *Medical Gazette*. He supposes that the aroma of the sweet-scented vernal grass, the *anthoxanthum odoratum*, is the principal exciting cause of the complaint. He found the symptoms more speedily and effectually removed by the tincture of lobelia inflata, than by anything else that he had tried at that time; and he recommended the cold shower bath as the best preservative against the attack. But in a subsequent communication to Dr. Elliotson, he states that the sulphates of quina and of iron, given in combination, had proved completely successful in emancipating from their tormenting disorder the two patients, from whose cases he had principally drawn up his account; although they had, in spite of all previous treatment, suffered an annual return of it for fifteen or twenty years.

The susceptibility of this troublesome affection of the respiratory mucous membrane, from a peculiar cause, which to most people occasions no uneasiness, appears sometimes to run in families; and this is nothing more than one might expect.

Dr. Elliotson, thinking it possible that the chlorides, which have the power of decomposing, and disarming of their noxious qualities, certain *animal* effluvia, might exert a similar control over the *vegetable* emanations that excite the hay-catarrh, suggested to one of the sufferers a trial of the chloride of lime, or of soda. He desired him to have it placed in saucers about his bed-chamber; to have rags dipped in it, and hung about the rooms of the house; to wash his hands and face with it night and morning; and to carry a small bottle of it with him, to smell from repeatedly in the course of the day: and this plan gave so much relief, — either by destroying the emanations, or by lessening the irritability of the mucous membranes, — that it was tried in other cases; and though it did not succeed in all, it did in most of them. Three patients out of four derived advantage from it. This expedient, therefore, is worth carrying in mind.

The son of an old acquaintance of mine was thought to have been benefited, in an attack of hay-fever, by the *Tinctura nucis vomicæ* of the Dublin pharmacopœia, taken in ten minim doses three times a day.

But the most hopeful preventive and remedy of this vexing disorder is that which has been lately suggested by Dr. Mackenzie. Led, as he has informed me, by the apparent analogy between certain irritable conditions of the skin, and of the mucous surfaces, and by the ascertained beneficial influence of *arsenic* upon some of the former, he tried that drug in the case of a gentleman suffering from hay-fever, in whom previous disappointments had produced despair of obtaining relief from any treatment. The use, however, of from three to five minims of the *liquor potassæ arsenitis*, in distilled water, immediately after each of his three daily meals, was attended with wonderful and speedy success. A like encouraging result followed the adoption of the same plan in other instances by Dr. Mackenzie, and by professional friends to whom he had mentioned it. Since the year 1850 I have recommended this remedy to several persons who complained of being annually harassed by the hay-fever: and from most of these I have subsequently received very favourable reports of its good effects. Dr. Mackenzie believes it to be most serviceable when the symptoms are of a catarrhal rather than of an asthmatic character.

There is another vegetable substance, better known to us, which produces, in some few individuals, symptoms very like those of the hay-asthma: I mean the powder of ipecacuan. I recollect a servant employed in the laboratory of St. Bartholomew's Hospital, when I was a pupil there, who had the peculiar ill-luck to be liable to this affection. Whenever that drug was under preparation, he was obliged to fly the place. This idiosyncrasy is not very uncommon. A very small quantity of the ipecacuan dust is sufficient, in such persons, to bring on a paroxysm of extreme

dyspnoea, wheezing, and cough, with singular anxiety, and great weakness. The distress usually terminates by a copious expectoration of mucus. There are persons who are similarly affected by the odour of cats.

These effects of a powdered root, and of certain emanations from grass or hay, lend weight to the hypothesis which ascribes the influenza to subtle vegetable matter floating in the atmosphere.

I would suggest a *trial* of the Respirator, as a defence against the particles of ipecacuan, and against the volatile exciting cause (whatever it may be) of hay-asthma.

Catarrh is very often met with in a chronic form; in other words, the mucous membrane of the air-passages is very liable to be affected with chronic inflammation. The accounts which you may read of this are exceedingly puzzling. Authors have endeavoured to draw nice distinctions between different *species* of chronic catarrh; sometimes according to varying qualities in the matters expectorated: thus you have *chronic mucous catarrh*—*pituitous catarrh*—*chronic pituitous catarrh*—and *dry catarrh*, which, after all, is *not* dry, but only accompanied by less expectoration than some of the others; and then again there is *symptomatic catarrh*. You will find all these enumerated by Laennec; and the majority of writers since his time have trodden with too submissive reverence in his footsteps. There are by no means such differences in the symptoms or in the proper treatment of the several varieties of chronic inflammation of the membrane in question, as to make these numerous subdivisions of any practical utility. Chronic or moderate catarrh is often a sequel of acute bronchitis; it is a very common accompaniment of disease of the heart; it frequently arises during the course of the febrile exanthemata; it is seldom entirely absent in cases of continued fever; and it is a form of complaint that is full of interest on this account, if on no other, that it has so often been mistaken, and is so liable to be mistaken still, for tubercular consumption; of which indeed it is very frequently the companion.

The *constant* symptoms of chronic catarrh, or bronchitis, are cough, some shortness of breath, expectoration of altered mucus. The *variable* symptoms those which are oftentimes of the most importance, as determining the slight or the serious character of the disorder, consist in the quantity and quality of the matters expectorated, and the presence or absence of wasting, and of hectic fever.

You will continually be meeting with cases of *this* kind. A person advanced in years has what he calls a slight cold, in the winter. He coughs, and expectorates a certain quantity of grey or transparent mucus. In the summer his cough diminishes, or ceases altogether. The next winter the same thing happens again; and each successive return of the colder seasons of the year brings back in increasing severity the cough and the expectoration: and if you listen to the breathing of such persons, while the cough is on them, you will find crepitation at the lower part of their lungs. Now these are examples, I believe, of a chronic state of slight inflammation of the membrane,—or it may be of passive congestion and effusion,—depending upon slowly advancing *cardiac* alterations. Peripneumonia notha is very apt to supervene on this condition.

But chronic bronchitis may take place at any age, as a sequel to the acute: just as active inflammation of other parts of the body is liable to degenerate into the chronic form; and such cases are sometimes very equivocal and deceptive. Several years ago, a lady became my patient, having cough, expectoration of puriform matter, night-sweats, and diarrhoea. She had had hooping-cough a short time before; but though the hooping and other symptoms proper to that disease had ceased, she continued to cough and to waste. Gradually she got thinner and weaker, her pulse became like a thread, and beat 120 times in a minute; she took to her bed, the diarrhoea was scarcely restrained by astringents and opiates, and I thought she could not live a week. And upon being pressed by her brother for my opinion, I said so. She had scarcely allowed me to listen to the sounds in the thorax: but I had once done so fairly, and I could find no morbid sounds, except at the lower part of the lungs. If I had trusted to that circumstance alone, I should have said that she had not pulmonary consumption; but I had not then so much faith in the indications afforded by auscultation, nor in my own accuracy of ear in such matters, as I might have now; and I concluded that she *was* dying of tubercular phthisis. Almost on the day,

however, on which I ventured to give this prognosis, some slight amendment began: and she did gradually recover, and is alive and quite well at this time. Now it is in cases of this kind that cures are performed by those who boast of curing consumption.

In truth, chronic bronchitis is, in some cases, as incapable of recovery, and as surely and progressively fatal, as tubercular phthisis itself, and even more so than some of the forms of phthisis. So long, however, as *no organic change* has taken place in the air-tubes, or in the mucous membrane lining them, these chronic forms of bronchitis that simulate phthisis in their general symptoms, are within the reach of cure. They are to be treated by counter-irritants to the chest—and by such measures as are calculated to relieve the most urgent symptoms. Opiates for cough, or for diarrhoea. Sometimes the patients bear steel well, and then it is almost sure to have a beneficial effect. Sometimes sarsaparilla appears to do good; but, as far as I have observed, one of the most effectual restoratives in these cases is to be found, when the weather and the strength permit, in frequent change of air and place; in gentle gestation in a carriage, or in a boat; and in a nourishing but bland and unstimulating diet. When the membrane, and the tubes which it lines, become *altered in structure*, and pour forth a fluid which has all the qualities of pus, hectic fever generally is present, and the chronic disease tends, slowly perhaps, but surely, to death.

There are certain cases of chronic bronchitis which are especially remarkable, on account of the great abundance of the bronchial secretion: so great that the patients appear to die principally from the daily exhausting drain thus made upon the system. There are sometimes no other evident signs of inflammation; so that, as Andral observes, one might be led to separate these fluxes from the truly inflammatory affections. They differ from them apparently in their nature, and certainly in the treatment which they require. Andral has detailed two or three instances of this kind in his *Clinique Médicale*. The patients expectorated every day large quantities—a pint or more—of frothy fluid, resembling weak gum-water in colour and consistence. They had no fever; neither frequency of pulse nor heat of skin; but they were exceedingly pale, like persons blanched by hæmorrhage, and their emaciation and weakness were also extreme. Very little appreciable deviation from the healthy state was detectable when the lungs and heart were examined after death.

It does occasionally happen that even larger quantities—three or four pints daily—are, for a considerable period spat up, without much wasting.

Andral says, whether, in such cases as these, which certainly occur, although they are not very common, the first indication of treatment should not be to check and diminish the excessive bronchial secretion; to treat it as you might treat a gleet of the other mucous membranes, with balsams, administered either by the stomach, or in the shape of vapour. He conjectures that it might have been in cases of this nature that the vapour of tar, and tar-water, were once thought to be so useful. Probably the creasote would be well adapted to such cases. Certainly I have seen the excessive expectoration diminish, and the patients gain strength, under the use of the balsams; the compound tincture of benzoin, for example; a form of medicine much employed, formerly, and too much neglected, I apprehend, at present.

[Balsam Copaiba will be found a very valuable remedy in this form of bronchitis. Dr. La Roche relates in the *North American Medical Journal*, Vol. III., several cases illustrative of its efficacy.—C.]

Another remedy from which I have derived great advantage in some cases of the same kind, is the sulphate of iron, given in two or three-grain doses, in the compound infusion of roses, thrice daily. When there is any fever present, these remedies are apt to augment it: but when the pulse is quiet, and the skin cool, I am quite sure that they are often of the greatest service; and this you will likewise find to be the opinion of various practical authors.

There is another very remarkable condition of the pulmonary mucous membrane, constituting also, I imagine, a species of chronic inflammation, and characterized chiefly, like the last, by the matters expectorated. I mean that state in which a firm substance, resembling a false membrane, forms in the smaller bronchi and in their ramifications, and is coughed up, from time to time, in fragments. I mentioned in a former lecture that the false membrane of croup sometimes descends a long way into

the bronchi; even to their extremities. But I am speaking now of a different and less acute form of disease, in which the trachea being unaffected, concrete masses, evidently moulded in parts of the hollow bronchial tree, are spat up; somewhat like bunches of worms, or the branching roots of a small plant. This I presume to be uncommon; for I have met with it twice only in my life. It has been described, however, by several observers. The first, Dr. Warren, has a paper upon it in the first volume of the *Medical Transactions*, where he gives representations of the substances coughed up, which he calls *bronchial polypi*. Dr. Paris has told me that a patient of his coughed up considerable quantities of these branching casts of the ultimate air-tubes, now and then, for a long period. An interesting paper of Mr. North's, on the same subject, was read at one of the evening meetings at the College of Physicians. That gentleman possesses some beautiful specimens of these mis-called polypi. Sir R. Carswell gives a figure representing them. When the affection is extensive, it is attended with great distress, and dyspnoea, and violent fits of coughing; and the symptoms are wonderfully calmed upon each expulsion of the solid matter. The surprise is that such patients should ever recover; but I have never heard of an instance in which the complaint proved fatal.

The two examples of it which have fallen under my own observation, were invested, by the circumstances attending them, with a peculiar interest. They occurred, within less than a twelvemonth of each other, in the persons of two brothers, of middle age, the one a barrister, the other well known to you all as one of my most valued colleagues in this place. Both of them were, and are, remarkably stout, strong, and healthy men.¹ In both cases the expulsion of the so-called polypi was preceded by hæmoptysis, which came in considerable gushes, and was repeated at intervals of a few days, until the solid matters began also to be expelled, and then the hæmorrhage soon subsided.

The barrister, after having been annoyed for nearly a year by some huskiness of the voice, spat up, all of a sudden, a small quantity of bright blood; and soon afterwards expectorated several ramifying masses of tolerably firm consistence, resembling fibrinous coagula of blood, deprived of most of its colouring matter. Some of them, which I saw and examined, were solid; others, I understood, were hollow. I found slight circumscribed crepitation in the lower and posterior part of his left lung. This trivial degree of hæmoptysis, with the expulsion of what looked like casts of the interior of a bronchial tube, was once or twice repeated within a few days. He had no fever—no dyspnoea. Mercury, *inter alia*, was prescribed; but as the patient did not feel in any way ill, I believe he soon became tired of physic, and of medical restraint. Whenever I have since seen him, he has appeared to be in perfect health.

Of the Professor's illness I saw more. In the midst of health which had been uninterrupted, save by a solitary fit of gout some years before, he also spat some mouthfuls of florid blood. He had no cough, but the hæmoptysis was accompanied by a rattling sensation in the right side of his chest.

For about three weeks he continued, at intervals varying from three to six days, to expectorate blood, in gushes. The smallest quantity brought up on any one occasion was two ounces; the largest, eight. Just above the right nipple the respiratory murmur was mingled with large crepitation, which was always sensibly increased, and quite perceptible by the patient himself, during the attacks of hæmorrhage. In the intervals between them his breathing and his pulse were perfectly tranquil and regular.

With the florid blood came up, in general, some black coagula; and at the end of three weeks, or thereabouts, in these black masses ragged shreds of a different and firmer material began to be visible: and presently afterwards, complete branch-like casts of the ramifying air-tubes were expelled; and the bleeding ceased.

Of these casts there were two kinds: the one solid, somewhat coloured, evidently fibrinous, and resembling the branching coagula that may sometimes be drawn out of the arteries in the dead body; the other white, membranous, tubular, but ramifying also. None of them were of very firm consistence.

Till these substances made their appearance, our anxiety about the patient was extreme, and he underwent some rigorous discipline at our hands. He was confined

¹ Since this was written, Professor Daniell has been suddenly cut off, to the great grief of all who knew him, by a stroke of apoplexy.

to bed, forbidden to speak, kept strictly to the slenderest slop diet, several times bled, and extensively blistered. Lumps of ice were given him to swallow, and pounded ice was applied to his chest whenever the blood broke forth afresh. He took mercury till his gums were tender, and afterwards the acetate of lead, and other reputed styptics.

To most of this I was a consenting party; but looking back upon the case now that its nature and result are known, I must confess that the treatment, though fairly justifiable at the time, was unnecessarily active.

Mr. North, in the paper to which I have alluded (you may see it in the twenty-second volume of the *Medical Gazette*), draws a distinction, of which he gives the credit to Dr. Cheyne, between the hollow, membranous concretions, expelled without any blood; and the solid branching masses which accompany or succeed hæmoptysis, and are obviously mere coagula of blood moulded in the smaller air-tubes, where it had stagnated. He points out the comparatively harmless character of the cases in which the first occur; and the far more serious import of the second: the hæmorrhage denoting the presence of some organic mischief within the thorax, and the "polypous concretions" being simply an accident of the hæmorrhage.

I doubt the accuracy of this distinction. The brothers of whom I have spoken continue to be, as they were before, free from any symptom or suspicion, either of cardiac or of pulmonary disease. Moreover, in hæmoptysis depending upon tubercles in the lungs, or upon organic disease of the heart, these concretions are very rarely observed. I have never seen them in such cases, common as such cases are. The barrister had a husky voice, and the Professor was noticed to have been often "clearing his throat" for some time before the first eruption of blood: from which circumstances I infer a previous unhealthy state of the mucous membrane. Upon the whole I incline to the views expressed by Dr. Todd, with whom I had the advantage of consulting in the latter case — that a chronic and limited inflammation of certain of the bronchial tubes first occurred, disclosing itself by no marked symptoms, but leading to the formation of tubular membranes; that, after a while, these membranes began to be detached; that hæmorrhage resulted, and continued till the separation was complete; and that, at the same time, some of the extravasated blood coagulated in, and took the shape of, the air-tubes, and was afterwards expectorated.

I understand that the barrister has since had a recurrence of this strange complaint, which he treated very lightly, and soon got rid of.

He entertains a fixed belief that his attacks were attributable to the presence of one of Dr. Arnott's stoves in his chambers; the heated atmosphere of which always produced a slight feeling of constriction and distress within his chest. Whether this notion be well or ill founded I cannot pretend to say: but it is curious that the Professor also had been using a similar stove, which, placed in his sitting-room, warmed both it and his bed-room adjoining it.

In the fifth volume of the *Transactions of the Pathological Society of London* is contained an interesting summary by Dr. Peacock, of thirty-four cases, recorded by various authors, in which fibrinous casts were expelled from the air-tubes.

From this summary it would appear:—

That the affection occurs more frequently in males than in females.

That though it is not limited to any period of life, it is most common in persons of middle age.

That generally the moulds proceed from bronchial tubes of the third or fourth magnitude, and from their branches; the trunks having the average size of an ordinary goose-quill.

That hæmoptysis is quite as often absent as present; and that the only pathognomonic sign of the complaint is the expectoration of the membranous shreds or casts.

That the attacks generally cease after a week or fortnight, but are sometimes protracted over several weeks; and that they are apt to recur, from time to time, for months or years.

That in itself the complaint is not a dangerous one; and that when death takes place during its progress, the fatal issue results from some disease of which the membranous exudation is merely a complication. Some of the patients who died were manifestly consumptive; and most of those in whom this peculiar expectoration has

been noticed were originally of delicate constitution; or had shown previous tokens of pulmonary weakness, or of pulmonary disease.

A word or two more, before we separate, as to the morbid anatomy of these tissues.

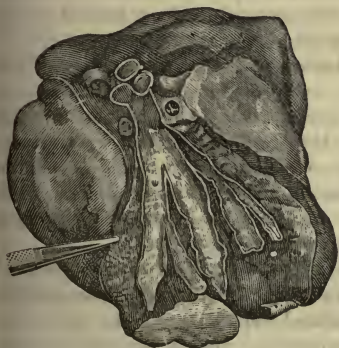
Chronic inflammation of the aerial mucous membrane may lead to changes in its colour; or to thickening of the membrane; or to ulceration; or to dilatation of the bronchi, and of their ramifications. And it is proper that you should be informed respecting these morbid conditions.

In general, when chronic inflammation has existed during life, the mucous membrane is found to be red: but it is not a bright redness; it is rather a livid, or violet, or brownish tint. And what is very curious, in some instances in which all the symptoms of inveterate bronchitis, with *puriform* expectoration, had been present, the inner membrane of the air-passages has been found scarcely rosy — or even perfectly white — throughout its whole extent. Of course we are not to infer from this that there has not been *inflammation*; for the same thing is known to occur in the intestinal mucous membrane, in that of the bladder, and even in serous membranes. Where pus is poured forth there must have been inflammation.

One effect of inflammation, as I formerly showed you, is a softening of the membrane; but this is a much less common result of inflammation in the mucous membrane of the bronchi, than in that of the digestive organs. In regard to ulceration likewise there is a great difference between the two mucous surfaces: in that of the air-passages it is comparatively rare.

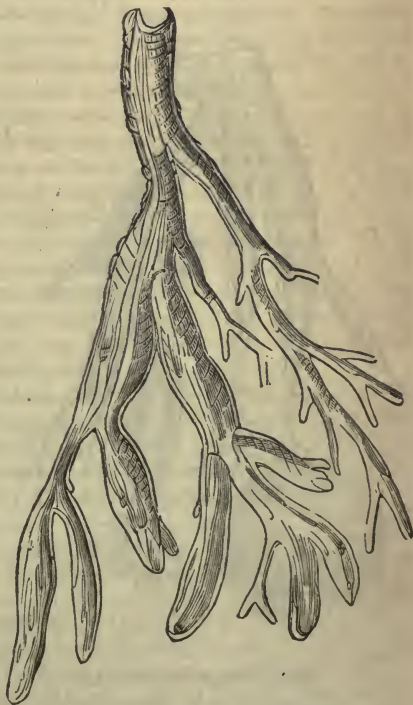
Thickening of the mucous tissue occurs also in various degrees: and in connexion with the various morbid conditions of this membrane, I must bring briefly under your

Fig. 44.



Dilated bronchi, from a female aged 52, who had suffered from chronic pneumonia and bronchitis for three years; the pulmonary tissue intervening between the bronchi was much condensed.

Fig. 45.



Uniform dilatation of the bronchi.

notice some remarkable changes, produced by disease, in the tubes which it lines; and specially the dilatation of those tubes.

There are two or three varieties of this dilatation. In the first of them, one or more of the bronchi present, throughout the whole or the greater part of their extent,

an increase of capacity which is very often considerable: so that tubes which result from the fourth, or the fifth, or even the sixth division of the principal bronchus of each lung, may equal or exceed in diameter that bronchus itself. Tubes that ought to be no bigger than a crow-quill may become as large as the finger of one's glove. The parietes of the dilated tube are thickened, and its circular fibres hypertrophied, as you may see in this preparation, and in Sir R. Carswell's plate. Sometimes this kind of dilatation is seen in a single branch only, sometimes in many branches. It may affect the bronchial ramifications of an entire lobe. It is more common in the branches of a bronchus than in the bronchus itself.

With respect to this sort of dilatation, it is observable that the pulmonary tissue lying in immediate contact with the enlarged tubes is usually, if not always, impermeable by air; it may be from some bygone inflammatory process; it may be from mere collapse and its attendant atrophy. Dr. Corrigan, and after him Dr. William Gairdner, have shown the strong probability that the dilatation is a consequence of this unnatural condition of the surrounding tissue; and that it is gradually produced by "the expansive forces of inspiration acting upon the bronchi of atrophied lung."

In the second variety of this change, instead of the uniform dilatation of one or more bronchial tubes, throughout their whole extent, we find a bellying, or globular expansion, at the extremity of one of them; and the walls of the tube, instead of being thick and hypertrophied, are wasted, and in a state of atrophy. The tissues composing the tube are often so thin, that when the cavity, for such it must be called, is laid open, the colour and structure of the pulmonary tissue may be seen through them. These cavities are generally found filled with a thick, tenacious, straw-coloured, muco-purulent fluid. They are seldom met with except after those affections which

are characterized by considerable secretion from the membrane, and by much and repeated cough: as after some forms of chronic bronchitis, and after whooping-cough.

A third variety of dilatation is that in which the same bronchus bellies out in different places; is dilated at intervals; so as to present in its course a series of successive enlargements and contractions. Here, again, the walls of the bronchi, though they may be traced in the parts dilated, do not appear to be thickened, but rather are diminished in thickness. This variety of dilatation is more frequent in children than in adults.

Dr. William Gairdner gives a plausible account of the formation of these globular or sacculated expansions. He states that, after bronchitis, pus is sometimes found imprisoned in the central air-branches of a collapsed lobule. The coats of these tubes, injured and softened by disease, gradually give way and ulcerate; and the pus, which accumulates, is at length surrounded by a false membrane exactly similar to that of an abscess in any other part. The continuity of this membrane with that of the ministering bronchus may either exist from its first formation, or be established subse-

FIG. 46.



Obliteration of the bronchi.

quently. These "bronchial abscesses" communicating at length freely with the tubes leading to them, may afterwards be expanded beyond their former dimensions by the inspiratory force.

In whatever way dilations of the bronchi may take place, it is to be expected, when they are numerous or extensive, that they should be attended with some habitual shortness of breath.

But the most important consideration arising out of these conditions of the bronchi, is this; that the signs, both general and physical, by which they are accompanied, are apt to be exactly those which are most distinctive of phthisis. And it is on that account that I have now described these changes. I shall revert to them again when I come to the symptoms, revealed by auscultation, of tubercular disease of the lungs.

LECTURE L.

*Hooping-cough: symptoms; duration; complications; pathology; treatment.—
Pneumonia: its stages and morbid anatomy; auscultatory signs.*

I HAVE yet to consider one very important disorder, which is usually classed among the catarrhal affections, but which is marked by features so peculiarly its own, as to distinguish it effectually from every other form of disease. I allude to *hooping-cough*: a remarkable complaint, well known everywhere, I believe, and much dreaded by parents. It has received a variety of names: *chin-cough*; *kink-hoast*; *coqueluche*; *tussis convulsiva*; *tussis ferina*; and *pertussis*. This last name, which Sydenham bestowed upon it, and which was adopted by Cullen, is the technical appellation of the disease in this country, as *hooping-cough* is the popular.

The phenomena that characterize *hooping-cough* are, I say, remarkable. It begins with the symptoms of an ordinary catarrh arising from cold. The child (for it is most especially a disease of children) has coryza, and coughs: and mothers and nurses are aware that the disease commences in this way, and express their apprehensions lest it may turn to the *hooping-cough*. After *this* the *catarrhal* stage, has lasted eight or ten days, or a fortnight, or sometimes a day or two longer, that kind of cough begins to be heard which is so distinctive. It comes on in paroxysms, in which a number of the expiratory motions belonging to the act of coughing are made in rapid succession, and with much violence, without any intervening inspirations; till the little patient turns black in the face, and seems on the point of being suffocated. Then one long-drawn act of *inspiration* takes place, attended with that peculiar crowing or *hooping* noise, which denotes that the rima glottidis is partially closed, and which gives the disease its name. As soon as this protracted inspiration has been completed, the series of short expiratory coughs, repeated one immediately after the other till nearly all the air appears to be expelled from the lungs, is renewed; and then a second sonorous back-draught occurs: and this alternation of a number of expiratory coughs, with one shrill inspiration, goes on, until a quantity of glairy mucus is forced up from the lungs, or until the child vomits, or until expectoration and vomiting both take place at once. During the urgency of the paroxysms the face becomes swelled, and red or livid, the eyes start, the little sufferer stamps sometimes with impatience, and generally clings to the person who is nursing him for support, or lays hold of a chair or table, or of whatever object may be near him, to diminish (as it would seem) the shock and jar by which his whole frame is shaken. As soon as expectoration or vomiting have happened, the paroxysm is over. The child may pant a little while, and appear fatigued; but commonly the relief is so complete, that he returns immediately to the amusements, or the occupation, which the fit of coughing had interrupted, and is as gay and lively as if nothing had been the matter with him. When the fit terminates by vomiting, the patient is in general seized immediately after with a craving for food, asks for something to eat, and takes it with some greediness.

Each paroxysm may consist of several alternations of the gasping coughs, and the characteristic hoop or kink; but Cullen remarks, that the expectoration or the omiting usually takes place after the second coughing, and puts an end to the fit.

The number of paroxysms that occur in the twenty-four hours is variable also: and they come on at irregular intervals. The nocturnal paroxysms are commonly more

severe than those of the day. When the complaint is uncomplicated, the child, during the intermissions, appears to be quite well. This is another striking feature of the disorder. In the earlier paroxysms the mucus expelled is scanty and thin; and in proportion as this is the case, the fits are the longer and the more violent. By degrees the expectoration becomes more abundant: and sometimes it is very copious: at the same time it is thicker, and more easily brought up; and on that account the fits of coughing are less protracted.

The ordinary duration of the disease is from six weeks to three months; but it may run its course, I believe, in three weeks; and it may continue for six months, or more.

In an uncomplicated case, if you listen at the chest during the intermissions, you will probably hear the sounds that are proper to catarrh—some degree of rhonchus or sibilus: and in some parts there may be puerile respiration; and if you percuss the thorax, you get the natural hollow sounds. But what happens when you apply your ear to the chest during the paroxysms of coughing? Why, the information given us in this case by auscultation is very curious. You may perhaps hear, between the short explosive shocks of the cough, some snatches of wheezing, or of vesicular breathing; but during the long-drawn noisy inspiration that succeeds, all *within* the chest is silent. This is supposed to result from the slow and niggardly manner in which the air passes towards the lungs through the chink of the glottis, which is spasmodically narrowed. It may also depend, in part, as Laennec supposed, upon a spasmodic condition of the muscular or contractile fibres of the bronchi and their branches. When the fit is at an end, the ordinary sounds of healthy, or of catarrhal respiration are resumed.

Children are very susceptible of this complaint; and it is a complaint which spreads by *contagion*. Hence it follows that few children escape an attack of it. It is also one of those contagious maladies which do not in general affect the same individual twice; and hence again it follows that it is rarely met with in adults. Such is the fact; and such, I apprehend, is the explanation of it. It is not that adults are insusceptible of hooping-cough: for adults that have not had it during their childhood are readily affected when exposed to the contagion. But it is that the disorder, with very few exceptions, protects the system somehow from its future recurrence; and that most adults have *had* it when they were young, and for that reason do not take it afterwards.

During the very early periods of infancy, *i. e.*, within the first two or three months, hooping-cough is said to be rare: I mentioned a case, however, before—and I have read of others—in which the disorder appeared to have been contracted before the patient was born. My bedmaker's daughter in Cambridge had a child ill with hooping-cough in the house with her during the last week of another pregnancy, and the new comer hooped the first day he came into the world.

As long as this disease is uncomplicated—unmixed with inflammation, and therefore, unattended with fever, or only with that slight inflammatory condition proper to mild catarrh—it is seldom a *dangerous* disease. Probably it *will*, under the most favourable circumstances, run a certain course. By degrees the violence and the frequency of the paroxysms diminish; they occur only in the morning and the evening, then in the evening alone, and at length they cease altogether. But for some time after the disorder has apparently come to an end, if the child take cold, and get a cough, the cough is apt to assume a spasmodic character, and to be attended with a hooping noise in inspiration.

Nevertheless, when it is very intense, the disorder *may* have a fatal issue, without any organic complication. Before the little patient has fairly rallied from one paroxysm, another succeeds it; the spasmodic closure of the larynx is more and more complete and lasting, until, at length, little or no air can pass; the characteristic hoop becomes short and faint, or ceases altogether; and the child dies of apnoea.

Unfortunately, too, hooping-cough is, in a great many cases, not simple—not uncomplicated. It becomes mixed up with other kinds of disease, in the chest, or in the head. In the chest severe bronchitis supervenes upon it, or inflammation of the substance of the lungs; and then fever is lighted up, and permanent dyspnoea is present. When the disorder has been long drawn out, and has at last terminated fatally, dilatation of the bronchi, such as I described in the last lecture, is often found

upon dissection; still more commonly what is called *emphysema* of the lungs — a change which I have yet to bring before you; but most frequently of all, pulmonary collapse of the lobular kind.

Collapse of the lung I have already shown you to be ordinarily the result of obstruction of the air-tubes by mucus accumulated within them. The risk of such obstruction is always present in hooping-cough. But there are co-operating causes; to most of which these young patients are also liable. Whatever impedes the free and full indraught of air may be a co-operating cause: the spasm therefore which, narrowing the inlet, diminishes the supply of air; a tumid abdomen, hindering the contractions of the diaphragm; weakness of the muscles of inspiration, arising from general debility. And there is yet another accessory cause, which is peculiar to the early years of life. In the full inspiration of an adult the thorax is enlarged by the separation and the upward movement of the unbending ribs, and by the simultaneous descent of the diaphragm. But in young children, when the inspiratory act is difficult and forced, the ribs yield under the power of the contracting diaphragm, which drags them inward, and thus the full expansion of the lung is stinted. In this way permanent deformity of the chest is sometimes produced. We need not wonder then that more or less of pulmonary collapse should be a nearly constant phenomenon after death from hooping-cough.

Neither can we be surprised that the disease frequently leads to cerebral disorder. During the fits there is a great and visible determination of blood towards the head, or rather a detention of the blood in the veins that proceed from the head; — passive mechanical congestion: the transmission of the blood through the lungs being obstructed, and its return from the head interrupted. Hence, the face becomes turgid, the eyes are prominent, the superficial veins full and projecting, the lips and cheeks turn livid; sometimes hæmorrhage takes place from the nose or ears; or the eyes become blood-shot; or the patient actually falls into convulsions; nay, apoplexy is occasionally the result of the straining; and when life is not thus suddenly cut short, chronic mischief is apt to be set up in the brain, and the child may ultimately die hydrocephalic.

All this is the more to be feared in proportion as the child is the younger. Head affections are particularly to be dreaded in scrofulous children; and in any children during the first dentition. When the disease occurs within the first two years of life, it is usually attended with convulsions: and many more die within that period than afterwards. And Cullen's remark is undoubtedly true; that the older children are, the more secure they are, *cæteris paribus*, against an unhappy event.

Hooping-cough may be complicated also with a disordered condition of the bowels; and with infantile remittent fever. This complication is more accidental, and less a *consequence* of the hooping-cough than the former; but it may very materially add to its peril.

Dr. Cullen was of opinion that the complaint may exist in even a milder form than that which I have called simple hooping-cough. He thought he had seen "instances of a disease, which, though evidently arising from the chin-cough contagion, never put on any other form than that of a common catarrh." Others again believe that adults may have it without hooping. But all this seems to me very doubtful. Catarrh is an exceedingly common malady; and I should be slow to consider any case a genuine case of pertussis, unless the characteristic paroxysms of coughing, and the stridulous inspiration, were present.

Divers opinions have been held respecting the seat, and respecting the nature, of hooping-cough. Some suppose it to have its seat in the brain: others that it is a specific variety of catarrh, and has always therefore more or less of an inflammatory character: others again, looking to its spasmodic symptoms, ascribe the disease to some morbid influence exercised upon the pneumogastric nerve: and this last I believe to be the truest view of the matter. The disorder belongs to a very remarkable group of blood-diseases, of which I shall have much to say hereafter, and is produced by an animal poison. Certainly the simple form of the disease is often unattended with any appreciable fever: and that is a strong ground for concluding that its peculiar phenomena are not *necessarily* connected with inflammation. They who have ascribed the complaint to a morbid condition of the brain have deduced that opinion, I presume, from the cerebral symptoms that are sometimes so plainly marked

in whooping-cough. But these symptoms are oftener, to all appearance, the consequence, than the cause, of the paroxysms of coughing. I would suggest it as an interesting point for your future inquiry, whether the pathology of whooping-cough may not receive some elucidation from the researches of the late Dr. Ley respecting the crowing inspiration of infants. You remember his suggestion, that mere inflammation of the mucous membrane of the air-passages might cause swelling of the absorbent glands of the bronchi, or of the neck. This is a circumstance which I have myself long thought probable, from having found enlargement of the cervical glands springing up during the existence of pulmonary irritation. Take notice that the spasmodic fits of whooping-cough are always preceded for some days by mere catarrhal symptoms. Observe further how the parts supplied by the pneumogastric nerve are affected in these paroxysms: the larynx, the lungs, the stomach. This conjecture, that the crowing inspiration of infants, and the crowing inspiration of whooping-cough—though quite distinct affections—may both depend upon irritation of the recurrent nerve, or of the pneumogastric nerve generally; and that even the irritation might in both cases arise out of enlargement of the glands that lie in the course of that nerve: this natural conjecture had presented itself to Dr. Ley's mind; for, towards the end of his book, I find this note:—"Recently four children have been brought to my house, labouring under whooping-cough. In all, the glandulæ concatenatæ near the trachea were very considerably enlarged. Is this (he says) merely an accidental combination? or is there any essential connexion between the two? May it not be that an enlargement of these glands, from a specific animal poison, similar to that of the parotid glands in mumps, is, after all, the essence of whooping-cough? The subject at least deserves inquiry and further observation."

In corroboration of this conjectural view of what *may* ultimately prove to be the true pathology of whooping-cough, I may remark that among the morbid appearances described as being met with after death from that disease, "an unusual swelling of the bronchial glands" is set down. It is also stated, by some of the Germans, that that portion of the pneumogastric nerve which lies in the cavity of the chest has been sometimes found red. Yet I should lay no stress upon this; for others have asserted that they have looked in vain for this redness: and even supposing it to exist, it is no sure or safe token that there had been inflammation of the nerve. The nerve, all things considered, would be likely to become tinged of that colour soon before, or even after, death, from the gorged condition of the lungs. In some cases, as you may well believe, serous fluid is met with in the ventricles of the brain, or in the meshes of the pia mater: in others the consequences of inflammation are traceable in the bronchi, the lungs, or the pleuræ. Portions of what is called hepatized lung are not unfrequently seen in the fatal cases: though less frequently than portions of collapsed lung.

[The leading opinions in relation to the pathology of whooping-cough, may be referred to one or other of the following heads.]

1st. With some the disease is essentially a *spasmodic affection of the air-passages*, arising from a primary irritation, inflammatory or nervous, of the brain, or of one or other set of the respiratory nerves. This theory, variously modified, is that advocated by Hoffmann, Cullen, Hufeland, Jahn, Löbel, Holzhausen, Leroy, Coiter, Guibert, Breschet, Gardien, Bauer, Albers, Clarus, Webster and Copland. Dr. Webster considers the affection of the respiratory organs to be secondary, and dependent on a primary inflammatory irritation of the brain or of its membranes, or of both. Most of the advocates of the nervous theory regard the pneumogastric nerves as the primary seat of the affection; others, however, locate the irritation in the phrenic nerves—the principal of these are Jäger, Lobenstein, Löbel and Leroy; others again, as Albers of Bremen, Pinel, Laennec, Blache and Roe, refer the irritation to both the mucous membrane of the bronchi and the pneumogastric nerves.

2d. Others consider the disease as an *inflammatory affection of some part of the mucous membrane of the air-passages*; this opinion numbers among its advocates, Darwin, Watt, Marcus, Alcock, Dewees, Dawson, Pearson, Guersent, Fourcade-Prunet, Boisseau, Broussais, Rostan and Dugés. Dr. Watt believed it to be "in all cases, an inflammatory disease, whose chief seat is in the mucous membrane of the larynx, trachea, bronchi, and air-cells, possibly attended with a minute exanthematous erup-

tion there." He considers that, when mild, "this inflammation runs its course without materially disturbing the other functions of the body, or even the functions of that very membrane in which it is seated, and that whenever whooping-cough proves dangerous or fatal, it becomes so by the degree of inflammation in the natural seat of the disease, or by that inflammation extending or being translated to other parts." Most advocates, however, of the inflammatory origin of the disease, limit the inflammation to the trachea and bronchi. Dawson confines it, at first, to the larynx, or, strictly speaking, to the glottis—an opinion, by the way, not widely differing from that of Astruc, who of old describes the disease as an "inflammation of the superior part of the larynx and pharynx."

3d. Many view the complaint as at first inflammatory, and afterwards spasmodic, or as a specific inflammation of the respiratory mucous membrane combined with irritation of the respiratory nerves, in consequence of which the muscles to which the latter are distributed are thrown into spasmodic action; the nervous irritation continuing after the inflammation has ceased. This doctrine, variously modified, is a favourite one with the writers on whooping-cough. Desruelles makes the disease to consist in a primary inflammation of the bronchi, complicated with a consecutive cerebral irritation, which by its influence over the diaphragm and other respiratory muscles, and over those of the larynx and glottis, changes the simple cough of bronchitis into one of a convulsive character. This opinion is the one adopted, also, by Dr. C. Johnson in his able article on whooping-cough in the *Cyclopædia of Practical Medicine (Philadelphia Edition, vol. 2, page 453)*: other writers who admit the *occasional* presence of cerebral disease, consider this to be invariably secondary to the bronchial affection: of this opinion are Boisseau, Begin, Otto and Vondembush.

4th. By not a few, especially of the older medical writers, whooping-cough is referred to an irritation of the stomach and lungs. Rosen, who supposed the disease to be produced by either an insect or a morbid poison, partly inhaled into the lungs and partly swallowed with the patient's saliva, considered that the stomach suffered more from the irritation than the lungs. Danz believed that the irritation is seated primarily in the stomach and bowels, the affection of the respiratory organs being secondary. This opinion had been previously advanced by Stoll, and is also that advocated by Chambon in his work "*Des Maladies des Enfants*." Tourtelle likewise describes the disease as a catarrhal affection of the lungs and stomach (*affection pneumogastrique pituiteuse*). Millot entertains a somewhat similar opinion; he supposes, however, that the irritation of the stomach is secondary, and not so intense as that of the lungs and larynx. Opinions somewhat similar are maintained by Meltzer, Holdefreund, Butler, Klinge and Strack.

5th. The peculiar phenomena of the whooping-cough have been ascribed, by a few, to a physical or chemical irritant introduced into the larynx, either from without, or from the blood where it is engendered, or from the secretions of the respiratory organs. Under this head may be arranged the opinion of Linnæus, who referred it to the presence of minute insects; that of Sydenham, who imputed it to a subtle and irritating vapour in the blood, which affected the lungs; that of Bohme and Klinge, and also, partially, that of Rosen, who ascribed it to a peculiar miasm, acting chiefly on the nerves. Dr. Bland, of the Hospital Beaucaire (*Revue Médicale*, March, 1831), considers the primary cause of whooping-cough to consist in an irritation, not inflammatory, of the mucous membrane of the bronchi, under which the glands and follicles of the membrane are caused to pour forth a specific secretion saturated with hydrochlorate of soda, the irritation caused by which, when it reaches the upper part of the trachea and larynx, throws the muscles of the glottis and of respiration into spasmodic action for its expulsion, in a manner exactly similar to any foreign body which may accidentally enter the larynx. This theory of the disease Mr. Streeter, of London, in a paper read before the Physical Society of Guy's Hospital, November, 1844, has adopted and zealously advocated, "as the one most consistent with the phenomena observable in the symptoms, pathology, and successful treatment of the disease."

Mr. Streeter regards, however, the primary affection of the bronchial membranes as inflammatory, and believes that it will be found, on careful observation, to be attended by more or less fever of an analogous character to that which attends the influenza.

The evidences upon which this gentleman rests the truth of this theory, are—"the

testimony of adults, who have been attacked by the disease, to the unusual and excessive saline taste of the expectoration so long as the paroxysms are severe—the resemblance of the expiratory efforts in the whooping-cough to those made by the excitatory system for the expulsion of a foreign body from the larynx—the very adequate explanation it affords, both of the extraordinary and spasmodic muscular actions which accompany the cough, and of its occurrence in paroxysms after intervals of uncertain duration—and, lastly, the key which it furnishes to the chaotic host of apparently opposite remedies that have obtained professional or popular reputation in its treatment. Of these remedies, we find one group adapted to lessen the original bronchial affections, and favour the expulsion of the offending mucus—as emetics, antimonials, and counter-irritants applied over the chest; another which acts by altering the quality of the secretion, as the alkaline carbonates, ammonia, and sulphuret of potass, so strongly recommended by Dr. Blaud; another, by exciting a new action in the bronchial membrane, and, by constricting the vessels, to put a stop to the secretion, in a manner perfectly familiar to the physician in chronic bronchitis, and to the surgeon in purulent ophthalmia—as the super-acetate of lead, alum, common resin, tinc. cautharid., bals. copaiba, tar vapour, and even the inhalation of nitrous vapours, &c. Others, again, as musk, both native and artificial, camphor, arsenic, conium, belladonna, opium, and hydrocyanic acid, are more especially adapted for the nervous lesions; while antiphlogistic measures meet the inflammatory lesions of the third or complicated stage.

“When, he remarks, the convulsive whooping is fully established, it very commonly happens that symptoms which mark the third, or what may be appropriately termed the complicated stage, are developed, and continue to mark the varying and formidable phases of the disease which mostly attract attention in practice.” These tertiary phenomena usually manifest themselves “1st—as special lesions of the nervous and muscular systems—an exalted sensibility and morbidly susceptible state of the membrane of the larynx, the pharynx, the epiglottis, under which death from asphyxia may suddenly occur—morbid association of the action of the muscles of the glottis and respiration, in consequence of which the cough continues from mere habit, or is reproduced by the most trivial irritation of the air passages—reflex irritation often passing into inflammation of the nervous centres of the pneumogastric nerves, involving those of the phrenic nerves also; and, finally, these reflex affections may extend to the whole of the brain, or to the medulla oblongata and their meninges, and prove fatal by inducing general convulsions or hydrocephalus. All these, be it observed, are pathological conditions of the nervous system, which have been so constantly put forward in high relief, by the advocates of the nervous theory, as proximate causes of the disease itself.” A second class of tertiary phenomena include “the various congestive and inflammatory affections that result from the mechanical disturbance of respiration and circulation, and the extension of the primary bronchial inflammation to the trachea, larynx and pharynx, and to the tissues of the lungs themselves. Epistaxis, hæmoptysis, and fatal emphysema from extensive rupture of the air-cells, have occurred within my own experience, and have apparently resulted from the mechanical violence of the cough acting upon tissues previously weakened by disease.” A third class of tertiary phenomena “include fever and cachexia,” which are “present in individual cases in every conceivable variety of combination.” “In the absence of cerebral or pulmonary inflammation, the fever of the third stage is always asthenic, and often assumes a remittent type when the cachexia is of a malaric character.”

For an account of the various lesions met with in the bodies of those who die of whooping-cough, the reader is referred to *Condie on Diseases of Children*, 4th ed.—C.]

The object of rational treatment in whooping-cough, supposing the disease to be simple, is to *keep* it simple: to keep it *mere* whooping-cough: to obviate serious inflammation, or mischief, in the chest and head: and, if possible, to mitigate the severity and shorten the duration of the fits of coughing. I have no notion that anything we can do in the beginning will materially abridge the duration of the complaint as it appears in its unmixed form. It *will*, I say, in all probability, run a certain course; and our business is to conduct it evenly and safely to the *end* of its course. For this purpose the diet must, in the first place, be regulated and reduced. The

child should be allowed to eat but little meat; it may be nourished as well, and more safely, upon milk, and unstimulating farinaceous matters. The bowels should be kept moderately open: and the patient in cold weather should be confined to the equable temperature of the house, or protected by warm clothing; and care should be taken to keep the internal temperature equable. The air of the bed-room should not be colder than that in which the child has passed the day. It should not be much above nor much below 60° Fahrenheit. You will find different persons employing and praising different plans of treatment; the object in all cases, however, being the same, viz., to *ward off inflammation*, and to *quiet irritation*. One very good plan, as I believe, is that of giving a grain, or a grain and a half, of ipecacuan, three or four times a day. This generally keeps the bowels sufficiently open, and seems to have a beneficial operation on the mucous membrane of the air-passages also. Or a few grains of rhubarb and of ipecacuan may be given every night: and if the cough be very troublesome and urgent, small opiates may be administered: syrup of poppies: or the extract of hyoscyamus: as many grains *per diem* as the child has years. There is a method recommended many years ago by a namesake of mine, which some people swear by. Sir William Watson's prescription was one grain of tartarized antimony and twenty drops of laudanum in an ounce of water. A tea-spoonful, or a dessert-spoonful, of that mixture was given every evening, or every other evening. I have heard the late Dr. Gooch say that his mother became famous as a village doctress by the help of that prescription. Fothergill's method was to give an emetic every day; or three or four times a week: and this plan is the more suitable when there is much wheezing, and the phlegm is brought up with difficulty. The best emetic substance in such cases is, doubtless, ipecacuan: and the best time for giving it is the evening. Mr. Pearson—who has had, I fancy, many imitators—used to prescribe, after the operation of an emetic, one drop of laudanum, five drops of ipecacuan wine, and two grains of carbonate of soda, in a draught, every fourth hour, for several days. Under some such treatment as this, the disease will reach its termination in from six to twelve weeks: and it frequently happens that when the child is quite well in all other respects, it still continues to *cough*. The cough would almost seem to be kept up by the mere influence of *habit*. Now, under these circumstances, change of air will often remove the cough, as if by magic: and the shower-bath, and iron in some shape, will sometimes succeed, if change of air be not practicable.

There is a great variety of medicines lauded as *specifics* against hooping-cough: but they are not to be trusted to. Many persons think highly of the prussic acid, as a remedy for the paroxysms of coughing. Others employ and praise the extract of belladonna. But these are gigantic remedies to employ upon such young subjects. If you give them at all, you must give them in very small quantities, and watch their effects. Dr. West thus records the result of his own experience in respect to the hydrocyanic acid, administered in minute doses—"This remedy sometimes exerts an almost magical influence on the cough, diminishing the frequency and severity of its paroxysms almost immediately; while in other cases it seems perfectly inert; and again in others, without at all diminishing the severity of the cough, it exerts its peculiar poisonous action on the system, so as to render its discontinuance advisable." The artificial tincture of musk is another substance which some have found useful. Three or four minims of it may be given in the outset, and the dose increased till some sensible effect is produced; and then the dose that has been so reached should be persisted in, without further augmentation. I have been assured, by a most intelligent practitioner, that he had got considerable credit by prescribing this medicine, after other persons, with other modes of management, had failed. Alum, in doses of three or four grains every four or six hours, has been given, with apparent benefit, when there has been much expectoration, and no fever. Digitalis, and cantharides, are other, and, I think, hazardous remedies. Safer drugs recommended, and, for aught I know, equally efficacious with these poisons, are cochineal, oil of amber, musk, camphor, and the meadow narcissus. Of late the carbonate of iron has been greatly commended by some of the continental physicians.

[Horst recommends the flowers of sulphur as almost a specific in hooping-cough. He gives it throughout the disease, from its onset until its termination. Schneider,

Roffy, Randhan, Riecken, and others, confine its use to the more advanced periods of the attack. Jadelot is said by Barthez and Rilliet to have derived the best effects from the sulphur as a remedy in whooping-cough, as well as in bronchitis unattended with fever. The sulphur is given to children between two and four years of age in the dose of from 6 to 8 grains, two or three times a day, and to older children in doses of from 15 to 20 grains, — in both cases the dose being gradually augmented.

Assafoetida will be found a very excellent remedy in the spasmodic stage of whooping-cough. Dr. Lombard states that he has often known frictions to the spine with the tincture of assafoetida of great service, and we have found a plaster of assafoetida applied to the chest promptly to relieve the cough, which is liable to remain after the more acute symptoms have subsided.

It is stated that M. Berger has found the plan of treating whooping-cough the most successful, to be; in the first stage, a moderate course of antiphlogistic remedies, purgatives, and repeated emetics of ipecacuanha in combination with tartar emetic. In the spasmodic stage, in which the indication is to quell the existing nervous irritation, being dissatisfied with the remedies ordinarily employed, he was induced to administer the nitrate of silver, from which he has obtained results singularly beneficial. He administers it in doses of from a sixteenth to a twelfth of a grain, at first three times, and afterwards four times a day. Of course the remedy should not be administered in cases where the state of the digestive organs contra-indicates its employment (*Annuaire de Thérapeutique*, 1846).

Dr. Golding Bird remarks (*Guy's Hospital Reports*, April, 1845), that, in the second or nervous stage of pertussis, after all inflammatory symptoms have subsided — and when, with a tolerably cool skin and clean tongue, the patient is still severely distressed by the more or less copious secretion of viscid mucus from the bronchi — each attempt to get rid of which produces the exhausting and characteristic cough — no remedy will be found to act so satisfactorily, or to give such marked and often rapid relief to the child as alum. He has not yet met with any other remedy which is equally efficacious. Dr. Bird generally gives the alum in doses of from two to six grains to children of from one to ten years of age, repeated every four or six hours. For a child of two or three years he employs generally the following formula: —

R Aluminis, gr. xxv; extr. conii, gr. xij; syrup. rhædos. ʒij; aq. anethi, ʒiij. m. Dose a medium sized spoonful, every sixth hour. Dr. B. has never met with any inconvenient stringent effects on the bowels during its exhibition; on the contrary, in more than one instance it produced, he says, diarrhœa. The only obvious effects resulting from its use were, diminished secretion, and of a less viscid mucus, with a marked diminution in the frequency and severity of the spasmodic paroxysms.

Dr. Davis, of London, (*Notes to Underwood*) is disposed to attach more importance to alum, as a remedy in whooping-cough, than to any other tonic or antispasmodic — others speak of it in equally favourable terms.

The belladonna has been strongly recommended as an almost unfailing remedy in the spasmodic stage of the disease by several of the German physicians. Dr. Wallor, of London, also, bears testimony to its efficacy, and we have in its favour the evidence of many distinguished practitioners in this country. Dr. Turnbull, of Philadelphia (*Trans. Penna. State Med. Soc.*, vol. v.), says that he has found nothing so prompt and efficacious in the second stage of whooping-cough as the extract of belladonna. He has employed it successfully in fifteen cases — four of which, of a most aggravated character, were in members of his own family — five males and ten females, of which the youngest was nine months and the oldest ten years of age. His mode of administering it was, after preparing the system by an antiphlogistic treatment, until the entire removal of fever and inflammation, as indicated by the reduced state of the pulse, and diminished heat of surface; there being, at the same time, no evidence of congestion of the brain or lungs present, to place the system fully under the influence of the belladonna — indicated by the dilated pupil, confused vision, and reddened skin). The medicine to be then intermitted until its effects pass off, when it should be again resumed in slightly increased doses, so as to keep the patient under its influence for several days, or until the paroxysms are fully checked, which will usually occur towards the sixth, eighth, or fourteenth day.

Dr. Turnbull directs the extract of belladonna to be triturated with water or simple syrup, and if to be kept for some time, and in warm weather, with the addition of a

little alcohol or spts. lavend. comp. The dose for a child, three months old, is one-sixteenth of a grain every three hours—for a child one year old, one-eighth of a grain, and so in proportion for other ages.—C.]

External applications are also much in fashion in the treatment of whooping-cough. Frictions to the spine and to the chest; and as counter-irritants, they probably are of some service. The tartarized antimony is the least innocent of these applications. It will often cause foul and very troublesome sores upon an adult skin: and till I am better advised than I am at present of its certain efficacy as a remedy for whooping-cough, no one (however authorized professionally *ludere corio humano*), should rub it upon a child of mine. Mothers are many of them fond of using Roche's Embrocation for the Whooping-cough. This (Dr. Paris tells us) consists of olive oil, mixed with half its quantity of the oils of cloves and of amber.

Such is the plan of management which you will do well to enforce—and such are the expedients which you may, if you please, make use of as auxiliaries to that plan—when the disease is *mere* whooping-cough. But when it becomes complicated with symptoms of inflammation within the chest, or with head symptoms—(and for such symptoms you must jealously watch)—then you must employ antiphlogistic remedies (in addition to the antiphlogistic regimen) adapted to the circumstances of the case. Now we have reason to believe that the *bronchi*, or the *lungs*, are affected with inflammation, when we find that the child has *fever*, and that there is *permanent dyspnoea* between the paroxysms of spasmodic cough. In such a case we must have recourse to the treatment required in such inflammation: leeches to the surface of the chest, tartar emetic, small doses of nitre, the warm bath, and blistering; and to these measures, modified and combined according to the particular emergency, it will be well to add small and repeated doses of mercury; of the hydrargyrum cum creta, or of calomel; the state of the bowels determining which. Some have recommended friction with the tartar emetic ointment upon the *chest* in such cases: but I have the same objection to it there, in patients so young, as upon the spine.

Permanent dyspnoea, unattended with fever, is more likely to depend upon collapse, than upon inflammation, of the pulmonary substance. Under such circumstances any active antiphlogistic measures would be out of place, and even hurtful. You must search therefore for auscultatory evidence as to the condition of the lungs, and adjust your treatment accordingly.

When any head-symptoms come on, threatening hydrocephalus, or apoplexy,—such as squinting, convulsions, stupor—those remedies must be adopted which I endeavoured to describe to you when I spoke of those diseases; leeches to the head, cold applied there, purgatives, the warm bath; but, except in very young children, I believe there is more danger of fatal *pulmonary* changes in this disagreeable, and sometimes intractable disorder, than of cerebral mischief.

I might pass, by a very natural transition, from the consideration of whooping-cough, to that of *spasmodic asthma*. But this last complaint is found to exist in connexion with *various* organic changes within the chest, few of which have yet been treated of in these lectures. I shall therefore postpone what I have to say respecting asthma, till I have gone through some other thoracic diseases. And I now proceed to *pneumonia*, or inflammation of the *substance of the lungs*. Questions have been raised as to the precise part and texture in which the inflammation begins; and to these questions I may briefly advert as we go on; but I hold that in pneumonia *all the textures* composing the pulmonary substance in the part inflamed are involved in the inflammatory process.

Now of pneumonia it is especially true, that we ascertain its situation, its extent, and every step of its progress, by means of the ear. All the symptoms that give us the most sure information respecting the nature of the disease, its increase and aggravation on the one hand, or its abatement and diminution on the other, spring out of the actual changes wrought in the pulmonary substance itself; and these changes are disclosed to us by the method of auscultation. It is necessary, therefore, that you should understand, first of all, what those changes are which are produced by inflammation of the substance of the lungs: that you should know the morbid anatomy of pneumonia, as an indispensable groundwork for a knowledge of its pathology.

There are three well-marked, and very constant conditions of the lung, corresponding to different degrees and periods of its inflammation. I will describe them in succession, in the order in which they take place.

The first stage or condition is that of *engorgement*. All modern observers agree, I believe, both as to the nature and as to the name of this condition. The substance

FIG. 47.

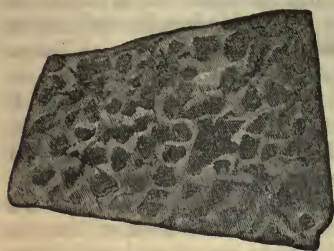


FIG. 48.



Fig. 47.—Pleural surface of a portion of splenified lung, affected with typhoid pneumonia, from a female æt. 25, who died of typhoid fever. The lung closely resembled the spleen in consistency, was of a brownish red hue, interspersed with deep purple spots of an apoplectic character.

Fig. 48.—From a lung in rather an advanced stage of inflammatory engorgement, magnified 430 diameters. *a*. Fibrous tissue forming the walls of the air cells, distinct and enclosing a few granules; *b*. Very granular epithelial cells of bronchi and air vesicles; *c*. Small nucleated cells and exudation corpuscles, lying partly within, partly outside the air vesicles; *d*. Blood corpuscles. *From Da Costa.*

of the lung is gorged with blood, or bloody serum. It is of a dark red colour externally, and crepitates less under pressure than sound lung does. We feel that there is more liquid than air in its cells. It is heavier also than natural, and inelastic, and

FIG. 49.



Elements observed in lungs that had been in a state of chronic hyperæmia. *a*. Epithelial cells being transformed into granular cells; *b*. Flakes enclosing blood corpuscles which are irregular and of a yellowish colour; *c*. Blood-corpuscular cell, breaking up into a dark, reddish-brown pigment; *d*. Situation of freshly-formed pigment, in the tissue of the lung. *From Da Costa.*

retains, in some degree, the impression of the finger. When the engorged portion is cut, we find it red, and we see a great quantity of a reddish and frothy serum flow from it. Its cohesion is at the same time diminished: it is more easily torn; more, in that respect, like the spleen; and accordingly the term *splenization* of the lung has been given to this stage of its inflammation, as hepatization has to that which succeeds it. In this stage of engorgement the mucous membrane of the smaller bronchial ramifications is of a deep red colour. The portions most engorged, although their specific gravity is increased, will nevertheless almost always float on water.

Now it is necessary to caution you in the outset, against a very frequent source of fallacy in respect to this condition of inflammatory engorgement. Such a state of the pulmonary substance as I have been describing, you will meet with in half, at least, of the dead bodies which you may have to examine; and you must not necessarily infer therefrom that the persons deceased had *inflammation* of the lungs. There is almost always some degree of *mechanical* engorgement of the back part of the lungs; or of that part which has been undermost during the last hours of life, or after death; and the two kinds of engorgement can scarcely be distinguished from each other by their anatomical characters alone. Andral at one time held, indeed, that if the engorged part were more friable, more easily torn or broken down under pressure than natural, that was sufficient evidence of its inflammation; but he afterwards saw reason to change that opinion. We judge by the *situation* of the engorgement sometimes: if it be not in a depending part of the lungs, it is surely inflammatory. We judge also by the antecedent symptoms.

If the inflammation continue, the lung undergoes a further alteration, and presents the following characters. It is still red—externally and within; but it crepitates no longer under pressure; and it sinks in water: it contains in fact no air. Its cut surface presents sometimes a uniform red colour; sometimes a slightly mottled or variegated appearance, produced by an intermixture of specks of the black matter of the lung, and of the interlobular areolar tissue, which is less red than the other parts, and more than naturally obvious to the sight; but the spongy character of the organ is lost: it is evidently solid; and the cut surface very much resembles the cut surface of the liver. Hence Laennec, and after him most other writers, have applied to this altered condition of the lung the term *hepatization*. There still flows out, under pressure, from the surface, when a fresh incision is made, some red fluid, but

FIG. 50.



Fig. 50.—Appearance of lung-tissue in red hepatization. *a*. Air vesicles filled with inflammatory cells, some of which are nucleated; *b*. Concrete albuminoid masses; *c*. Granule cells. From Da Costa.

FIG. 51.

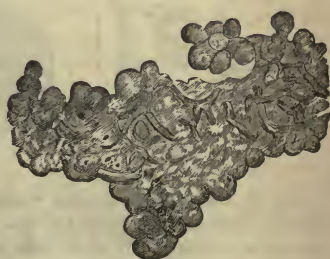


Fig. 51.—Lung in a state of red hepatization; the air-cells are filled with corpuscular fibrin or exudation-matter, and are surrounded by enlarged and congested vessels. Magnified twenty diameters. (Bayle's Granulations.) From a man æt. 66, who had double pneumonia.

it is much less in quantity than in the former degree; and it is not foamy; and if the surface be gently scraped with a scalpel, you may often perceive in the red fluid so collected, some traces of a thicker and yellower matter, the first indication of commencing suppuration. The hepatized lung is denser and more solid than before, but it is also more friable; more easily crushed and broken: and this results from the softening of the areolar tissue which holds its component parts together.

If you tear a portion of hepatized lung, and examine the torn surface with a magnifying glass, the pulmonary tissue will appear to be composed of a crowd of small red granulations, lying close to each other. These are, I presume, the air-vesicles clogged up, thickened, and made red, by the inflammation. As no air is contained in the lung in this stage of the inflammation, it follows that if the entire organ be involved in the disease, it will not sink down when the thorax is laid open; and will therefore appear to be increased in bulk. It is swelled, in fact,—just as other inflamed parts are swelled—by the congestion of its vessels, and by the effusion of blood, or of some of the constituent parts of the blood, into its hollows and interstices. The marks of the ribs are frequently visible on the surface of the distended lung. The texture of the lung in this condition is sometimes so rotten, that a moderate degree of pressure between the fingers will suffice to reduce it to a state of pulp; and this diminution of consistence has made Andral quarrel with the term *hepatization*: and he proposes to call this second stage of pneumonia, red softening, *ramollissement rouge*. All this is very unimportant, provided that you recollect the sense in which either nomenclature is employed. But as Laennec and Andral are both great authorities, and both have their disciples in this country, it is well that you should understand their language.

In a degree still further advanced, the pulmonary tissue, dense, solid, and impervious to air, as in the last stage, undergoes an alteration of colour: it presents a reddish yellow, or straw, or drab, or stone colour; or it is of a greyish hue, sometimes mottled with red, or with the black pulmonary matter. The little granulations which I just now mentioned are whitish or grey, instead of being red; and the texture of the lung is still more rotten and friable than before. It is full, in fact, of puriform matter, which is sometimes so abundant that it oozes out plentifully when incisions are made into the lung: or it may be made to exude by gentle pressure. The grey pus shows itself upon the cut surface in the form of minute drops. The more the pulmonary texture is soaked or drenched with this fluid, the softer and more friable it becomes. When crushed between the thumb and fingers, it is reduced to a yellowish grey pulp, exactly like the fluid itself only rather more consistent

FIG. 52.

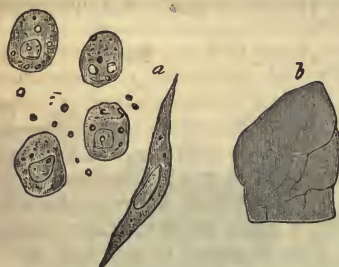


FIG. 53.

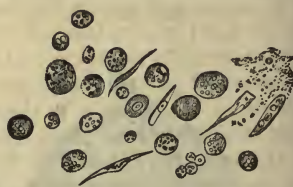


FIG. 52.—Elements in peculiar “yellow” condensation, in a lung which was completely and uniformly infiltrated, magnified 480 diameters. *a*. Nucleated corpuscles and fibroid cells; *b*. Basis-substance in which many of the cells are imbedded. From *Da Costa*.

FIG. 53.—Microscopic characters of the contents of an air-vesicle in grey hepatization, consisting of granular matter, pus-corpuscles, exudation-cells, and cylindrical epithelium.

And by gently forcing the finger into any part of the parenchyma in this state, a small cavity may be made, which soon fills with pus, and which might readily be mistaken for a recently formed abscess.

Laennec has called this third stage of the process of inflammation in the lung, *grey hepatization*, or *purulent infiltration*. Andral denominates it *grey softening*—*ramollissement gris*. In fact, it consists in *diffused suppuration* of the pulmonary texture. And it is a very remarkable circumstance, and one which the researches of modern times have brought to light, that, in the lung, inflammation going on to suppuration, does not lead to the formation of a circumscribed abscess, as it does when it affects the areolar tissue, or the parenchymatous tissue, in other parts of the body. Abscess of the lung used to be spoken of as a very common thing; but it is

a very rare thing. In several hundred dissections of persons dead of pneumonia, made by Laennec during a space of more than twenty years, he only met with five or six collections of pus in the inflamed lung. Once only did he find a *large* abscess

FIG. 54.



Elements found in the lung in *grey* hepatization. *a*. Granular exudation corpuscles and free oil; *b*. Corpuscles when treated with ether; *c*. Fibrous tissue infiltrated with oil globules—fibres very indistinct; *d*. Small vessel, the walls of which are coated with granules; *e*. Pus corpuscles, rather dark and granular. *From Da Costa.*

of that sort. Once only has Andral seen a real abscess of the lung form as a consequence of pneumonia. You may find collections of pus in the lungs sometimes, occurring in connexion with the inflammation of veins. Several instances of that kind have happened very recently in patients who have died in the Middlesex Hospital. But these are not ordinary cases of pneumonia. I need scarcely caution you not to take tubercular vomicae and cavities, containing pus, for genuine abscesses of the lung. These, and the phlebitic deposits of pus, are not *exceptions* to the general statement: they arise from different forms of disease: and you will find a circumscribed collection of pus, surrounded by hepatized lung, as a consequence of common pneumonia, to be an exceedingly rare event.

Can we account for this in any way? I do not know that any satisfactory explanation of the fact has ever been offered. But I would submit to your consideration what has occurred to my mind on this subject. When I was speaking of inflammation in general, I pointed out to you the remarkable influence which the presence of atmospheric air in contact with the inflamed part has in accelerating, or determining, the event of suppuration. In a recent cut through the skin, the admission or exclusion of the air to the cut surface will make all the difference between the adhesive and the suppurative inflammation; and so in other cases which I then mentioned, and will not now trouble you by repeating. Now it seems to me that the same principle obtains in inflammation of the lung. First, there is an effusion of serum and blood, then of lymph and blood; but the air, passing into the surrounding sounder tissue, and penetrating for a time even the inflamed portion itself, causes the suppurative process to supersede the adhesive; and so no wall of circumvallation is formed by the coagulable lymph, as is the case in areolar tissue, which is not accessible by the air. Whether this be a sufficient explanation of the *fact* (all explanations being the resolving a given fact into a certain class of other facts more general and comprehensive), I say, whether it be a reasonable and satisfactory explanation, you will judge: at any rate it may serve to impress upon your memory that fact which it endeavours to elucidate.

Gangrene is sometimes, but very seldom, the result of acute inflammation of the lung. It is almost as uncommon as the formation of an abscess. Yet it certainly does now and then occur, as a consequence of acute inflammation of the pulmonary substance. It is somewhat more common (though under any shape rare) as an independent and primitive affection. Sometimes it occupies a large portion of the lung, and is uncircumscribed; and sometimes it is more limited. The colour of the part which has thus perished under inflammation, is dark, of a dirty olive, or greenish-brown colour. The gangrenous portion is moist and wet; sometimes of the consist-

ence of the engorged lung; more commonly softer, and even diffuent; and it stinks most abominably. This horrible odour is in truth, during life, the most distinctive character of gangrene of the lung. It sometimes renders the room in which the unhappy patient is lying, scarcely endurable. I should have stated before that the puriform infiltration of the third stage of pneumonia is attended with no fœtor.

There are some other points, connected with, or learned from investigating, the morbid anatomy of pneumonia, which I may as well take this opportunity of telling you, before we go on to consider the symptoms, physical and general, of that disease.

There are two lungs, just as there are two tonsils, and two eyes; and in the one case as well as in the others, inflammation may affect both organs at once, or it may affect one of them alone. Technically speaking, pneumonia may be either double or single. Again, the inflammation may occupy a part of one lung, or the whole of it: in other words, it may be partial or general; but it does not affect all parts, or both sides, indifferently or capriciously. In the first place, it is (why I know not) greatly more common on the right side of the body than on the left. I will give you some statistical statements collected by Andral, in respect to this point. Of one hundred and fifty-one cases of pneumonia, noticed at La Charité, ninety were of the right lung alone; thirty-eight only of the left alone; seventeen of both sides at once; and in six the situation was uncertain. He was at the pains of collecting the particulars of fifty-nine other examples of pneumonia, from different authors, so fully described as to leave no doubt about the nature and situation of the disease. Among these, the inflammation existed in the right lung alone in thirty-one patients; in the left alone in twenty; and on both sides at once in eight. Hence, taking both series of observations together, we have two hundred and ten cases of pneumonia; and there were one hundred and twenty-one in which the right side was solely the seat of the disease; fifty-eight in which the left; twenty-five in which the pneumonia was double; and six in which the seat was uncertain. So that, at this rate, pneumonia is more than twice as common on the right side as on the left; and does not occur on both sides together so often as once in eight times.

Again, with regard to that *part of the lung* which is most obnoxious to inflammation, there are remarkable differences. It is well known, and it is a very important fact in respect to diagnosis in some cases, that the lower lobes are more liable to inflammation than the upper. I speak, of course, of active idiopathic inflammation. But this circumstance, much insisted on by Laennec, and quite true in the main, has perhaps been somewhat exaggerated. I have not had leisure to frame any numerical statement of the cases that have come under my own observation, but the general impression which they have left upon my mind is in favour of the correctness of Laennec's statement—that pneumonia generally commences in the lower lobes, and spreads upwards frequently to the superior lobes. But I may adduce Andral's statistical representation in respect to this question also. Of eighty-eight cases of pneumonia, he found that the inflammation affected the inferior lobe forty-seven times, the superior lobe thirty, and the whole lung at once, eleven.

Inflammation of the bronchi constantly accompanies inflammation of the parenchyma. The mucous membrane presents a red colour both in the large and in the small branches of the air-passages. And when a single lobe is inflamed, it has been observed that the redness of the mucous membrane existed in those bronchial tubes alone which were distributed to that lobe. You may have bronchitis without pneumonia: but pneumonia without a corresponding extent of bronchitis, is perhaps never seen.

The majority of cases of pneumonia are attended also with a degree of inflammation of the investing membrane of the lung: there is some pleurisy. So frequently indeed is this the case, that certain writers, Andral among others, call the disease by the compound name of *pleuro-pneumonia*. However, pneumonia may and does sometimes occur without any concurrent pleurisy. Of the latter complaint I must speak by itself; and I merely notice now the frequent combination of the two—the occurrence of a slight degree and extent of pleuritis in most cases of pneumonia—that you may the better understand some of the general symptoms of pneumonia.

Now such being the changes which the lungs undergo when inflammation affects the pulmonary texture, we may next inquire what signals of its existence the inflammation holds out; and how far we, not having the power of *seeing* what is going on within the cavity of the thorax, may nevertheless ascertain the important processes which are there transacted.

If the ear be applied to the surface of the chest, with or without the intervention of the stethoscope, and the portion of lung subjacent to that surface happen to be in the first stage of inflammation, that of engorgement, what does the lung, so suffering, say? what audible notice does it give of its morbid condition? Why it speaks very plainly. You hear a peculiar crackling sound: the smallest and finest-possible kind of crepitation: which has been happily illustrated by saying that it resembles the multitudinous little crackling explosions made by salt when it is scattered over red-hot coals. Andral has another resemblance for it, and not a bad one; he says the noise is often like that which is produced by rumpling a very fine piece of parchment. Dr. Williams observes that a pretty correct idea of this sound may be obtained in a ready way, by rubbing between the finger and thumb a lock of one's own hair, close to the ear. Laennec calls this *crepitant rhonchus*: I would speak of it as *minute crepitation*; or the *crackling of pneumonia*. This may be heard in a very limited spot in the beginning. And what an important sound it is! "It is a direct symptom having immediate reference to the structure of the part. And (says Dr. Latham) if we consider what the part is, and what the disease; the part, the lungs, and the disease, inflammation: we cannot too highly value this single symptom (simple and mean as it may seem) which gives the earliest and surest intimation that such a disease has begun, as tends to disorganization, and the inevitable loss of life, unless quickly arrested by its counteracting remedy."

At first, when you catch the inflammation in its earliest stage, this minute crepitation, which announces *commencing* engorgement of the part, is heard mingling with the ordinary vesicular breathing; obscuring the natural sound, though it does not yet entirely cover it. But as the inflammation advances, the crackling becomes more and more pronounced, until at length it totally supersedes the natural sound. So long as the natural vesicular breathing overcomes the crackling, we may conclude that the inflammation is slight. But if the crackling should, in its turn, become predominant, if it should ultimately mask the murmur of respiration entirely, that infallibly denotes the advance of the pneumonia, and teaches us that it tends to pass from the first into the second degree. But the crackling sound does not *long* remain in any part. As the case proceeds, the sound is less and less heard, and at length is not heard at all, in that spot; and it may be succeeded by one of two very different things. Its place may be taken by the natural respiratory murmur again. When this is so, it denotes the *resolution* of the inflammation. But the crackling may cease, and either no sound be heard at all in its stead, or another morbid sound which I shall presently describe: and this teaches us with absolute certainty, that the disease is growing more severe and serious; that the lung is becoming, or has become, *hepatized*.

Let us inquire, for a moment, before we go any further, what is the nature and where the seat of this minute crepitation, so characteristic of the commencement of pulmonic inflammation. With respect to its *seat*, I apprehend, there can be no question. It proceeds from the very smallest ramifications of the bronchi, and from the air-vesicles themselves. The common opinion is, and such, I confess, is mine, that the sound is the same in cause and kind, only different in degree, with the large and the small crepitation described in a previous lecture: that it results from the passage of air through liquid; from the formation and bursting in quick succession of a multitude of little air-bubbles. The bubbles are necessarily minute, for they are formed, and they explode, in very slender tubes. This is Andral's view of the matter. Laennec does not appear to have formed very clear notions on the subject. But a different explanation has been offered by a well-known and able writer on the auscultatory signs of disease, in this country: I mean Dr. C. J. B. Williams. He holds that the distended blood-vessels, and the interstitial serous effusion, press upon the minutest bronchial ramifications, and obstruct, without wholly preventing, the passage of the air through them: that these small tubes are lined by a viscid secretion, such as is expectorated, and such as I shall have to describe: that the sides of the tubes stick together in consequence of the presence of this viscid matter; and that it is the separation of these adhering sides by little portions of air which successively pass in and out, that gives rise to the characteristic sound. However, what it is important to remember is, that the crackling sound proceeds from the minutest divisions of the air-tubes, and from the ultimate vesicles of the lungs.

[Dr. Boling, of Alabama, describes "a fine mucous or crepitant rhoncus, seemingly seated in the larynx, loud enough to be heard distinctly at the distance of two or three feet from the patient, and so *persistent*, that it is not removable, or but momentarily, by any effort to expectorate which the patient may make, while at the same time there are present none of the signs of bronchitis or laryngitis." The patient seems indifferent to its presence. On applying the stethoscope just above the clavicles, it is found, to proceed from the apex of the lung. This is *diagnostic of pneumonia of the apex of the lung*, one of the most dangerous forms of the disease.—*Amer. Jour. Med. Sciences*, July, 1847. — C.]

Sometimes, I say, when this crackling ceases, the ear applied to the corresponding surface of the chest, feels it heave up in inspiration, but catches no sound at all. Much more commonly, however, a *new* sound reaches the ear. It is not the vesicular rustle; it is not the minute crepitation: but a whiffing sound is audible, like that produced by blowing through a quill. Little gusts of air are puffed in and out; most distinct, often, at the termination of a slight cough or hem. This is the sound to which the term *bronchial respiration* has been given: and the name expresses well the fact. I mentioned before that in the healthy state we do not hear the air pass through the larger bronchi during inspiration and expiration: the sound doubtless is made, but it is obscured and hidden by the smooth rustle of the vesicular breathing, which comes from the spongy lung surrounding the large divisions of the bronchi, and intervening between them and the ear. But that spongy structure is now filled up. The hepatized lung admits air to pass through the larger bronchi, which are still patent, but it admits none into the vesicles and smaller tubes. It crepitates not when pressed between the thumb and finger; in fact, it is converted into a solid substance, and conducts the sound, in the living body, as any other solid substance might do: and therefore the whiffing, blowing, gusty sound of the breath, as it enters and departs from the larger bronchial tubes, which still remain open, is conveyed to the ear, and *bronchial respiration* is heard. At the same time, and in the same place, another auscultatory phenomenon generally arises, and admits of a similar explanation. The *voice* of the patient descends into the pervious bronchi, and is conveyed to the ear of the listener through the solid lung: and it is quite altered by that circumstance. The tone of it is modified; it sounds like the voice of one speaking through a tube. It is totally different from the same voice heard through the healthy lung at the corresponding point on the other side. It approaches in distinctness and quality, but it does not reach, the sound of the speaker's voice heard through a stethoscope placed over his trachea. A humming and muttering are audible, but the words are not distinctly articulated into the ear. It is hard to describe these things in words. Three minutes, at the bed-side of a patient in whom the bronchial breathing and the bronchial voice were tolerably well marked, would put you in possession of them for ever. They are striking sounds, requiring no fine tact to distinguish; and they are exceedingly informing sounds. But I must resume this subject when we meet again.

LECTURE LI.

Pneumonia continued: its general symptoms; pain, dyspnœa, delirium, cough, expectoration. Course of the disease. Prognosis. Treatment.

I WAS describing, at the close of the last lecture, the auscultatory signs which lead us to the knowledge that the inflamed lung, in a case of pneumonia, has passed from the first into the second stage of inflammation, and become solid, or hepatized. The altered condition of the organ gives rise to altered sounds. Instead of the vesicular breathing, which is the natural sound; or of the minute crepitation, which is

the sound belonging to the first stage of the inflammation; we either hear no sound at all, though we feel the chest heave up against our ear, or we hear what I described under the denomination of *bronchial respiration*; that is to say, a blowing sound, which is conveyed to the ear from the larger and still pervious branches of the bronchi, through the solid portion of lung around them, and through the solid walls of the chest. This is what the listener hears when the patient *breathes*. And when he speaks his *voice* is heard, much more resonant than is natural, much more resonant than in the corresponding spot on the opposite side of the chest, entering the same open air-tubes, and conducted to the ear by the dense and solid lung. We thus become acquainted with two entirely new sounds; sounds which are never heard in the healthy state of the lungs; *bronchial respiration*, and *bronchial voice*, or *bronchophony*: and you will do well to remember these two sounds, and to familiarize your ear with them; for they speak a most significant language in *other* pulmonary diseases, as well as in pneumonia.

But I say, sometimes we hear these morbid sounds, in the case in question, and sometimes we hear *no* sound at all during the breathing. How is that? Why the existence and degree of the bronchial respiration, and bronchial voice, vary according to the place and extent of the inflammation. These morbid sounds are most plainly marked, where the number and size of the bronchial tubes involved in the hepatization are the greater. They are most distinct, therefore, when the inflammation occupies the upper part of the lung; or the central parts, what are called the roots of the lungs: and when it extends thence to the surface: but when the lower portions alone are inflamed, or the inflammation is merely superficial or partial, they may not be heard at all. Again, if the hepatization should be so general and complete, as to prevent the chest, on the affected side, from expanding—you will, in that case, hear *no* bronchial *respiration*; for the air in the large bronchi must be stagnant. *Bronchophony*, however, may remain.

When we have the bronchial respiration, usually also we have dulness on percussion. The degree in which this is present will depend upon the circumstances of the case. If a portion of crepitant and permeable lung, even a thin portion, should intervene between the inflamed parts, and the walls of the chest, there will still be resonance on percussion, though it will not be exactly the natural resonance. If the hepatized part come close up to the ribs, the sound elicited by mediate percussion will be flat or dead. With all this, you will generally hear, in the sound lung, if the whole of the other be engaged in the inflammation;—or in those parts of the inflamed lung that are healthy;—you will hear, I say, *puerile respiration*: and this is a strong confirming symptom that a part of the breathing apparatus is spoiled, and that the remaining part is endeavouring to compensate for its deficiency.

Now this period in pneumonia, when no sound but bronchial breathing is audible during respiration, is a period of anxious and painful interest. We cannot tell whether the lung will revert gradually to its healthy state; or whether it is passing into the third stage, that of purulent infiltration. But taking first the most favourable of these two suppositions—what happens? Why, *there*, where for a while we heard nothing but bronchial respiration, a slight crepitation begins again to be distinguishable, especially at the end of each act of inspiration: gradually this increases in extent and intensity, and as it increases, the bronchial breathing, and the bronchial voice, become proportionally less distinct, because the texture of the lung is again becoming permeable by air, and therefore a worse conductor of sound. By degrees, the bronchial breathing and voice disappear altogether; the vesicular murmur begins again to mix with the crepitation, and at length supersedes it; and the lung is restored to its previous fitness for the purposes of respiration. The same symptoms therefore recur, over again, but in a reversed order; the *returning* crepitation is however coarser and larger, and less regularly diffused, than that of the *advancing* pneumonia:—and even when nothing is heard in the ordinary condition of the breathing, but the natural vesicular rustle, some crepitation is found for some little while to mingle with it towards the end of a full inspiration. Next, let us take the *worst* of the two suppositions. Auscultation has traced the disease, *through* its stage of engorgement, and *into* its stage of hepatization. Can it trace it further? I believe not with any certainty. We cannot say whether the lung remains in the state of hepatization (as it may remain), or whether it has passed into the third

stage. But at last, if the structure of the lung break down, and a portion of it be expectorated, air finds its way into the vacant spot, and gives rise to large gurgling crepitation. But other signs sometimes come to our aid when this state has been reached.

We often find, after death, the three degrees of pneumonia existing in different parts of the same lung; and therefore it is not to be wondered at that the different parts of the chest should during life yield sounds indicative of each of those degrees, or at least of the two first; minute crepitation *here*, bronchial breathing, and bronchophony, and dullness on percussion *there*, and in another spot, *no* sound at all, or on the other hand, *peurile* respiration.

Again, it must be confessed—and I am desirous of confessing it, for I am sure that the method of auscultation is brought into undeserved suspicion and disrepute by attempts made to assert its all-sufficiency in all cases—it must be confessed that in some instances, although pneumonia exists, the ear is able to collect nothing of it: nothing indicative of its situation, or of its extent, or even of its existence. The pulmonary expansion is clear, all over the thorax; nay, much more strong than is natural; and this circumstance justifies the belief that, from some cause or other, not necessarily from pneumonia, a portion of the lung has ceased to discharge its function, and the other portions have taken it up. This failure on the part of auscultation happens when the inflammation occupies a small portion only of the lung, and that portion is central, or deeply situated; at a distance from the walls of the chest.

Such are, then, the physical signs that accompany and reveal the successive changes of texture, destructive and reparatory, which take place in inflammation of the lungs. I do not know whether I have made them clear to you; but I know that no very long apprenticeship, if I may so speak, in the wards of a hospital, will be sufficient, with a little guidance, to render you master of them. There are indeed varieties, and modifications, and exceptions, which nothing but such an apprenticeship can ever teach you. Of these it would be idle and unprofitable for me here to speak: and I go on to consider the *general* signs of pneumonia; some of which, either in themselves, or in combination with the *physical* signs, are of no less importance than these.

In the majority of cases the commencement of inflammation of the lung is marked by shivering, followed by heat and increased frequency of pulse; in one word, by inflammatory fever: and at the same time, or presently after, a stitch in the side comes on, with cough, and a sense of oppression in the chest. In other instances, the disease steals on more insidiously, and succeeds to bronchitis; the inflammation appearing to propagate itself by little and little from the larger to the smaller bronchi, and ultimately to reach the air-vesicles themselves, and the interstitial textures; and this may be accomplished with or without the sharp pain or stitch in the side. At first the cough may be dry, but it soon is attended with a very characteristic sort of expectoration. The dyspnoea is sometimes but slight in the outset; sometimes considerable.

Apart, therefore, from the physical signs, we may say that the usual symptoms of pneumonia are pain, more or less severe, on one side of the chest; dyspnoea; cough; a peculiar expectoration; and fever.

[Dr. Boling, of Alabama, states that he has frequently observed in pneumonia, a symptom which he does not remember to have seen mentioned by any author, and which he has never noticed in any other disease. It consists in a deposition on the teeth, just along the margin of the gums, of a matter of different shades of colour, from a light orange to a dull vermilion, forming a line of about the sixteenth of an inch wide; deeper in tint at the gums and paler as it recedes, unlike the blue line in lead poisoning, and the line at the same part of a deeper shade than the rest of the gum, noticed by Dr. Theophilus Thompson in phthisis, (*London Lancet*, Sept., 1852). The deposit in pneumonia is upon the teeth; from which by careful wiping, it may in a great measure be removed, leaving, occasionally, a somewhat durable stain upon the enamel. Dr. Boling thinks that it is probably an exudation from the margin of the gums. It is not a deposition from the colouring matter of the bloody sputa, as it has been met with in cases unattended with the expectoration of bloody matter, even in cases of latent pneumonia, without cough or expectoration. In one instance, from the presence of this deposit alone, Dr. Boling was lead to suspect the presence of the latter form of the disease, and actually detected it by auscultation. It is probable, Dr.

Boling remarks, that miasmatic poisoning may lead to the development of the deposit in question, inasmuch as, if it were of as frequent occurrence in other localities as in the malarial region in which he has observed it, it would have been noticed before. Still he does not recollect of ever meeting with it in any of the forms of uncomplicated miasmatic fever. Dr. Boling thinks that he has observed the presence of the deposit in at least one-third or one-fourth of the cases of pneumonia treated by him. The cases in which it occurs are generally severe; it is very rarely found in mild cases.—*American Journ. Med. Sci.*, July, 1852.—C.]

The pain in pneumonia appears to exist only in those cases in which the inflammation of the lung is accompanied by some degree of pleurisy. But these are the most numerous cases. It is most commonly experienced on a level with, or a little below, one or other breast; but it may exist in almost any other part of the thoracic parietes. Generally it is most severe at the beginning, declines by degrees, and ceases altogether for some time before the *pneumonia* ceases. It is aggravated by cough; by a full inspiration; often by sudden changes of posture; by pressure made upon the ribs or intercostal spaces; or by percussion of that part. For the same reason the patients cannot lie on the painful side. Andral declares that in all the individuals in whom he had noticed this pain, and who died, he found the pleura inflamed, and covered more or less with coagulable lymph; and, on the other hand, that he had constantly known the absence of pain coincide with a sound condition of the pleura. When there is no sharp pain, there is, however, some morbid sensation, of trouble, or tightness, or weight, or heat, on the affected side. He quotes, with approbation of its justness, the ancient observation respecting pneumonia—"Affert plus periculi quam doloris." When I come to speak of pleurisy as a distinct and substantial affection, I shall revert to this pain.

It is, or it was, a common doctrine, that one of the general symptoms of pneumonia relates to the posture which the patient assumes; that the *decubitus*, to speak technically, is on the side affected. The truth, however, is what I have just now stated. The *breathing*, indeed, is more impeded when the patient lies on the sound than when on the diseased side; but in point of fact, patients labouring under this disease almost all lie upon their backs; the *decubitus* is dorsal. The disturbance of the breathing deserves some notice. In general it bears a direct proportion to the extent and severity of the inflammation. But there are many exceptions to this. In some persons the inflammation of even a small portion of one lung is attended with great constraint or hurry of the respiration. In others, who have a much larger portion of the pulmonary tissue intensely inflamed, the dyspnoea appears to be but slight. So that the degree of difficulty of breathing is not a *certain* measure of the seriousness, or rather of the extent and the degree, of the inflammation. It is probable, that if we knew of what kind was the ordinary breathing of the individuals thus differently affected, we should find that they whose respiration is generally indistinct, or noiseless, who do not seem to *want* all their lung for the purpose of breathing, would best bear to have a part of it inflamed; and *vice versâ*. *Cæteris paribus*, inflammation of the upper lobe causes greater dyspnoea than inflammation of the lower. I may observe further, with respect to dyspnoea in general, that you must not trust implicitly to what patients tell you on that head. They will often deny that they have any shortness of breath, when one may see them respiring with unnatural rapidity, or observe that in their discourse they pause between every three or four words to take breath.

However, the dyspnoea that occurs in pneumonia varies greatly both in degree and in kind in different cases. Sometimes it is so slight that the patient is not conscious of it, and the physician scarcely perceives it. Sometimes it is so extreme, that the patient, entirely regardless of what is going on about him, seems wholly occupied with respiring; is unable to lie down; can scarcely speak; his face becomes lividly red or pale, and is expressive of the utmost anxiety; his nostrils are expanded, his shoulders elevated, and all the muscles which are auxiliary to the diaphragm and intercostals, in full and evident action. In one word, the breathing is *laborious*. Now this is the sort of dyspnoea which I mentioned before as being characteristic of obstructive bronchitis. When it accompanies inflammation of the lungs, we may conclude that bronchitis is superadded to that disease. Sometimes again the respiratory movements are simply frequent and very short or shallow, as if the air were not able to penetrate

beyond the primary divisions of the bronchi. Dr. William Gairdner, who has closely studied these differences and their meanings, expresses them clearly in the following sentences:—The dyspnœa of *pure pneumonia* is a mere *acceleration* of the respiration, without any of the heaving or straining inspiration observed in bronchitis, or in cases where the two diseases are combined. So much is this the case, that I have repeatedly observed patients affected with a great extent of pneumonia in both lungs, and in whom the extreme lividity, and the respirations, numbering fifty or sixty in the minute, showed infallibly the amount to which the function of the lung was interfered with; and who nevertheless lay quietly in bed, breathing without any of the violent effort, or the disposition to assume the erect posture, so constantly accompanying the more dangerous forms of bronchitis. If this freedom from dyspnœa and laborious breathing be not uniformly characteristic of true pneumonia, it is because that disease comparatively seldom exists uncomplicated by some degree of bronchial affection."

Between these states of extreme rapidity or extreme labour of breathing, and the slightest hurry or embarrassment of respiration, there are of course many degrees.

Delirium is a symptom which very frequently occurs in the course of an attack of pneumonia; and a very ugly symptom it is. It denotes that the due arterialization of the blood is largely interfered with by the pulmonary affection. It measures, in one sense, the quantity of mischief which is going on within the thorax: and it is a direct evidence that the pectoral mischief is telling, through the circulation of venous blood, upon the *brain*.

The cough, in pneumonia, has no particular character; and affords but little information. It does not usually take place in paroxysms; and its severity and frequency are not always proportioned to the intensity and extent of the inflammation. It is usually dry in the outset; but in a few hours it is accompanied by the expectoration of peculiar sputa, which constitute one of the most certain indications of the presence of pneumonia: and as this is a symptom which every one can easily recognise, I will describe this characteristic expectoration and endeavour to explain the cause of it.

The expectoration of pneumonia, when well marked, consists of transparent and tawny or rust-coloured sputa, uniting, in the vessel containing them, into one jelly-like and trembling mass: and of such viscosity that the vessel may be turned upside down, and strongly shaken, without their being detached from its bottom or sides. It cannot be said that when there is no such expectoration as this, there is no pneumonia: but it may be affirmed that where we do find such expectoration, there almost certainly we have pneumonia. At the outset of the disease, either nothing is spat up, or simply some bronchial mucus: but on the second or third day generally, the matters expectorated assume the characteristic appearance: *i. e.*, they come to be composed of mucus, intimately united and combined with blood. It is not that the sputa are *streaked* with blood, as often happens in *bronchitis*: nor have we the *unmixed* blood of *hæmoptysis*. But the blood and the mucus are amalgamated together: and in proportion to the quantity of the former, the sputa become of a yellow colour, or of the colour of rust, or of a decided red: and at the same time they become glutinous and tenacious; they adhere together, so as to form one transparent homogeneous mass. So long as this mass flows readily along the sides of the vessel when it is tilted, so long have we reason to *hope* (judging from that circumstance alone) that the inflammation of the lung does not pass its first degree. But, as I said before, the sputa often acquire an extraordinary degree of viscosity: so as no longer to separate themselves from the vessel when it is inverted: you cannot even shake them out. When this happens, we are obliged to *fear* that the pneumonia reaches its second degree. In fact, when the sputa become thus rusty and very viscid, the stricken chest almost always returns a duller sound, and the vesicular breathing is abolished, and bronchial respiration takes its place. The pneumonia is then at its acme; and the expectoration remains for some time stationary. At length, if the inflammation recede, the sputa become again less tenacious, less red or yellow, and more like the expectoration of mere catarrh. But if the disease go on from bad to worse, the rust-coloured sputa may continue to the end. Commonly there is *less* expectoration in that case, or even none at all. Not that the mucus ceases to be secreted, but that its *excretion* is no longer possible: either on account of its

extreme tenacity, or on account of the patient's debility. The sputa then accumulate in the bronchi, trachea, and larynx, in succession: they fill up the air-passages, and suffocate the patient. In some instances the expectoration, in the advanced stages of the disease, consists of a fluid having the consistence of gum-water, and of a brownish red colour: like (as Andral says) liquorice-water, or plum-juice. He states that the mere occurrence of this kind of expectoration has led him to announce the existence of the third stage of pneumonia; and that the subsequent examination of the dead body has seldom failed to justify his diagnosis. Sometimes again, during the third stage, very perfect pus is excreted.

That the colour of the sputa peculiar to pneumonia depends upon an intimate union of blood with the altered mucus, is perfectly obvious when that colour is deep. And even when this transparent mucus is yellow, you may satisfy yourselves by the following simple experiment that the source of the colour is the same, and that the yellowness does not result, as some have fancied, from an admixture of bile with the matter expectorated. If to water, rendered viscid by dissolving a certain quantity of gum in it, you add blood, drop by drop, you will obtain in succession, all the shades of colour that are presented by the pneumonic sputa: first a yellow tinge; then a tawny yellow which loses itself in a red, and comes to represent the colour of the rust of iron; and lastly an intense red. The sputa may, indeed, sometimes, but I believe *that* does not often happen, be coloured by bile: but bile is not the source of the yellowness which they assume in cases of pneumonia.

Sputa composed of very red mucus, indicate pneumonia less surely than such as are tawny. The very red masses, in which there is more blood than mucus, often belong to pulmonary apoplexy.

Although these rust or orange-coloured sputa are commonly present during the more active period of pneumonia, and, as far as my experience goes, are peculiar to that disease, you ought to be aware that they do not *constantly* accompany it. Sometimes the matters expectorated are like those of catarrh: and sometimes there is scarcely any expectoration at all.

When the pneumonia passes into gangrene—which I repeat is an exceedingly rare consequence of inflammation in that organ,—the expectoration becomes of a greenish, or reddish, or dirty grey colour; is more liquid, and exhales a fœtid smell, resembling that which proceeds from gangrene of the external parts. So again the puriform expectoration which at length ensues in the rare cases of circumscribed abscess of the lung, from pneumonia, is horribly offensive.

I have now described, *seriatim*, the main symptoms, general and physical, which mark the existence and the progress of pneumonia. And in order to give you a just notion of each, I have spoken of them separately. But they *exist together*; and they must be *studied together*: and some will be found to confirm or to correct the indications that might be drawn from the others. I must briefly therefore run over the phenomena of the disease we have been considering, as, in most cases, it actually presents itself.

The first symptom felt is commonly pain in the side; which may or may not have been preceded by rigors. At the same time the breathing is constrained; and the patient coughs without expectorating. At this period, the ear may generally detect a slight degree of minute crepitation, which is not strong enough to mask entirely the vesicular rustle; and the stricken thorax still sounds well; and there is fever withal. This assemblage of phenomena constitutes the first period of the disease. From the second to the third day, new symptoms appear. The expectoration, hitherto absent, or merely catarrhal, becomes characteristic; being at first moderately viscid, and having a degree of colour proportioned to the variable quantity of blood which it contains. The minute crepitation increases, and drowns or supersedes the natural respiratory murmur: the clear sound produced by percussion begins to diminish on that side on which the crackling is heard and the pain is felt; and that pain is commonly less sharp than in the beginning. The dyspnoea augments, as is quite apparent from the short and frequent inspirations made by the patient. If the pain be acute, he cannot lie, on that account, on the side affected; neither can he place himself on the sound side, because in that position his respiration becomes more embarrassed; he remains therefore, almost constantly, lying upon his back.

In this condition of pneumonia, though the disease may be severe, the inflam-

mation is as yet in its primary stage. It often remains stationary for a while, and then recedes, and terminates by resolution. The dyspnoea diminishes, the slight dulness of sound disappears, the crackling is gradually displaced by the natural murmur of the pulmonary expansion, the sputa again become those of simple bronchitis, the fever subsides, and ceases; and all is well again.

At other times, instead of retrograding towards resolution, the pneumonia becomes more intense, or rather more extensive, without passing beyond its primary stage; and the patient may die while it is still in that stage. But this is unusual. Ordinarily, if the inflammatory engorgement do not cease by resolution, and the symptoms that announce it are exasperated, we must expect that the second stage will be established. And we may be certain that it exists when we observe the following phenomena:—the breathing becomes more and more constrained, short, accelerated; the speech ceases to be free; the patient can do no more than pronounce a few interrupted words in a panting manner. The sputa acquire such a degree of viscosity, that they can no longer be detached from the vessel by shaking it; the sound afforded by percussion, on the side affected, is decidedly dull; generally there still is crepitation, less fine in its character however than in the outset, without the admixture of any pure vesicular breathing; sometimes the crepitation ceases entirely, and either no sound at all is perceived by the ear, or, in the part where the percussion is dull, bronchial respiration is heard, and this is almost always accompanied with bronchophony. The patient continues to lie on his back.

In this degree of the disease the prognosis is always uncertain. The patient often sinks rapidly, and dies from apnoea. Yet even in this degree resolution may still take place. In that case the dulness on percussion diminishes; the bronchial breathing disappears; we hear afresh a coarse kind of crepitation, at first alone, then mixed with the natural respiratory murmur, which, in its turn, becomes alone audible. The sputa return to their catarrhal character. In the meanwhile the dyspnoea and fever diminish, and then cease entirely.

It would doubtless be very interesting to determine, in a given case, whether the lung of our patient was in the second or the third stage of inflammation. But there are no certain means for making this distinction. We may *guess* that the third stage is established if the face become exceedingly pale and corpse-like; we may be more confident of it if the prune-juice expectoration, or if puriform expectoration should occur; and our presumption will be strengthened if the disease have existed for a certain *time*. However, this last circumstance will not help us *much*; for sometimes the lung has been found to be in a state of suppuration on the fifth day of the disease, and sometimes it has been found still in a state of red hepatization after fifteen or twenty days.

Whether, when the lung has reached this third stage, it is still susceptible of repair, is a question which no one can answer. We have not the materials for its solution, inasmuch as we have no sure sign of the existence of this third stage during life. I should *think* that recovery from diffused suppuration of the lung is not possible. The rarer form of circumscribed abscess certainly is not of necessity fatal.

The *duration* of pneumonia may be laid, upon an average, at ten days, or a fortnight. In a table collected by Andral for another purpose, viz., to determine whether there were any fixed *critical* days in respect to the termination of the disease (a question which I shall not now discuss), the duration, in 112 cases, varied from four days to six weeks. But one only was thus protracted; 23 cases lasted each seven days; and only 15 of the 112 instances continued longer than a fortnight.

I have very little to add to what I have stated already of the morbid anatomy of pneumonia. Of the changes which the *lung itself* undergoes you are now I hope fully apprized. The pleurisy, which often attends the disease, is seldom accompanied by much effusion; indeed, when the whole of one lung is solidified by inflammation, it fills the cavity of the pleura, and *prevents* much effusion. The heart is found to be in that condition which I formerly described to you, as being both a consequence and an index of death by apnoea. Its right cavities especially are distended by black coagulated blood; and a remarkable degree of venous congestion is frequently met with in the liver, and spleen, and intestines. The amount of this varies according as the process of dissolution—what the French call the *agonie*—has been more or less protracted, and the breathing more or less difficult.

Neither need I enter upon any formal discussion of the *causes* of pneumonia. Sometimes *no* cause can be traced; often the disease is clearly the consequence of exposure to cold: especially under those circumstances which were formerly described as aiding the injurious operation of cold upon the human body. Why, in one person, such exposure causes peritonitis, in another pleurisy, and in a third inflammation of the substance of the lungs, we can give no satisfactory account.

It remains, then, only that I should speak, first of the *prognosis*, and secondly of the *treatment*, of pneumonia; and of the first of these matters, of the prognosis, I have already, incidentally, told you nearly all that is made out, or worth knowing. It is almost superfluous to say that the first degree of the disease is less dangerous than the second, and the second than the third. There is no doubt that pulmonary inflammation may still undergo resolution, although a great part of one lung should be hepatized; but there are no facts which prove—indeed there is no possibility of proving—that the lung may recover from the state of purulent infiltration—the third degree.

Something will depend upon the *extent* of the inflammation; I mean that pneumonia in the first degree and of great extent, is generally as serious as pneumonia in the second degree but much more circumscribed. Inflammation of the upper lobes is also more perilous than inflammation, to the same extent and degree, of the lower.

Of the *general* symptoms, those which we learn independently of auscultation, the *respiration*, as a prognostic sign, is the most important. Laboured breathing, and shallow and frequent breathing, are both of them symptoms of bad omen. We get less help from the state of the *pulse*. If, however, a feeble pulse go along with great difficulty of breathing, and if it do not develop itself under venæsection, we must conclude that the case is a serious one, and deduce an unfavourable prognosis. The supervention of *delirium* is also a discouraging circumstance. You will have inferred already the information which may be gleaned from the character of the expectoration, in respect to the probable issue of the disease. Great viscosity of the sputa, and a deep rusty colour, announce intensity of inflammation: their return to the catarrhal condition indicates that resolution is going on. Watery and brownish sputa, more or less like plum-juice, should induce us to suspect suppuration of the lung, and are therefore of evil augury.

To dictate the treatment of pneumonia is not an easy task. It may sound like a paradox, but concerning this disease I believe it to be true, that the very perfection of modern diagnosis has helped to bring uncertainty and vacillation into our practice. Inflammations of the lung, which might escape all other modes of investigation, reveal themselves infallibly to the ear. By the same sense we learn, as surely, that many of these otherwise latent inflammations run their course without any great commotion of the general system, whether they kill, or whether they pass gradually away. These forms of pneumonia neither require, nor would they endure, nor have they had addressed to them, so far as I am aware, the active measures which, prior to the use of auscultation, were enjoined as proper in unmixed inflammations of the lungs. On the contrary, the current has set, and is setting (too strongly I conceive), in the opposite direction. A most distinguished French author, M. Louis, has endeavoured to show that venæsection has not much control over the progress or the issue of pneumonia in any of its forms: and in our own country that doctrine has been adopted by at least one very accomplished physician—adopted and extended, for Dr. Hughes Bennett maintains that antiphlogistic remedies in general, and blood-letting in particular, are unsuitable, and even hurtful, in all acute inflammations. I believe that I might ascribe similar opinions to physicians and surgeons of eminence in this town.

Now, although this, in my humble judgment, is a mistake, although venæsection and tartar emetic are still, in my opinion, the proper remedies for the early stages of those flagrant forms of thoracic inflammation with which our predecessors were dealing when on the basis of experience they put these remedies in force, and recommended them to others, I do not profess myself a partisan of any extreme views in the matter, either theoretically or practically. My own experience teaches me that such flagrant and sthenic forms of pneumonia have become very rare among us. Years have passed by since I have met with any instance of that disease which has required

phlebotomy. I may say much the same of inflammatory diseases in general. They have all, as I firmly believe, been less tolerant of blood-letting since the cholera first swept over this country in 1832. I may be fanciful, but I think that great epidemics, such as those of cholera and of influenza, leave traces of their operation upon the health and vitality of a community, long after they have ceased to prevail as epidemics. But while, upon these great questions, the mind of the profession remains unsettled and even divided, I would have you be cautious observers, rather than eager disciples. More than once or twice I have admonished you that, in prescribing, you must not be guided by the mere *name* of a disease: in this particular instance you must not be guided even by the *thing*, *pneumonia itself*, as disclosed by the evidence of auscultation. The constitutional symptoms must direct the treatment, while the local symptoms identify the disease. If, with the physical signs of pulmonary inflammation, in a patient who was previously strong and healthy, there be conjoined high fever, a hot and dry skin, a hard, firm pulse, pain of the chest, and restricted breathing—and especially if you meet with such symptoms in their early course, accompanying the first stage of the disorder, the stage of engorgement, before the spongy texture of the lung has been obliterated—then you may bleed your patient, not only, as I believe, without harming him, but to his great benefit and safety. And the bleeding should be carried to that point at which some sensible impression is made upon the symptoms; until the *pulse* becomes *softer*; or if it were contracted, until it becomes *fuller*; until the sensation of painful *constriction* is abated, and the *dyspnœa* relieved; or until syncope appears to be at hand. Whether the venæsection should be repeated, must be determined by the subsequent progress and aspect of the case. As an auxiliary to the lancet, and in less severe cases as a substitute for it, I should advise abstraction of blood from the surface of the chest by means of cupping-glasses, or of leeches. I scarcely need say that the antiphlogistic regimen should be at the same time enforced; that the patient must keep his bed; and that all superfluous exertion of his lungs in speaking must be forbidden.

When the inflammation has advanced into the second stage, we cannot expect that the removal of blood will have any direct influence upon the inflamed and solid parts; but even then, if duly moderated, and under the guidance of the constitutional symptoms, it may be serviceable, by diminishing the force of the heart and arteries, and so tending to prevent the extension of the inflammatory process; by lessening the whole quantity of blood circulating through those portions of the lung which are still pervious, and thus relieving dyspnœa; and by putting the system at large into the condition most favourable for the re-absorption of the lymph by which the air-tubes and vesicles of the affected parts have been blocked up.

But a time arrives when bleeding is no longer of use, or when it is positively hurtful: when it ceases to have any good influence on the local disease, and has an injurious influence on the whole system; reducing the patient's strength, and incapacitating him for bringing up, and ridding his lungs of, the tenacious mucus exhaled by the bronchial membrane. This is what takes place in those cases in which the expectoration is said to be *stopped* by a bleeding. We want some remedy, therefore, to aid the blood-letting, or to employ alone when the abstraction of blood is no longer expedient, or has been inexpedient from the first: and we have two such, in *tartarized antimony*, and in *mercury*. The tartar emetic plan I believe to be the best adapted to the first degree of the inflammation—to that of engorgement; and the mercurial plan to the second—to that of hepatization.

I need not tell you that the tartarized antimony is not given in this disorder with the object of producing vomiting. It is a very curious thing that although, when administered in a considerable dose, its first effect is usually sickness, followed perhaps by purging, a repetition of the same dose is, in the majority of cases, at length borne without any further vomiting. The stomach comes to *tolerate* the medicine, as our continental brethren say; and then its beneficial influence upon the disease is no less marked than when nausea and retching take place. Some patients do not vomit at all; others, the majority in fact, vomit two or three times, and then *tolerance* is established. If the sickness and purging go on, they may be checked by adding a few drops of laudanum to each dose. Dr. Thomas Davis, who had tried this remedy largely, and, as he tells us, with great success, gives the following as his own plan of administering it; and perhaps it is as good as any. After the requisite bleeding, he

begins with one-third of a grain of tartar emetic in half a wine-glassful of water, with a few drops of laudanum or syrup of poppies. Two doses of this strength he gives at the interval of one hour from each other. He then, if the patient do not vomit, omits the opium, but continues it if he do, doubling, however, the quantity of the tartar emetic, giving two-thirds of a grain for two successive hours; and in this way he goes on, adding every two hours a third of a grain, until he reaches two grains hourly. This last quantity he has not exceeded, and he says that he has continued it for many days without producing any injurious consequences.

Under this plan of treatment the symptoms will often undergo a marked change for the better, in three or four hours. Sometimes, however, the relief is not conspicuous for twenty-four or even for thirty-six hours. He states, and this is accordant with my own experience of the remedy, that the tartar emetic always acts best when it produces no effect except upon the inflammation itself; *i. e.*, when it does not cause vomiting, or purging, or a general depression of the powers of the system. This is an important practical remark, because many persons have supposed that it subdues the disease only when it previously gives rise to these symptoms. I consider this testimony of Dr. Davis's, to the power of the tartarized antimony in controlling inflammation of the lungs, the more valuable, because he informs us, that before he had occasion to see its admirable effects in the first stage of pneumonia, he had been in the habit of trusting to the free use of mercury, after due depletion.

When the dyspnœa has been appeased by antimony thus exhibited, the medicine may be intermitted; and if the inflammation show any disposition to rekindle, it must be again be repressed by a repetition of the tartar emetic.

When, however, the inflammation has reached the second stage, that of solidification, mercury is more worthy of confidence, in my opinion, than tartarized antimony. And I have little or nothing to add to what I formerly said in respect of the mode in which it ought to be administered. The object of giving it is to make the gums tender; and it is expedient to do this as speedily as may be. Small doses of calomel repeated at short intervals—a grain every hour, or two grains every two hours, or three grains every three hours—combined with so much of laudanum or of opium as may be requisite to prevent it from running off by the bowels—offer the most certain way of accomplishing our object. If the bowels prove irritable under the calomel—blue pill, or the hydrargyrum cum cretâ, may be substituted for it with advantage: and if the internal use of mercury be anyhow contra-indicated, or if it appear slow in producing its specific effect, the linimentum hydrargyri may be rubbed in, or the strong mercurial ointment.

Many persons, I am persuaded, are saved by treatment of this kind, pushed to slight pytalism: the effusion of lymph, tending to spoil the texture of the lung, is arrested; and the lymph already effused begins to be again absorbed: and the ease and comfort of the patient, as well as the alteration for the better of the physical signs, attest the healing qualities of the remedy.

After the inflamed lung has become solid and impermeable, the treatment must still be regulated rather by the state of the system at large, than by the actual or presumed condition of the lung: we still look for guidance more to the general symptoms, than to the physical signs. If the pulse continue steady and firm, wait patiently the effect of the mercury. But when sunken features, a pallid face, coldness of the surface or extremities, a tendency to delirium, and (above all) a feeble or irregular pulse, proclaim that the vital powers are giving way, it will be requisite, as in other cases where death is threatened by asthenia, to administer cordial and stimulant medicines; the carbonate of ammonia in a decoction of seneka; wine: and to feed the patient well on milk, or beef-tea.

Among what may be called the routine remedies of pneumonia, we must rank counter-irritation by means of blisters. And I believe that they are often applied to the chest much too early in such cases. In the outset, while there is yet considerable fever present, they add to the irritation, and distress the patient; and probably tend to aggravate the existing inflammation. But when the fever is no longer high, and the skin no longer burning, though the expectoration is still difficult, the dyspnœa considerable, and a sensation of pain, or tightness, or oppression, is experienced in the chest, then a large blister is often productive of very sensible benefit; but it should be a large one. The patient should have a waistcoat almost, or at any rate a

breast-plate, of blistering plaster. I have never seen such good effects from placing blisters upon distant parts in this disease, upon the thighs or arms for instance, as would lead me to plague the patient with them in those situations.

Purgatives are of less certain value in pneumonia than in many other inflammatory diseases: and less, especially, than in cerebral inflammation. Still it will always be right to give an active aperient at the outset; and afterwards to take care that the bowels be unloaded at least once every day. A continued drain by purgation would not consist at all with the mercurial plan, which promises to be most useful when the inflammation has already reached the stage of hepatization.

This, then, is an outline of the treatment which is most likely to save the life of those who are affected with acute idiopathic pneumonia. Different cases will require different modifications of it; for which, I repeat, no particular rules can be laid down.

All that I have hitherto been saying relates to *acute* pneumonia, occurring in a previously healthy person. But pneumonia, having that character, and so occurring, is (I repeat) a much less common disorder than most persons appear to suppose, or than I formerly thought it to be. I have been surprised to find how few cases of pure idiopathic inflammation of the lungs present themselves among my hospital patients. Five or six in the year are as many as I see there. Intercurrent pneumonia, however,—pneumonia engrafted upon some other pre-existing disease,—is abundantly frequent; and requires, in general, a much less vigorous, and more wary plan of treatment. Inflammation of the pulmonary substance is apt to supervene insidiously upon various disorders which are of every-day occurrence: upon bronchitis; upon phthisis; upon disease of the heart; and upon fevers, especially the exanthematous fevers. In these cases, while the physical signs are necessarily the same as in the unmixed acute disease, the general symptoms are often but slightly pronounced. During the progress of continued fever of a low type, inflammation may steal upon the lung, and run quickly through all its stages, and spoil the organ irrecoverably, without giving any notice of its presence: unless, indeed, you suspect it, and search for it with your ear. The pneumonia is said, in such cases, to be *latent*. It seldom needs, the associated disorder would seldom bear, any active depletion. Much benefit often follows the abstraction of small quantities of blood, but they should be taken from the surface of the chest by the cupping-glass, and not by the lancet from the arm; and it is often good practice thus to aim at reducing the local mischief with one hand, while with the other we support the patient's strength by means of ammonia, wine, and nourishing broths. Blisters are also of service: more so than in the sthenic forms of pure pneumonia; and they may be applied at an earlier period. In conjunction with these remedies I should advise the cautious employment of mercury.

When the convalescence from acute pneumonia is decided, and real, it is shorter than might have been supposed. From the period when the pulmonary inflammation is fairly over, the strength returns with unexpected facility, even though large bleedings have been practised and repeated. But we have to guard, more perhaps in this disease than in most others, against false or merely apparent convalescences. A patient can never be pronounced perfectly secure so long as any trace of crepitation remains in the affected lung, and this may often continue long: nay, it not unfrequently ceases only upon the supervention of another more surely fatal, though less rapid a disorder—viz., tubercular consumption; of which, however, I must treat as a distinct disease.

[PNEUMONIA IN CHILDREN.—Pneumonia is a very frequent disease of childhood; and as it is then marked by some very important modifications in its general characters and results, it demands a separate consideration.

The inflammation may be confined to the minute lobules of the lung, or it may attack the pulmonary lobes. The first constituting lobular, and the second lobar pneumonia. In both forms of the disease the inflammation may be confined to a small portion of one lung, or extend to nearly the whole of one or both.

In very young children the symptoms of pneumonia, particularly at the commencement of the attack, are very obscure, and even when more distinctly marked, they differ but little from those of bronchitis.

In infants the attack is very generally preceded by symptoms of a mild bronchitis, to which there usually succeeds a chill, differing in its severity and duration in different

cases; this is followed by increased heat and dryness of the surface, increased frequency of pulse, accelerated respiration, dyspnoea, and a short dry cough. These symptoms quickly augment in intensity—the lips become of a bright red, the tongue of a florid hue, and somewhat dry, and coated along its centre with a thick white fur. Often there is from the commencement of the attack vomiting, and, in young children, diarrhoea. In many cases there is considerable agitation and anxiety, in others decided drowsiness, and more rarely convulsions; these are sometimes violent and repeated, and often followed by an entire loss of consciousness. Convulsions, according to the observations of Rilliet and Barthez, are confined to cases occurring in young infants, where the inflammation is seated at the summit of the lungs.

In infants at the breast, in the early period of the attack, the breathing is no longer affected solely through the nose, but the little patient lies with the mouth partly open, and drawing in the air through it. The tongue becomes in consequence preternaturally dry, and the child sucks by starts—seizing the nipple with eagerness, sucking for a few moments with greediness, and then suddenly dropping the nipple, and in most instances commencing to cry or moan.

The inspiration in children affected with pneumonia is marked by a peculiar abruptness, occurring suddenly before expiration is completed, and with a more or less dilatation of the nostrils.

The frequency of the respiration is not always in proportion to the violence of the attack, and does not always increase with the progress of the disease. When pneumonia occurs in the course of chronic enteritis, there is often either a very slight or no acceleration of respiration; it may, also, be masked by an accompanying affection of the abdominal organs or of the brain. In cases of broncho-pneumonia, the dyspnoea is often so intense as to threaten suffocation. The dyspnoea is always proportionate to the violence and extent of the pulmonary inflammation, and augments with the progress of the disease. It is often attended with great anxiety, and a sense of impending suffocation which renders a recumbent posture insupportable.

The cough is at first frequent, short, dry, and painful, but soon becomes moist; when expectoration occurs, which is seldom the case in young children, it is, at first, slight, and consists of a whitish, viscid mucus, which becomes subsequently reddish. It is rarely, however, rust coloured. As the cough becomes more moist, it gradually declines in violence until towards the eighth or ninth day, when it rapidly diminishes, and finally disappears. When, however, the pneumonia becomes more diffused, or occurs in the course of some acute affection, and in children not much debilitated, the cough is very frequent and intense, and is rarely attended with a free expectoration. The attacks of pneumonia that so frequently occur in the course of chronic enteritis, are seldom attended with much cough—occasionally, indeed, it is entirely absent. There is seldom any very decided pain of the chest—when present, it is generally acute, though occasionally dull, and is usually experienced at the anterior margin of the axilla—it is augmented by the cough, and often disappears long before the *other* symptoms.

The disease in these cases is so often destitute of leading symptoms, that its existence might be overlooked, were it not for the physical signs revealed by auscultation—the skin is pale and cool, the pulse small, and the face and extremities œdematous.

In young children, or those under six years of age, pneumonia is frequently preceded by extensive bronchial inflammation. The inflammation of the lungs, which is probably produced by the extension of the inflammation from the extreme branches of the bronchi to the preliminary tissue, often comes on so gradually, that it is impossible to fix, in most cases, the exact date of its occurrence. Occasionally, at the period the pneumonia supervenes, there is a well-marked accession of fever and dyspnoea, and an aggravation of all the symptoms. The febrile symptoms are less than in the other forms of the disease, but the dyspnoea and distress are usually greater, and the face presents from the first a more livid hue. The cough is less hard, but often occurs in paroxysms which greatly distress the patient; the respiration is more hurried and irregular; the irregularity coming on at an earlier period. Head symptoms are more frequent, the patient's rest is disturbed, and he often mutters in his sleep, and has far more restlessness and jactitation when awake. Convulsions and coma more frequently precede death, which occurs at an earlier period than in the other forms of pneumonia.

Pneumonia in children is generally attended with loss of appetite and increased thirst, and a torpid state of the bowels; when the disease, however, is complicated with enteritis, profuse diarrhœa may be present throughout the attack. In these cases furunculi or ecchymoses occasionally cover the skin, while the blistered surfaces are liable to become ulcerated.

In favourable cases of primitive bronchitis, the acceleration of pulse and respiration attain their greatest intensity generally by the end of the fourth, fifth, or seventh day, or, at the farthest, by the ninth day: when the heat of the skin and the frequency of the pulse diminish, respiration becomes slower, and the inspiratory movement is unattended with dilatation of the nostrils. The cough becomes more free and moist, the fever quickly disappears, the face becomes paler, and the expression of the countenance more natural. The respiration finally assumes its normal rhythm, the cough rapidly diminishes, and in a few days convalescence is fully established.

In the more severe and unfavourable cases, there is intense dyspnœa from the very commencement of the attack—intense anxiety, a pallid hue of the face, with a violet tinge of the lips and summits of the cheeks, an extremely small, rapid pulse, and a frequent, short, dry, painful cough. The general symptoms continue to augment in violence, and death may occur on the third or fourth day. In other cases, the symptoms, from the beginning of the attack, are less intense, and do not increase in severity after the fifth or sixth day, but remain, as it were, perfectly stationary. Soon, however, in addition to the cough and dyspnœa, there occur pallor of the face, great emaciation, and constant diarrhœa; and, at the termination of from three to four weeks, the patient, being reduced to a state of the utmost debility, expires. In other cases, again, after all the prominent symptoms have declined, and a speedy convalescence is anticipated, from some accidental cause, the inflammatory action is re-excited in the lungs, the dyspnœa, cough, acceleration of the pulse, and fever, return with increased violence, and death speedily ensues. The fatal termination may also be hastened by the occurrence of pleurisy, measles, gangrene of the mouth, &c.

Acute pneumonia, occurring in the course of some other acute disease, is generally of rapid progress, and terminates either in health or fatally, within a few days.

The physical signs of pneumonia, occurring in children, are of primary importance. By them alone, in many cases, can a correct diagnosis be established—while it is only by them, also, that the extent and progress of the local disease can be determined.

In simple primitive pneumonia of children, if of the lobar form, there are present, from the very commencement of the attack, crepitant ronchi, and often bronchial respiration on one side the thorax towards its base. The crepitus is generally larger than in adults. If for a short time it cannot be heard, it may be generally reproduced by causing the child to cough, when it is again to be distinguished, in the strong inspiration which succeeds the cough, the sound resembling the cracking of a whip.

In the lobular form of pneumonia, the subcrepitant ronchi are more generally diffused, while the bronchial respiration is seldom heard. The extent of the latter increases with the progress of the inflammation, while the ronchi decrease. When the acceleration of the respiration and pulse has attained its maximum, the bronchial breathing is very audible, and is accompanied with bronchophony, or with a resonance of the voice and extensive dullness of the chest. As the general symptoms decline, and the cough becomes more moist, the subcrepitant ronchi are very abundant, and the resonance of the voice extends over a greater space; the bronchial breathing continues, but the dullness of the chest diminishes. Even after convalescence is established, a slight prolongation, as well as a different resonance of the voice, is still perceptible. The respiration continues feeble for several days, and only slowly recovers its proper vesicular tones.

In more violent cases, humid ronchi are heard throughout the chest, intermixed with others of a drier character, or with bronchial expiration and a diffused resonance of the voice. Bronchial breathing is always a very grave sign. In eleven out of twenty cases in which it was detected by Dr. West of London, the disease had a fatal termination.

The mucous rhonchus is heard in most cases of broncho-pneumonia in children, and in cases of lobar pneumonia, in the neighbourhood of the subcrepitant rhonchus, and occasionally where distinct bronchial respiration exists. It often persists for a long time after every other sign of disease has disappeared.

In the lobular pneumonia of young children, the physical signs are at first limited to the mucous and subcrepitant ronchi, dependent upon the increased secretion of mucus in the bronchi. The subcrepitant ronchi are frequently continued throughout the attack, being often the only stethoscopic sign present. They are rarely replaced by the fine crepitant ronchi, as in the pneumonia of adults. Bronchial respiration becomes developed when induration has extended to a considerable portion of the texture of the lungs. It is chiefly heard at the upper and middle portions of their posterior parts—it is rarely heard in the lower lobe, from the comparative smallness of the bronchi, and their early obliteration by the progress of the inflammation. The respiratory sounds are very peculiar—the inspiratory murmur, in place of being full and expansive, as in a healthy child, is short, obscure, and almost without the vesicular murmur, and may or may not be accompanied with the mucous or subcrepitant ronchus; the expiration is rarely distinct, unless the bronchial respiration is fully developed, when it is usually louder than the inspiration. The sounds indicative of inflammation of the texture of the lung are often entirely obscured by the mucous ronchi of bronchitis.

The anatomical lesions in the pneumonia of children differ very materially, according as the inflammation is lobular or lobar, and according to the period of the disease when death takes place.

In lobular pneumonia the lung is usually soft and flaccid, of a mottled appearance externally; patches of a deep red or violet colour, generally distinctly circumscribed, of a circular or oval form, hard to the touch, and slightly projecting, being interspersed in the midst of the natural greyish-red tint. They are commonly situated at the inferior edge of the lung, but may occupy other portions of its surface. The part occupied by these patches does not collapse upon the chest being opened.

In some cases this marbled appearance of the lung is absent; but spots of induration, more or less deeply seated, may be detected by the fingers, giving to the lung an uneven or knotted feel.

The same marbled appearance is presented by a section of the lung. The dark patches are more or less accurately circumscribed in the first stage of the disease; they are firmer than the surrounding tissue, swim when thrown into water, crepitate beneath the finger, and, however carefully separated from the sound portion of the lung, yield upon pressure a reddish fluid intimately mixed with air. In the second stage the dark patches form nodules of a firmer and more compact consistence. Their cut surface is smooth, and when carefully detached from the surrounding parts, they crepitate none or but slightly, and sink rapidly when thrown into water. When the central portion alone of the nodule is subjected to pressure, a red sanious fluid escapes without any admixture of air. It is seldom that single lobules are found affected, the induration usually comprising four or five, forming a mass of the size of an almond. If the patient lives for some time, the intervening substance usually becomes affected, the lobular is converted into lobar pneumonia, and the inflammation runs its course, as in the ordinary cases of this latter form of the disease.

In the third stage of the disease, the section of the lung presents a greyish colour, inclining more or less to yellow in different cases. The pulmonary tissue is friable, and when pressed gives discharge to a purulent fluid, its texture being infiltrated with pus. Upon minute examination, some of the lobules will be found more projecting than others, their vesicles not being compressed, as in the less projecting parts.

According to Barthez and Rilliet, there are two forms of lobular pneumonia. The first, which they denominate the *mammelonated*, is marked by nodules of hepatization, comprising one or more lobules, differing but little in colour and appearance from the surrounding tissue. Their limits are well defined, even when the neighbouring tissue is engorged. They are frequently surrounded by a layer, of about half a line in thickness, white, firm, and of a fibrous appearance. The hepatized portions slightly project above the surface of the incised lung, from the collapse of the surrounding air-cells. They vary in size from that of a hemp-seed to that of a pigeon's egg. They are of a spheroidal form, and have usually a regular surface. But a single nodule may exist in one lung, or there may be from twenty to thirty, or more. They are almost invariably surrounded by a circle of pulmonary tissue in a state of engorgement. The diseased portions generally attain the third stage of pneumonic hepatization, which terminates in the formation of an abscess.

In the same lung there may exist nodules of hepatization in the first and second stages, and also rounded masses of a straw colour, very humid when cut into. At a more advanced stage, the pus, which is at first deposited in the interstices of the pulmonary tissue, becomes collected in the centre of the nodules. This minute collection of pus is surrounded by two concentric zones, the internal one yellow—hepatization of the third degree;—the external one red—hepatization of the second degree. At a later period, the size of the abscess is increased at the expense of the inner zone, the outer zone at the same time passing into suppuration. At a still later period, an incision through the diseased portion of the lung reveals cavities varying in size from a few lines to four or five. These cavities are in general round, more rarely oval, and contain usually a thick tenacious yellow or greenish pus, perfectly free from any admixture of air. Sometimes small coagula of blood are mixed with the pus. These cavities are at first formed by a layer of hepatization, lined with a layer of concrete pus, or with a false membrane, yellow, soft, and easily detached. Subsequently, this membrane becomes changed into a thin, smooth, polished tissue, analogous to a serous membrane. The abscesses sometimes communicate freely with each other, and at the point where the bronchi penetrate their cavity, its mucous membrane presents a true solution of continuity. When the inflammation invades separately several neighbouring lobules, the cavity of the abscess is multilocular, each cell being isolated by a lamina of a hepatized tissue, upon the rupture of which the different abscesses communicate with each other. These abscesses have usually a tendency to approach the surface of the lung. An adhesion often takes place between the opposing surfaces of the pleura, or the pleura of the lungs becomes gradually thinned and finally perforated, giving rise to a true pneumo-thorax. It has happened, that an adhesion has formed between the lung and diaphragm, and the abscess of the lung has thus communicated with the peritoneal cavity. These abscesses are often completely isolated, and, with the exception of the thin layer of hepatization in their immediate vicinity, are surrounded with a perfectly healthy pulmonary tissue. Occasionally, however, they pervade the greater portion of the whole of one of the lobes. Their size is ordinarily in inverse proportion to their numbers. In the greater number of cases, they are confined to one lung, and are more frequent in the left than the right. They are met with most frequently in children under six years of age.

Partial, which is the second form of lobular pneumonia, is much less circumscribed than the preceding. Indistinct limits exist between the healthy and diseased portions, and the disease occupies a larger portion of the lungs. The diseased portions may throughout have reached the second stage, or at the centre alone, the circumference being still in the first stage; and this being in contact with several other portions of the lung, in a similarly diseased condition, nearly the whole of an entire lobe may present an admixture of the characteristics of pneumonia in the first and second stages, and when the disease has reached the third degree, it is strictly lobar.

In the major part of the cases, lobular pneumonia is double—the disease, however, being frequently much more extensive on one side than the other. A union of the three forms of pneumonia—mammelonated, partial, and general—is often met with in the same patient. The first is equally frequent in every portion of both lungs, while general pneumonia is far more frequent in the inferior lobes. Partial pneumonia is often disseminated throughout the three lobes—it is more frequent, however, in the inferior. Mammelonated pneumonia is of much more rare occurrence beyond the sixth year than partial pneumonia, while general pneumonia is still more rare than the former between the sixth and fifteenth years. (*See Barthez and Rilliet—Maladies des Enfants, t. i., p. 61*).

There are well sustained reasons for doubting the dependence of all the changes in the texture of the lungs, described as characteristic of lobular pneumonia upon inflammation of the substance of the lungs. Many of them are, unquestionably, produced by a collapsed condition of certain of the air-cells, occurring in the course of an attack of bronchitis. For the investigation of this subject, the reader is referred to the Editor's *Treatise on the Diseases of Children*, 4th edition.

The anatomical characters of lobar pneumonia in children are the same with those met with in the pneumonia of adults.

Inflammation of the bronchi, particularly of the smaller bronchial ramifications, pleurisy, and emphysema of the lungs, are common complications of pneumonia in children.

The lobular form of pneumonia is of the most frequent occurrence during childhood—it is also the most serious at that age.

Lobar pneumonia, occupying only one lung, when it occurs in children between six and fifteen years of age, whose health has not been impaired by previous disease, unless complicated by some secondary affection, is not a very fatal disease, if judiciously treated from its commencement. Its serious character is however greatly enhanced by its being complicated with other diseases. The most common of these are measles, hooping-cough, chronic enteritis, and small-pox.

Pneumonia in children is produced by the same general causes, as give rise to it in adults. It is common to both sexes, but more frequent in boys. It is particularly liable to occur in the same child after a shorter or longer interval.

In the treatment of the pneumonia of children, bleeding will very generally be demanded in the commencement of the attack. In young children leeches or cups should be applied to the anterior parietes of the chest, beneath the clavicles, or between the shoulders. Their number should be proportioned to the violence of the symptoms, and to the age and strength of the patient. In older children, however, bleeding from the arm should be preferred. The amount of blood drawn must be proportioned to the extent of the disease. The propriety of repeating the bleeding will depend upon circumstances. When the first bleeding has been well timed and carried to a sufficient extent, a second will seldom be required; whenever, however, the leading symptoms, particularly the dyspnoea, continue with little abatement, we should never hesitate to repeat the bleeding, either from the arm, or by leeches or cups, according to the violence of the remaining symptoms, the age of the patient, and the amount of strength remaining. It is, however, in the early period of the attack alone, that any very decided advantage is to be expected from blood-letting; in the more advanced stages of the disease, it will seldom be beneficial or admissible; though cases may occasionally occur, when the cautious application of leeches or cups to the chest or between the shoulders will be attended with marked relief, even after the disease has existed for several days.

In the commencement of the attack, if the bowels are torpid, it will be proper to administer a full dose of calomel, to be followed in a few hours, by a dose of castor oil, or in robust children, over three years of age, by a mixture of equal parts of magnesia and epsom salts. Subsequently the bowels should be kept regularly open by enemata, or occasional doses of some mild purgative; a grain of calomel, with half a grain each, of ipecacuanha and extract of hyosciamus, administered twice or thrice a day, will usually effect this object. Costiveness is not, however, very common in the pneumonia of infants; an opposite state of the bowels is a more frequent and troublesome symptom.

Subsequent to blood-letting, in robust children over three years of age, tartarized antimony, given in small doses during the day, will be found highly beneficial. But in younger children antimony in any form is a dangerous remedy, more particularly when there is any tendency to disease of the alimentary canal. Small doses of ipecacuanha and calomel will be found very generally to act beneficially in allaying the cough, dyspnoea, and general restlessness, and may be given to the youngest patient without any inconvenience resulting; adding to each dose a minute portion of powdered digitalis will in many cases be found advantageous. Occasionally the calomel will purge; should this be the case it will be necessary to add to it a grain or two of Dover's powder. After the violence of the disease has been considerably abated, we have found advantage from the administration, every three hours, of small doses of calomel, ipecacuanha and hyosciamus. Under the same circumstances the compound honey of squill may be given; in children over three years of age it is well adapted to allay the cough and dyspnoea: to the dose given in the evening a few drops of paretic may be added.

Blisters to the chest are certainly very important remedies in the pneumonia of children. In violent cases, occurring in patients over five years of age, particularly when attended with much febrile excitement, they should not be applied until the violence of the symptoms have been abated by direct depletion; they should be kept on no longer than is necessary to produce a decided redness of the skin; they should be then removed and the reddened surface covered with a soft emollient poultice. In infants, as well as in slight cases, and in those occurring in children of a delicate

frame, or who are much debilitated, in place of a blister a thick slice of bread, dipped in vinegar and lightly sprinkled with powdered mustard, should be applied to the chest, and removed as soon as it has reddened the skin.

Warm sinapised pediluvia, or a sinapised hip-bath, will often produce a favourable revulsion from the lungs, and may be frequently repeated.

In stubborn cases Dr. West speaks highly of mercurial inunction; under its employment, he has seen recovery to take place even where the circumstances had seemed to warrant an unfavourable prognosis. The full value of mercurial inunction is seen in cases which have been neglected until the time for depletion has gone by, the patient having become exhausted, and the employment of calomel being forbid by the presence of diarrhœa. Dr. West directs one drachm of the mercurial ointment to be rubbed into the thighs and axilla every four hours, for children of four years of age. He has never seen salivation to be induced by it, but has observed the disease gradually to diminish in severity during its employment, and the solid lung to become once more permeable to air.

When in the advanced stage of the disease there is great exhaustion, with symptoms of impending suffocation, the use of carbonate of ammonia has been strongly recommended—but under such circumstances, there can be but little hope of the patient's recovery.

In chronic cases, our chief dependence is upon revulsives, applied either upon the chest, or upon the surface generally, and perhaps upon a judicious mercurial course, particularly by inunction.

In all cases of pneumonia, the exhibition of some mucilaginous drink, in small portions, and at short intervals, will be found to abate very sensibly the cough, and relieve the dryness of the fauces, which, in the early stage of the disease, is often a harassing symptom. A solution of gum acacia, or the mucilage of the elm bark, or pith of sassafras, sweetened, will be the best we can employ.

The diet of the patient, in the earlier period of the more acute cases, should be confined, almost exclusively, to these mucilaginous fluids—after the violence of the disease has been subdued, plain water gruel, arrow-root, or tapioca, may be allowed. When the disease occurs in young infants, they should be taken from the breast—the mother's milk being given to them in moderate quantities by means of a spoon, as well to prevent their stomachs from being overloaded, as to guard against the mischievous effects of the violent exertion of the respiratory organs in sucking.

The patient should be kept in a clean, well ventilated, and moderately warm apartment. It is all-important to guard him from cold and dampness, and from sudden transitions of temperature. He should be placed upon his bed or couch in a half-recumbent position, in order to render the respiration more easy, and to prevent the injurious consequences resulting from the stasis of fluids in the posterior portion of the lungs.

When the disease has arrived at an advanced stage, or involves a considerable extent of the lungs, the patient should be moved with the greatest care and gentleness, lest convulsions be induced.

After convalescence is fully established, gentle exercise in the open air, in mild, dry weather, may be taken; but, for a long period, the utmost care should be observed to guard against exposure to cold and dampness; and while a gradual improvement is made in the diet—if the child be weaned—all rich, stimulating, and indigestible food must be avoided, as well as the slightest excess in the use of those articles that are allowed. See *Condie on Diseases of Children*.—C.]

[TYPHOID PNEUMONIA.—A state of congestion or of inflammation, more or less intense, of the lungs, accompanied by that impairment of the sensorial powers, and morbid condition of the circulation and of the organism generally, which characterize the more grave forms of typhoid fever, has repeatedly prevailed in different portions of the United States, as an epidemic, often of wide extent, and, in its earlier visitations, producing an amount of mortality truly appalling.

The first notice we have of the appearance of this form of disease, remarks Dr. Dickson, (*Essays on Pathology and Therapeutics*, vol. i., 435), “was in the year 1806, in Medfield, a town of the State of Massachusetts, whence it spread gradually—extending itself, winter after winter, throughout New England, into Canada, and

the Middle States, progressing from village to village, and from one portion of the country to another, until, in 1813, it had reached Philadelphia. In the winter of 1815 it first prevailed in South Carolina, and was then, and more widely in 1816, epidemic; since which time its ravages in that State have been slight. It continues to show itself, sporadically, everywhere, I believe, where it has once found footing. In South Carolina we scarcely pass a winter without meeting with instances of it, especially among the blacks."

The disease is of frequent occurrence in other portions of the United States, and may be considered, to a certain extent, as endemic to several localities.

We are informed by Dr. Gibbes, (*American Journal of the Medical Sciences*, Oct., 1842,) that it prevails extensively during the winter months, on the rivers and swampy plantations, in the neighbourhood of Columbia, South Carolina, and destroys more negroes than all the other maladies combined to which they are ordinarily liable.

According to Dr. Dickson, it has not been observed farther south than the State of Georgia, nor is it of very frequent occurrence beyond the latitude of Charleston.

Typhoid pneumonia, as it prevails in the United States, is very similar in character to, and is probably the same affection as that described by Sydenham, Huxham, and others of the older writers, as peripneumonia notha. Sauvages has very accurately portrayed the disease under the denomination of peripneumonia typhoides. An account is given by the late Dr. John Bard, of New York, of an epidemic that prevailed on Long Island, in the winter of 1749, which he terms, a malignant pleurisy, that in all its essential features corresponds exactly with the disease under consideration; a similar affection is also noticed by Dr. Hugh Williamson, as having prevailed in North Carolina in 1792.

Typhoid pneumonia is described by Dr. Stokes as not uncommon in Ireland, while Dr. Burne, of the Westminster Hospital, mentions that a great number of cases of what he calls "the spotted fever," were brought into that institution in the year 1838. He describes the affection as "an adynamic or typhus fever, combined with a latent and dangerous pneumonia, and exhibiting on the surface a very regular and uniform spotted eruption — not petechiæ." This is evidently the same disease as the typhoid pneumonia of this country, which, when it first attracted attention, was so frequently attended with an eruption upon the skin, that it was very generally designated by the popular name of spotted fever. The eruption, however, soon ceased to attract attention; it was seldom observed as a phenomenon of the disease south of the Potomac, and even in the northern and middle portions of the United States, the eruption ceased early to present itself.

Dr. Macintosh, of Edinburgh, describes a form of pneumonic inflammation, attended by symptoms that are generally denominated typhoid, and which, in consequence, has obtained the name of pneumonia typhoides, as very prevalent among the British troops stationed in exposed situations along the coast, and in large garrisons where the duty is severe. Dr. Mackintosh, however, objects to the adjunct typhoides, as expressing erroneous ideas of the pathological condition of the system; — and the objection is not without foundation — for although that peculiar form of pulmonary engorgement quickly followed by inflammation and hepatization, to which the term typhoid pneumonia has been applied, does very frequently occur in connection with genuine typhoid fever, it is likewise a very common complication in certain seasons and localities of the bilious remittent fever, while it is also met with in cases of gastro-enteritis, of ambulant and erratic erysipelas, of diffuse cellular inflammation, and of other diseases, and by which the thoracic affection is more or less modified, and often completely masked; hence to denominate the pneumonic disease as essentially and invariably typhoid in its character is evidently a misnomer, and very liable to lead to serious errors in practice.

In the United States the disease is known by various names, according to the predominance of particular symptoms. In the more violent cases, the patient being suddenly seized with a very severe chill, accompanied with marked coldness of the surface, it is, according to Dr. Gibbes, frequently denominated the *cold plague*. The head being almost invariably affected, before the pneumonic symptoms are developed, it is often called *head pleurisy*. In the autumn or spring, when, in particular districts of country, it is frequently attended by the symptoms of ordinary bilious fever, it is called *bilious pleurisy*. From the symptoms of prostration by which the disease is

so generally attended throughout its whole course, and the rapidity with which the patients sink in the more violent cases, the term *typhoid pneumonia* has become, however, the one by which it is most frequently designated.

In its mode of attack, and the general symptoms by which typhoid pneumonia is ordinarily attended, there is no uniformity. Usually, however, the disease is ushered in by a chill, which is often of great severity and long continuance — the heat of the whole surface being to the touch much below the standard of health. The cold stage is not unfrequently so intense as to destroy the patient before the slightest reaction occurs. Where the disease is violent in its attack, the patient may suddenly become cold and pulseless, lethargic, and often insensible without previous complaint. Dr. Gibbes has known instances in which the patient was found dead, or died within three or four hours after being in a condition of apparent health.

During the cold stage, the respiration is short and oppressed, and a pain on one or other side of the chest is generally complained of; pain of the head is also present in most cases; and not unfrequently the patients experience severe pains in the back, limbs, and other parts of the body — similar, occasionally, to those of rheumatism. The muscular strength is greatly prostrated, and there is a sense of general uneasiness and great restlessness. After a period, longer or shorter in different cases, but usually protracted, reaction ensues, and the heat of the surface is restored — it is seldom, however, increased much above the ordinary standard of health. The heat is often unequally diffused over the surface — portions being decidedly hot, while others are comparatively cool. Occasionally, the skin becomes hot, dry, and harsh, while, at other times, it is relaxed, cool, and clammy. The pulse, when reaction ensues, becomes somewhat fuller, and more quick and frequent, but in very few cases does it acquire any degree of tension, excepting, perhaps, when the disease attacks young subjects and those who retain some degree of vigour. Most commonly the pulse is soft, or it yields to the slightest pressure. During the febrile stage, the pain, oppression of the chest, and difficulty of respiration are increased, and, very generally, a cough comes on within the first twenty-four hours, by which the pain in the thorax is greatly aggravated. When the cough is attended with expectoration, the pain in the side is considerably relieved, and the oppression of respiration diminished. When, however, the cough continues dry, or the expectoration is slight, all the more serious symptoms become aggravated. The matter expectorated is a thick, tenacious mucus, often tinged with blood, but at other times of an ash or dark-brown colour. The respiration, besides being oppressed, is usually hurried and irregular — the patient's spirits are greatly depressed — he often utters deep and heavy sighs, and complains of a sense of weight, or of constriction at the præcordia; he is often affected with nausea, and occasionally with vomiting.

Dr. Mann (*Med. Sketches*, page 308), notices, as a common symptom of the disease, a remarkable pink-coloured suffusion over the whole face, distinct from the usual febrile redness of the cheeks — the face becoming, at the same time, puffed or bloated. This appearance is most conspicuous in individuals of a light complexion; it is present, also, upon the body, but to a less extent. In many cases it is not observed even on the face, or in a very slight degree, and for a short period.

The tongue, in the first period of the disease, is often clean but red, particularly at its edges; in other cases, and perhaps most commonly, it is thickly coated with a yellowish mucus, which, in the progress of the disease, changes to a dark-brown — the tongue becoming, at the same time, dry, hard, and rough — often chapped. Upon the separation of the crust, the tongue presents, not unfrequently, a bright-red colour, which colour occasionally pervades also the fauces.

In many cases, the patient exhibits from the very commencement of the attack, particularly where there is much pain of the head, more or less delirium, and great restlessness.

As the disease advances, the teeth and the whole interior of the mouth become coated with a dark-coloured sordes; the breathing becomes shorter and more oppressed — the strength more prostrated, the pulse smaller and weaker — the patient sinks into a state of low muttering delirium or of coma, more or less complete, and the fatal event takes place as in the ordinary cases of typhoid fever.

The duration of the disease is very variable. Death, as we have seen, may occur within a very few hours from the commencement of the attack. Dr. Mackintosh has

seen the disease, in the strongest subjects, run its course to a fatal termination in from forty-eight to sixty hours; while Dr. Gibbes has known patients to be destroyed by it in from three to twenty hours. In other cases, the disease may continue for several days before a fatal event occurs.

In cases where, under an appropriate treatment early commenced with, the nervous power is quickly aroused, the activity of the circulation is excited and a free action of the peripheral capillaries restored, the attack has been known to be cut short, without the occurrence of any symptoms of pneumonic disease.

When symptoms of pneumonia become developed, these will often yield to a proper treatment, within the first thirty-six or forty-eight hours, though they will occasionally continue for six or seven days, or even longer. The pulse increasing in volume and in firmness, the surface becoming, throughout, warm, soft, and moist, the tongue cleaner and less red, the delirium diminishing, the expectoration becoming more free and copious, the cough less frequent and annoying, the pain of the thorax less intense, the respiration more full and easy, and the occurrence of a quiet sleep, from which the patient awakes refreshed and with greater cheerfulness, are the certain indications of amendment—recovery, however, is in general very slow—the period of convalescence being protracted, while relapses readily occur from slight exposure or the least imprudence of any kind.

Dr. Gibbes states, that he has frequently known patients who were convalescent and able to walk about, complaining, in fact, of nothing but debility, to “yield suddenly to a cold change of weather, and die in a few hours, or linger several days with pneumonic symptoms.” Symptoms that were before mild and favourable, will often become aggravated upon a change in the atmosphere to cold and damp. If a patient has been much enfeebled by a first attack, a second is very likely to prove fatal.

In many cases of pneumonia typhoides, instead of a gradual decline of the symptoms of thoracic disease taking place, these cease suddenly, and a severe pain is immediately experienced in some other and perhaps remote part of the body, as in the region of the liver, in the bowels, or in the head, attended with the other symptoms of inflammation in these parts. According to Dr. Gibbes, when upon the cessation of the thoracic symptoms, a hemorrhage from the bowels ensues, it is generally critical—when, however, inflammation of the brain or peritoneum takes place, the case usually terminates fatally.

The foregoing is the description of pneumonia typhoides as it most frequently presents itself. In the different epidemical visitations of the disease, at different seasons, and in different localities, or even during the same periods and in the same places, it not unfrequently, however, assumes a very different form. Thus, in some cases, the only symptoms which the patient exhibits are extreme prostration and wandering pains of the back, loins, shoulders or legs. In other cases, after a severe and protracted chill, the patient is seized with severe pain of the head and back, and great gastric distress—delirium soon sets in, which quickly gives place to coma, and death ensues within a few hours.

“Occasionally,” remarks Dr. Gibbes, “an acute pain in the back part of the eye, in the ears, or side of the neck, with stiffness of the muscles, is present. In severe cases the tonsils, the submaxillary and sublingual glands are swollen, with acute pain in swallowing; these are usually the worst cases.” In a few instances the symptoms of genuine erysipelas present themselves on some portion of the skin, and invade successively almost every part of it.

In these irregular forms of the disease, the symptoms of the thoracic affection are often very slight, or they may be entirely absent. There is usually, however, some sense of tightness about the chest, some difficulty of respiration, and a slight occasional cough—with or without expectoration; and it is remarked by Dr. Stokes, that the physical signs of pneumonia may frequently be detected in cases unattended with dyspnoea, pain, cough, or expectoration.

A very common form of the disease under consideration—and which at certain seasons, and in particular districts, is even more prevalent than that to which the appellation typhoid is ordinarily applied—is the one usually known as bilious pleurisy or bilious pneumonia.

In this, the attack commences with symptoms differing but little from those which usually usher in an ordinary case of bilious remittent fever—there is, perhaps, in

general, a more severe and protracted chill, and a sense of distress and oppression about the chest, which is not usually observed in the latter disease. Occasionally, the attack is preceded, for a day or two, by a sense of fulness and weight in the right hypochondrium, and, in a few cases, by symptoms of a dysenteric character. Very generally there is severe pain of the back and extremities, and often of the head also. Almost invariably, the attack is accompanied by an acute pain of the forehead—well-marked febrile symptoms, with, usually, decided exacerbations in the morning and remissions towards evening.—During the exacerbations the face is flushed, and as the skin, from the commencement of the disease is more or less tinged with bile, the mixture of red and yellow gives to the countenance a very peculiar, sickly aspect. The eyes are red and watery, the conjunctiva having often a deep yellow hue. In many instances, it is only after the fever has continued for several days that pain in the chest is complained of: this is sometimes very severe and acute, more commonly, however, it is obtuse—of an aching rather than of a lancinating character—there is, at the same time, a sense of weight and oppression of the chest, with more or less difficulty of respiration and cough. The cough is at first dry, and its repeated paroxysms cause an increase of the pain in the thorax and head. The expectorated matter is frothy and of a yellowish colour, often streaked with blood, and of variable consistence. The tongue is, at first, coated on its sides with a whitish mucus, while at the centre it is covered by a dark-yellow or brownish crust—the edges of the tongue are ordinarily of a decided red. As the disease advances, the tongue becomes dark-brown, dry, and hard. The pulse is usually small, frequent, and quick, with a slight degree of tension.

There is always more or less gastric distress, and very commonly vomiting, the matter discharged from the stomach being often bile or a thick ropy mucus mixed with bile. In many cases the epigastrium is hot, and painful upon the slightest pressure. The urine is always of a deep yellow colour from the presence in it of the colouring matter of the bile.

The stage of excitement in bilious pneumonia is ordinarily of short duration, and, unless the disease is arrested by an appropriate treatment, great muscular prostration speedily supervenes, and the same train of symptoms ensue as in the typhoid form of the disease.

In the affection we have described, under whatever form it may present itself, the period of convalescence is always slow, irregular, and for a long while imperfect. "Chronic hepatization, with or without hectic fever, or a lurking congestion, may continue for weeks; and although, under appropriate management, the disease may be ultimately removed, atrophy of the lungs, with or without ulcerative disease, is often established. In certain cases, months may elapse before the respiratory murmur is heard, and, in many instances, it is never re-established. On the other hand, it has been known to cease in a single day, on the supervention of an attack of gastritis or enteritis." (*Dunghison's Practice of Medicine*.)

The principle exciting cause of pneumonia typhoides is unquestionably protracted exposure to a damp and cold atmosphere, while the predisposition to its attacks is promoted by all those causes which tend to reduce the vital energies of the system—as bodily or mental fatigue, intemperance, improper or deficient diet, insufficient clothing, mental distress or anxiety, long watching, previous disease, &c. Nothing, remarks Dr. Mann, predisposes to its attack in so high a degree as an intemperate use of intoxicating drinks. The soldiers, says Dr. Mackintosh, were often seized with the disease who were exposed at night as sentinels—"instead of walking about they frequently stand shivering in their sentry-boxes, the surface continues long chilled, and with a view to fortify themselves, and to produce warmth, they are in the habit of drinking ardent spirits in considerable quantity."

It is a common complaint among the poor and labouring classes in many parts of this country, during the prevalence of long-continued cold and damp weather, such as frequently occurs during the spring and autumn, in the more northern states, as well as during the open winter of the middle states. In the south, according to Dr. Gibbes, it is rarely met with on highland plantations, and if at all, is confined to such negroes as are more or less exposed to work on low or wet ground. On the swamp plantations the disease is endemic.

As we have already stated, the disease occasionally occurs as an epidemic, but

generally, during seasons when the temperature of those districts in which it prevails is marked by sudden and considerable vicissitudes from heat to cold, and by extreme and long-continued moisture.

Typhoid pneumonia more frequently attacks males than females, and adults than children — this is a statement in which nearly all who have written upon the disease concur. The predisposition of adult males to its attacks, is very readily explained by the amount of exposure and fatigue to which they are usually subjected being much greater than that of females and children — and probably, also, from the fact that their habits, generally speaking, are much less temperate. The disease is not, however, confined to any age, nor are females and children entirely exempt from it, particularly when it prevails as an epidemic. It is usually, however, most severe in individuals over fifty years of age, and is very seldom seen in children under ten years. The negro race is particularly liable to its attacks.

In the treatment of pneumonia typhoides much must necessarily be left to the judgment of the practitioner. In the selection of his remedies, more perhaps than in any other disease, must he be guided by the particular character of the symptoms present in each case. It is all important that the true nature of the disease be carefully investigated in its earliest stages — it being then, in its ordinary form, readily controlled by an appropriate treatment, and, even when marked at its onset by symptoms of very considerable severity, it may often be arrested by prompt and judicious management.

In this disease, Dr. Gibbes remarks, and a similar observation is made by others, "if the cases are immediately attended to, it is found quite manageable, as much so as an ordinary catarrh; but when neglected for twelve or twenty-four hours, and the symptoms are at all aggravated, the patients are very apt to die."

We shall often succeed, when the patient is seen during the first period of the attack, or during the cold stage, as it is not improperly termed, in rousing the nervous energy — producing a free and equable circulation, and a due degree of heat and moisture upon the surface, by resorting at once to moderate doses of opium, camphor, calomel, and ipecacuanha, in conjunction with the milder class of diffusible stimulants internally, and the application of heat and rubefacients externally. By this treatment, early and judiciously pursued, and carefully watched so as to prevent a state of over-excitement being produced by it, the engorgement of the lungs may often be prevented or removed, and a speedy restoration of health secured to the patient.

In the more open form of the disease, known as bilious pneumonia, the early administration of an emetic will often be found beneficial. Richter states, that in this form of the disease, emetics will often remove the pain and affection of the chest as by a charm, and Stoll makes the same observation (*Ratio Meden.*, i.). Dr. Eberle states, that in the few cases of bilious pneumonia he had seen, the utility of emetics in its treatment was strikingly evinced. They, in general, bring on an uniform diaphoresis, promote expectoration, and allay the pain in the thorax, often almost immediately. Dr. Dickson also has been much pleased with the effects of the early administration of an emetic, or an emetico-cathartic. A combination of ipecacuanha and calomel may be given, or the sulphate of magnesia dissolved in a strong infusion of seneka or serpentaria, with the addition of twenty grains of ipecacuanha. Dr. Mann, likewise, speaks favourably of the effects of an emetic of ipecacuanha. In the more decidedly inflammatory cases, the emetic was not administered by him until after bleeding and cathartics had been employed; especially where the head or chest was affected with severe pain. When, after the occurrence of expectoration, this had become arrested from any cause, small emetics, he states, were found beneficial, and in many cases had to be frequently repeated. The emetico-cathartic or even the emetic alone, is, however, a remedy of very doubtful propriety in cases attended with early symptoms of prostration and extreme muscular debility.

The propriety of blood-letting in any of the forms of typhoid pneumonia, has excited not a little controversy. By some practitioners, the use of the lancet is condemned in every case, as a remedy fraught with danger, from its liability to induce speedily a state of prostration from which the patient is with difficulty roused; while others of equal authority insist upon the necessity of full and even repeated venesection — considering it to be an important, if not an indispensable remedy in the treat-

ment of the form of pneumonia under consideration. This discrepancy of opinion is satisfactorily accounted for by the very different characters under which the disease presents itself in different localities, and at different periods—often, indeed, during the same epidemic visitation. South of the Potomac, we are informed by Professor Potter, that the disease is seldom so inflammatory as along the shores of New Jersey, Delaware, and Maryland; and the farther south, the more it is said to assume a decided typhoid complexion. In the winter epidemic of 1815–16, along the northern frontier of the United States, we are assured by Dr. Mann, that in many districts “the disease was highly inflammatory from its commencement to its final resolution,” and that, “it was as idle to administer stimulants for its cure, as it would have been to have poured oil on fire to extinguish the flame.” “Its cure,” he adds, “depended upon the assiduous administration of the antiphlogistic regimen: evacuates, expectorants, and diaphoretics, with the aid of blisters.”

It is very certain that in the ordinary cases of typhoid pneumonia, the use of the lancet will seldom be demanded, and in many, would be altogether inadmissible. Cases, it is true, will not unfrequently occur, in which the detraction of a moderate quantity of blood from the arm, during the early period of the febrile stage, will be productive of the best effects. General blood-letting, however, should always be resorted to with the utmost caution—perhaps it would be better, in every instance where symptoms of a typhoid character early set in, and the loss of blood is considered advisable, to apply cups to the chest in preference to resorting to the lancet. In cases of bilious pneumonia, blood-letting will be much more frequently demanded than in the typhoid form of the disease; but even in these it should be resorted to only in the early period of the attack, and it is more safe to take away a sufficient amount at the first bleeding, than to trust to small and repeated bleedings. In this form of the disease, when much pain or tenderness of the epigastrium is present, leeches or cups to this part are always proper, and will often be found to afford very decided relief.

The bowels should be early evacuated by some mild but effective laxative—the best is unquestionably calomel in moderate doses, followed by castor oil or magnesia. Active or watery purging should always be cautiously avoided.

In the cases marked by early prostration, and extreme muscular debility, we should resort at once to moderately stimulating diaphoretics; of these, according to Dr. Dickson, camphor, nitrous ether, the carbonate and acetate of ammonia are the best; and they are rendered more beneficial by combining them with Dover's powder. “The efficacy of all these,” he adds, “will be much aided by the pediluvium, and by the application of warm fomentations, poultices, and heat in various forms to the surface of the patient. Cataplasms with mustard must be laid over the chest, if pain or dyspnoea is present, and applied also to the wrists and insteps. Epispastics are also of use, by their double power as stimulants and revulsives; the back of the neck, if the head be affected, the sternum and the epigastric region, if the lungs or heart or stomach suffer, should be selected for their application. There has been some dispute as to the preference due to the dry or moist form of heat, as the best to be resorted to in this disease. It is, I think, easy to decide. If the skin be dry, I prefer fomentations, or even the vapour-bath; if moist, I prefer bottles of hot water, heated bricks, bags of hot salt, chaff, &c. Sweating will, in the greater number of cases, come on readily, but must be regulated. If too profuse, and kept up for too long a time, it may prove injurious and debilitating. You restrain it by substituting dry for moist applications, and removing some of the bed-clothes, and changing cautiously the body linen of the patient; dry garments carefully and well aired, being put on in the place of those moistened by the cutaneous discharge.”

In cases where a tendency to sinking is early evinced, Dr. Gibbes recommends small doses of calomel as a general excitant of the secretions, with camphor, opium, carbonate of ammonia, and free vesication.

In nearly every form of the disease, whether typhoid, bilious, or more openly inflammatory, the effects of small doses of calomel, combined with ipecacuanha and opium are spoken of as particularly beneficial. Large blisters over the chest will also, in most cases, be found of the highest importance, from the very commencement of the attack. In obstinate cases, Dr. Mann directs them to be renewed daily until the pain of the chest is removed.

Where the patient's strength rapidly fails, and he is sinking into that low typhoid condition which marks the second period of the disease, stimulants must be freely resorted to: of these Dr. Dickson prefers "the volatile alkali in large doses, from five to ten grains every half hour or hour, with wine whey or brandy toddy; spts. nit. dule., spts. of turpentine, the tincture of cantharides." "I can set," he remarks, "no limit to the administration of this class of remedies, but the excitement of a notable degree of reaction, which being observed, will guide you in the future quantities to be exhibited; taking care, however, that the patient shall not suffer by their timid or inefficient amount, nor be allowed to retrograde by any sudden subtraction of dose."

"It is well to be reminded," observes the same writer, "that in this strange disease it is never permitted to despair of your patient, recoveries being in considerable number recorded, from circumstances the most deplorable, and, indeed, to all reasonable anticipations, absolutely hopeless."

During the whole period of convalescence the patient will require to be watched with care. — "You must abstract gradually from the amount of stimulants which have been given him, and substitute in their stead the more permanent tonics." Of these each practitioner has his favourite. Arsenic is preferred by many of the New England physicians, and is, according to Dr. Dickson, without doubt, highly serviceable. The muriate of iron is also spoken of as well adapted to this period of the disease — and is certainly preferable in all respects to the arsenic. Dr. Mann states, that the only stimulant employed by him in the convalescent state, was a mixture of spts. nit. dule., and aqua ammoniæ; a teaspoonful of this proved a cordial and expectorant, at the termination of the disease, when repeated every two or three hours; as did, also, a mixture of equal parts of the camphorated tincture of opium and antimonial wine, where the cough was troublesome. Other practitioners prefer, as a tonic, at the close of the disease, and during the period of convalescence, the cinchona or the sulphate or muriate of quinia. Dr. Dickson gives the bark in infusion, combining it with serpentaria, adding to each dose a small proportion of carbonate of potass, and camphorated tincture of opium. The patient should be supplied with a light diet of nourishing and easily digested food — and should be guarded sedulously from the slightest amount of exposure to cold or damp, and should be guarded from sudden alternations of temperature for a considerable time after recovery—he should to that end wear flannel next his skin, and adapt his clothing not only to the season, but to the temperature of each day and each portion of the day. — C.]

In the next lecture I shall speak of pleurisy.

LECTURE LII:

Pleurisy. Its anatomical characters; false membranes; liquid effusion; effects of these upon the shape and contents of the chest, and upon its healthy sounds. Symptoms of Pleurisy.

I PROCEED this afternoon to the subject of *pleurisy*; having in the last lecture concluded what I had to say on that of pneumonia: that is, I pass from inflammation of the *substance* of the lung, to inflammation of its *investing membrane*. The two frequently exist together: but when that is the case, the one, in general, predominates greatly over the other. Pleurisy, however, without pneumonia, is much more common than pneumonia without pleurisy. When both are present, but the pneumonia forms the main disease, the term *pleuro-pneumonia* is applied to the compound malady. The whole interest of such a case merges in the pneumonic inflammation. Again, when both are present, but the pleurisy predominates, the compound affection is sometimes called *pneumo-pleuritis*.

The pleura, as you know, is one of the serous membranes. Its inflammation is attended therefore with those *events* which I formerly took some pains to describe as belonging especially to that particular tissue. The inflammation is of the adhesive kind: it is accompanied by pain; by the pouring out of serum, of coagulable lymph, of pus, or of blood. I think it will be best, in this instance also, to lay before you some account of the morbid anatomy of the disease, before I consider its symptoms.

The alterations that take place in the inflamed membrane itself are not very striking or important. Experiments upon living animals, made by introducing some foreign substance, or injecting some slightly irritating liquid, into the cavity of the pleura, have proved that, as in other cases, inflammation is attended with *redness* of the part affected. But it is scarcely ever that we observe this effect *alone* of inflammation, in the pleura of a dead person; unless, indeed, he has died of some other complaint while he happened to have *incipient* pleurisy. The pleura has been said to be thickened by inflammation; but that I apprehend to be a mistake. It often *appears* to be thickened, in consequence of the superposition of a false membrane — a layer, or several layers, of plastic lymph. But actual thickening of the pleura itself seldom or never happens. Neither does the pleura easily soften, or readily ulcerate, under inflammation. It peels off, in some cases, from the lung, or from the ribs, with more facility than in the sound state.

The most remarkable effects of pleurisy result from the effusion of coagulable lymph, or of serous liquid, or of both, into a shut sac, having peculiar anatomical relations. One part of the membrane lines the firm walls of the chest: the other part envelopes the soft and compressible lung. The opposed surfaces of this closed and empty bag being *apposed* also, but freely moveable one upon the other, very different, and even contrary, effects may be produced by its inflammation. The pulmonary pleura may be glued to the costal pleura, so as to prevent all lateral movement between them, and to obliterate the pleural cavity: or the two surfaces of the membrane which are naturally in contact, may be forced unnaturally apart by a pouring forth of serum between them; or the opposite surfaces of the pleuræ may be united by coagulable lymph in some places, and separated by effused fluid in others. And great differences will arise in the symptoms, and in the gravity and tendency of the complaint, according as one or another of these different conditions of the contents of the thorax is established.

Let us first consider the effect of the throwing out of coagulable lymph only; or, of what comes to the same thing, the effusion of coagulable lymph with a certain quantity of serum, which last is soon reabsorbed.

One consequence of this is the formation of *false membranes*. These, indeed, are formed whether there be much or little serum poured out. We continually meet with them, sometimes when we least expect to do so, in the dead body. They vary

Fig. 55.

A

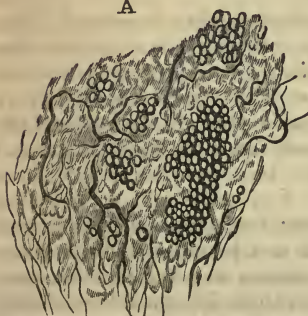


Fig. 56.

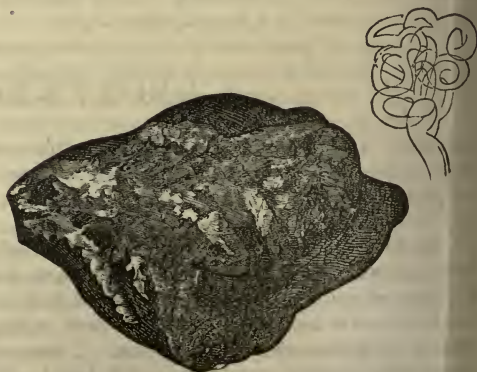


Fig. 55. — The lymph of pleuritis, with new vessels already formed in it; a deposit of fat has also taken place. Magn. 20 di.

Fig. 56. — Straw-colored lymph, coating the lower lobe of an inflamed lung, in recent pleurisy, before there was a trace of adhesion to the costal pleura. The outline represents one of two coils of new vessels, seen under the microscope in the fringe of lymph at the lower end.

greatly, in different cases, in respect of their thickness, situation, extent, organization, and effects.

When the lymph is first deposited upon the free surface of the inflamed pleura, it is soft, and of a greyish white colour, like paste somewhat. It soon, however, acquires an increase of consistence, and shows marks of vitality; becomes, in short, organized. Red points begin to appear in it, few in number and widely separated at first; but they presently multiply, and lengthen into reddish streaks, which run along the surface of the effused matter. Soon these red streaks may be perceived to be slender vascular canals; and at length they inosculate with the vessels of the pleura, and the lymph, converted into a false membrane, becomes a constituent part of the living frame.

It is curious, and useful too, to know how rapidly this work of organization may go on.

Andral made experiments upon the pleuræ of rabbits, by injecting acetic acid into them. He sometimes found, at the end of nineteen hours, soft and thin false membranes, traversed by numerous anastomosing red lines. In other rabbits, placed under circumstances which appeared to be exactly similar, no such result had taken place at the end of a much longer period; but the pleura contained only a serous or puriform liquid, mixed with unorganized flakes of lymph. Now similar differences have been remarked in the human subject, under disease. False membranes, already vascular, have been found in the bodies of persons who died of pleurisy after a very few days' illness: while in other patients, who had lived for many months after the invasion of the disease, there has been no trace of such vascular membranes. It is clear, therefore, that the organization of the lymph does not depend solely upon the length of time that has elapsed from the period at which it was poured forth. It has much more to do with the previous state and habit of the patient. *Cæteris paribus*, plastic lymph and early adhesion are more to be expected in young, strong, and healthy persons; curdy unorganized lymph, granular deposits, with copious and abiding serous effusion tending to become puriform, in such as are old, feeble, cachectic, and scrofulous.

The extent of these false membranes varies, according to the extent of the inflammation which has produced them. When that has been general, they cover the whole lung, and line the whole costal surface, and spread themselves over the diaphragm and mediastinum of the same side. Supposing that there is no serous liquid effused, or that it is absorbed, the lung then becomes everywhere adherent to the sides of the cavity which contains it. The medium of adhesion, which is soft and tender while it is recent, grows firm, and assumes the characters of areolar tissue, when the union is of old standing.

The thickness of the false membranes is also extremely variable. Sometimes it is not more than that of the pleura itself, and the lymph might then, in the absence of adhesion, be almost overlooked. But in the majority of cases their thickness is much greater than this. Frequently several distinct layers or strata are seen, superposed one upon another, to a considerable depth.

Are there any auscultatory signs of this process of adhesion, when it occurs? Yes. There is a morbid sound, not hitherto mentioned by me, whereby it is sometimes disclosed: the sound, namely, of *friction*; the sound produced by the rubbing together of the dry, or inflamed and roughened surfaces. You doubtless are aware that every time a tolerably deep inspiration takes place, the relation between the ribs and the lung undergoes a change. While the ribs are elevated, the lung descends a little: and consequently any given point of the surface of the lung is no longer in contact with the same point as before of the thoracic parietes. You may convince yourselves of this fact by carefully making a small incision through an intercostal space, in a living animal. Now the pulmonary pleura, when that membrane is inflamed, does not slip and glide over the costal pleura in its usual smooth and noiseless manner; but it makes a creaking or rubbing sound, which the ear, applied to the corresponding surface of the chest, readily catches. I have very many times heard this; yet it is not at all a familiar sound: indeed I had heard it, in one instance, some time before I knew what the noise meant. The sound has, mostly, an interrupted character, occurring in a series of three or four jerks. The patient is often made aware of the harsh movement, by some internal sensation; and a bystander, who places his hand flat upon the corresponding part of the thorax, may sometimes feel this grating of the

membrane upon itself. You may wonder, as adhesions are so common, that this sound, and these sensations, are not oftener heard, and felt. In truth, they are transitory phenomena, and cease, of necessity, as soon as adhesion prevents any further motion of the opposed pleuræ upon each other. If we do not happen to listen during that period, usually a short one, in which the pleuræ, roughened by inflammation and effused lymph, but not separated by liquid, still chafe against each other, we lose the opportunity of hearing the sound at all. This *rubbing* sound, this noise of *friction*, we shall find to be of greater importance in relation to certain diseases of the heart, than in cases of acute pleurisy. In pleurisy the *liquid* matters poured into the membranous sac have far more interesting consequences: and to these I now beg your attention.

In some instances we find, after death, a clear, serous, or watery fluid, without colour, or of a pale lemon-colour, and perfectly limpid and transparent. This may occur, independently of inflammation of the pleura; from some mechanical obstacle to the circulation. It then constitutes a species of dropsy; a true *hydrothorax*: and this, though less common than ascites, is by no means an uncommon consequence of disease of the heart. When the effusion does not proceed from a cause of that kind, it is always, probably, the result of inflammation of the pleura itself, although we may find only a slight degree of redness upon its surface, or a few patches of coagulable lymph. More frequently, besides this clear liquid, with flakes of albuminous matter floating in it, there is also a coating of lymph on the inflamed membrane. Very often the thinner fluid is turbid, or whitish, like whey; sometimes it is distinctly puriform; sometimes it is tinged more or less deeply with blood; sometimes it consists of nothing else but blood, which has separated into serum and crassamentum. There being no wound, nor visible rupture, of large or of smaller vessels, we conclude, in such cases, that the blood has exuded from the membrane by what I have previously spoken of as capillary hæmorrhage.

The different kinds of fluid effused into the pleuræ are always, or almost always, without smell; — provided that it has remained a closed bag: I mean when no communication has been established between the cavity of the pleura and the external air, either through an opening in the walls of the chest, or through a pulmonary fistula leading to the trachea, or through some breach in the œsophagus. I have met with but one exception, and that a doubtful one, to this rule. A patient died in the hospital, who, some years before, had nearly killed himself by swallowing, in mistake for beer, a solution of caustic potass. The result of this had been ulceration, and subsequently stricture, of the gullet. His left pleura was perfectly full of most stinking pus; and we were unable to detect any channel of communication with the outward air, although the circumstances of the case rendered it not improbable that such a channel might have existed.

Sometimes air, or gas, is found in the cavity of the inflamed pleura; either alone, or (what is much more common) together with a liquid. We ascertain this fact, in the dead body, by the hissing sound that takes place as soon as a penetrating incision is made between the ribs: or by opening the thorax under water, and noticing the escape of air in the form of bubbles. It is probable that these gases are sometimes secreted or exhaled from the diseased membrane; sometimes they are the product of decomposition within the cavity; but, for the most part, they are admitted from without, the sac of the pleura communicating somehow with the external air.

Such being the fluid matters that frequently occupy the cavity of the pleura when that membrane has undergone inflammation, let us next examine the necessary effects of their being collected in that part. These effects will obviously vary considerably according to the quantity of the fluid that accumulates.

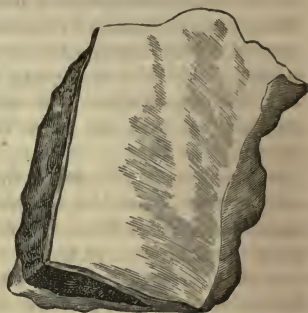
Now the quantity of fluid may vary from less than an ounce to several pints. At first it is lodged in the cavity of the pleura solely at the expense of the yielding lung, which is compressed to make room for it. But if the quantity continue to augment, other parts are at length displaced by the increasing pressure, the boundaries of the chest on that side are stretched, and even the abdominal viscera are thrust out of their natural position. The lung is pushed back towards the mediastinum and vertebral column, and flattened and brought to lie in the smallest possible compass; the diaphragm is forced downwards, which sometimes gives rise to a considerable prominence of one or the other hypochondrium, the spleen and stomach

being displaced on the left side, or the liver on the right. The ribs are separated too; the intercostal spaces become wider, and are pushed out to the level of the bones, and the whole of the affected side is smooth and obviously larger than the other. The mediastinum also undergoes some change of position, being driven more or less towards the side opposite to that on which the effusion exists. If the liquid happen to fill and distend the left side of the thorax, the heart may be moved out of its natural place, and be heard, and felt, and seen to beat on the right of the sternum. Andral mentions having met with only one instance of that kind. I suppose that I cannot have witnessed less than a score such. So again the heart may be carried beyond its proper place, to the left, by a large effusion into the right pleural cavity.

I say when the liquid is accumulated in very considerable quantity, the lung is pressed into the form of a thin cake, which occupies a very small space alongside the vertebral column: and if it happen to be covered over and concealed, as it often is, by a strong layer of adventitious membrane, we might fancy, at first examination, that it had completely disappeared. It was in cases of this kind — especially when the effused fluid consisted of pus — that the lung was erroneously represented by the older observers as having been *destroyed* by suppuration. However, you will always find the lung there if you take the pains to look for it, and to divide the false membranes that bind it down: and, in many instances, it is sound also. Its surface may indeed, be wrinkled, but the lung itself is capable of being restored to nearly its former volume by insufflation, as it is called; by blowing air into it through the principal bronchus of that side. In this compressed state the lung does not crepitate under the finger: it is dense, and sinks in water; in fact it is wholly void of air, and has been brought, by the pressure of the fluid around and upon it, into nearly the condition of the lung of the fetus that has never breathed. But its firmness, its resistance to being torn, and its capability of being again inflated, prevent our confounding it with hepatized lung. Sometimes its cellular texture is obliterated; the opposite surfaces of the vesicles and smaller air-tubes adhere together; the lung will not admit air: it looks like a piece of muscle, and is then said to be *carnified*.

Such is a general account of the anatomical characters of pleurisy, as they are disclosed to us by an examination of the body after death. We may now inquire what effect these changes are capable of producing on the *sounds* which are heard when the healthy chest is percussed, or listened at. We shall then be the better prepared to appreciate the several symptoms, general and physical, which are known actually to occur in pleurisy. Now it is clear that when the lung is pushed away from the walls of the thorax by fluid between the pleuræ, it will be compressed also; its capacity must be reduced; less air will be able to enter it. There will consequently be a proportional diminution in the intensity of the respiratory murmur; and this murmur will, moreover, be less audible in consequence of the distance, from the ear, of the structure in which it takes place. The lung is attached by its roots (so anatomists speak) to the spinal column. A moderate amount of effusion will, therefore, cause it to recede upwards and inwards: and a certain quantity of the liquid will ascend between the lung and the ribs, compressing the spongy pulmonary tissue around the larger and more resisting bronchial tubes. We might expect, in this condition of things, that the passing breath, and the voice, would be audible in those tubes, through the partially condensed lung, and through the circumfused layer of liquid: and it is so. We do hear bronchial respiration, and bronchial voice and cough; with some modification, indeed, to be noticed presently. In this respect,

FIG. 57.



Portion of the lower lobe of the left lung of a patient, compressed by turbid serum, occupying the pleural cavity. A thick layer of lymph covered the hepatized portion of lung: it was perfectly smooth from the contact with the liquid, and there was a free scalloped margin at some parts, of an inch in breadth. The exudation-matter consisted of filamentous matter, entangling corpuscular fibrin.

therefore, you will observe that pneumonia, which solidifies the spongy texture of the lung around the bronchial tubes by filling it with lymph or with blood, has the same effect, so far as acoustic principles are concerned, as pleurisy, which solidifies a portion of the lung by expressing air from it, and pours round the bronchial tubes a fluid which readily transmits sound. Hence bronchial respiration and bronchophony are not always indicative of the same condition of parts within the chest, but derive their true value and meaning from the context, if I may so say; from the circumstances under which they occur, and with which they are associated.

When the effusion is so copious as to squeeze all the air out of the spongy part of the lung, to pack the organ up along the vertebral column, to distend the thorax, and to compress strongly the bronchial tubes themselves, *no* respiratory murmur *can* then be heard, nor any tubular breathing; for the dilated chest can neither expand nor collapse, and, therefore, no air can pass along even the larger air tubes; neither can these compressed tubes vibrate with the patient's voice; wherefore bronchophony also ceases, or is but faintly audible.

Again, if percussion be made over a portion of the chest, beneath which, instead of healthy and spongy lung, there is incompressible serous fluid, a dull flat sound will be rendered. But a dull sound is rendered also when percussion is made over a solidified lung. Hence the mere dullness of the part struck does not inform us whether we have pneumonia or pleurisy to deal with, or some other disease that has the effect of making the lung solid, without plugging up the larger bronchi.

But an expedient presents itself, by which we may in some cases render this experiment of percussion conclusive. The dull sound occasioned by hepatization or other solidification of the lung occupies the same spot in every position of the patient. Not so, necessarily, the dull sound produced by the presence of liquid in the cavity of the pleura. The liquid will gravitate to the lowest part of the cavity, and will carry with it the dull sound. We place the patient, therefore, in different attitudes: and if we find the chest, when struck, resonant always in the higher, and dull always in the lower portions of the thorax, whatever its posture may be, then we may be sure that the cavity of the pleura contains liquid. In such a case, when the patient sits up, the dull sound will be elicited from the lower part of the chest, on one side, from the spine round to the sternum. When he lies on his back, the anterior of the thorax sounds hollow; the posterior dull: and when he reverses that position, and lies with his face downwards, these sounds change places also: the hollow sound is still uppermost, in the posterior part of the cavity; the dull sound still undermost, in its anterior portion.

There are just two states which may interfere with the true interpretation of the sounds produced by percussion in the manner now described; and these are, first, partial adhesions of the pleuræ, which may confine and isolate the effused liquid, and prevent its sinking from one part of the chest to another under the influence of gravity: and, secondly, so large an amount of effusion as to fill entirely the cavity and fix the compressed and empty lung in one position; for it is necessary, in order to obtain the shifting sounds in different attitudes of the body, that there should be light spongy lung to ascend, as well as heavier fluid to sink down, according to the posture of the patient. In this last case, that of excessive effusion, the whole surface of the affected side will yield a dull sound. It is seldom so in pneumonia: it is seldom that the entire lung on one side is so blocked up, in consequence of inflammation, as to give rise to universal dullness on percussion. But the diagnosis of these two conditions is an important one, and apt to puzzle a student. I hope to elucidate it as we go on.

We may now consider, with a better chance of understanding some of them, the *symptoms* which are generally met with in a case of pleurisy under its ordinary form and progress. The general signs, then, of that complaint are rigors, pain in the chest, dyspnœa, cough, difficulty or impossibility of assuming certain postures, and fever. Very much the same, therefore (as Cullen truly stated), with those of pneumonia, and, it may be added, with those of pericarditis: but auscultation differentiates these diseases. The physical signs I will examine presently. The general symptoms will bear, each of them, a short comment.

The *pain* which the patient feels — or the *stitch* in the side, as it is expressively called — is one of the most striking and characteristic signs of the disease. *Point de*

côté the French name it. It occupies a point or spot; and patients feel as if some sharp stabbing instrument were driven in at that spot every time that the act of inspiration goes beyond a certain limit. The Latin medical writers, attending chiefly to this prominent symptom, call pleurisy "*morbis lateris*."

This pleuritic stitch is subject to considerable variety in regard to its situation, its severity, and its duration. Most commonly it is felt on a level with or just beneath one or other of the breasts, in the part corresponding with the lateral attachments of the diaphragm: and this, even when the inflammation which occasions it is of much greater extent. Why is this? What is the cause of the pain? Wherefore should it be restricted to one small spot, when the inflammation pervades, it may be, the whole of the pleura? Pathologists have made attempts to explain these matters; but perhaps their explanations are not very much to be trusted to. They say that there is a larger amount of motion, at the lower part of the thorax, of the pulmonary over the costal pleura; and that the pain resulting from that friction, when the membranes are inflamed, is therefore felt where the friction is the greatest. However, the pain is not always confined to that spot. It is occasionally felt in other places, as in the shoulder; in the hollow of the axilla; beneath the clavicle; along the sternum; and sometimes it is complained of as extending over the whole of one side of the thorax. Andral states that he has observed the pain to prevail especially along the cartilaginous border of the false ribs, when the inflammation has attacked that portion of the pleura which covers the upper surface of the diaphragm. He says, too, that in such cases, the pain often affects the hypochondrium, and even extends as far as the flank, so that it might be mistaken for a symptom of abdominal inflammation. This observation is worth remembering. Sharp pain, occupying the right hypochondrium, belongs oftener to the pleura than to the peritoneum. I have known several instances in which such pain was erroneously supposed to be a sign of hepatitis, when in truth it resulted from inflammation of the pleura. Cruveilhier observes, also, that he has known the pain to affect the loins, and to simulate lumbago.

Whatever may be the situation of the pleuritic pain, it is generally increased by percussion, by intercostal pressure, by lying on the affected side, by a deep inspiration, by cough, and by different movements of the body.

In many patients the pain is exceedingly sharp, whether it be continued, or whether it occur only at intervals: the more *circumscribed* it is, generally the more *acute* it is. The patients are then in a state of great anxiety: they make very short and imperfect inspirations, through fear of aggravating the pain; they dread the least effort of coughing, or of sneezing, and suppress the desire to cough which the disease may occasion. There are other patients in whom the pain is moderate, is felt only when a *deep* inspiration is made, and is scarcely augmented by pressure or percussion. And there are even some cases of pleurisy which are unattended with pain from first to last.

The pain commonly exists from the very outset of the pleurisy. It is sometimes vague and fugitive at first, and becomes fixed and permanent after a day or two. In that case it may be mistaken for simple rheumatic pain; for pleurodyny; or for what is thought to be merely a nervous pain. When the pain is increased by slight pressure made *upon* the ribs as well as between them; when it extends over a large space; when it is unattended with fever; when it is inconstant or fugitive — we may *suspect* that it is situated in the fibrous and muscular tissues; but these circumstances do not afford any *certainty* that such is the case. In fact I have long been of opinion that some at least of the cases which pass under the name of pleurodyny, are really instances of what has been called *dry* pleurisy. You are aware perhaps that adhesions are very constantly found to exist between the lungs and the ribs in persons dead of pulmonary consumption. Such persons are liable to pains in the chest, beneath the clavicles, in the axillæ, between the shoulders, at the upper part of the dorsal region; in short, in those situations where the adhesions are found, after death, most frequently and in the greatest number. The pains indicated, it may be presumed, the periods at which the slighter forms of circumscribed pleurisy, attended with other effusion than that of coagulable lymph, took place. And it is probable that many cases of pleurodyny are really instances of the same kind of pleuritic inflammation. How often do we find, even when there are no tubercles in the lungs, firm adhesions between the pulmonary and costal pleuræ, in the bodies of persons who were never known to have had any pectoral disease! The pain alone marks the inflammation in those

cases; adhesion presently ensues; there is no fever perhaps, or none that attracts much notice; the pain soon subsides, and is soon forgotten; but the adhesion, the consequence of the inflammation, remains: and this is a morbid condition which is neither revealed to the sense of hearing, nor in any other way. I am much disposed therefore to agree with Cruveilhier in thinking that "*pleurodyne is nothing else (in many cases at least) than adhesive pleurisy.*"

I need scarcely repeat the fact which has so many times before been mentioned in these lectures, viz., that the inflammation of membranous parts, and especially of serous membranes, is attended with much more pain than inflammation of parenchymatous parts. We cannot have a better example of it than is afforded in most cases of pneumonia. Most cases of pneumonia are accompanied in the beginning with a stitch in the side; some cases are not. In those cases in which the stitch happens, the pleura also is inflamed to a certain degree, and the pain depends upon the co-existence of the pleurisy: they are cases of pleuro-pneumonia. In pure pneumonia, on the contrary, the pleurisy being wanting, the sharp pain is wanting also.

The respiration in pleurisy, at its outset especially, and while there is still pain, is considerably embarrassed; the movements of inspiration in particular are short, hurried, and often interrupted or jerking. And this depends evidently upon the pain, which forbids the free contraction of the muscles that dilate the thorax; and you may often observe that the dilatation is sensibly less on the affected side than on the other. Cruveilhier indeed denies this; or rather he states that he has never observed it: but it certainly is not an uncommon phenomenon. I have noticed it and drawn the attention of others to it, again and again.

When effusion has taken place—that, one can easily understand, will be likely to aggravate the dyspnœa; and it will aggravate it in a greater degree, or in a less, according to circumstances. Thus, if the *other* lung happen to be a diseased lung, then the compression of that which is on the side of the pleurisy will have a more injurious effect upon the breathing. The dyspnœa arising from the effusion and consequent pressure upon the lung will also be in proportion, first to the *amount* of the effusion; and secondly, to the *rapidity* with which it has taken place. When the effusion has been slow—or when it has long existed, and the case has become chronic—the circulation through the lung has had time to accommodate itself to the altered condition of the parts, the disturbed equilibrium between the quantity of air and the quantity of blood in the lung is restored, and the dyspnœa is consequently slight.

But there are very singular exceptions met with to all this. Andral states (and I have seen more than one instance confirmatory of his statement) that there are persons, with pleuritic effusion enough not merely to fill but to dilate that side of the chest on which it exists (and you will observe that we cannot doubt about the presence of the effusion in such a case), who appear nevertheless to be quite free from dyspnœa; and *that*, not while they are at rest merely, for they talk, get up, walk about, even take long journeys, without their respiration becoming so short as to make them complain of it. Now this is conceivable enough in old and chronic cases; but Andral further affirms that this absence of dyspnœa is not restricted to those cases in which the collection of fluid has taken place slowly; but sometimes happens, even in patients in whom pleurisy has led to abundant effusion in a few days. He gives a case of this kind, in which the patient was not prevented by an enormous pleuritic effusion from carrying on, without fatigue, in the streets of Paris, his business as a carter. I remember having a butcher in the Middlesex Hospital in exactly the same predicament; and nothing could persuade him that he was otherwise than well, and fit to go out; and out accordingly he went. Remember, therefore, that there are great varieties in this respect. In some patients the dyspnœa never ceases to be urgent from first to last; and these are apt to prove fatal cases. In others the respiration is very much impeded at first; then the difficulty of breathing diminishes; and at length it ceases, long before the fluid is reabsorbed. In others again, by some unaccountable idiosyncrasy, the respiration remains at all times very facile, both at the outset and during the progress of the disease.

Cough is another of the ordinary symptoms of pleurisy. It does not occur in paroxysms. It is small, half-suppressed, ineffectual. In some few cases this symptom also is entirely absent, even though the inflammation is intense, and the effusion into the pleura considerable. When cough does exist it is dry; or it is accompanied by

the expectoration of slight catarrh. If much frothy mucus should be expectorated, the pleurisy is complicated with bronchitis; if rust-coloured sputa be brought up, it is complicated with pneumonia: and in each case other signs, proper respectively to those two diseases, will be present.

A good deal has been said and written respecting the position which a patient assumes who is labouring under pleurisy. The manner of the *decubitus* has even been regarded as one of the pathognomic signs of the disease. Yet, strange to say, observers are much at variance with each other in respect to this so-called pathognomic symptom. Some affirm that the patient lies on the side affected; others that he can lie only on the sound side; others again that he lies neither on one side nor on the other; or even that he lies indifferently in any posture. But this dispute is an exact counterpart of the celebrated quarrel which took place about the colour of the chameleon: "they all are right, and all are wrong." I believe that, if you narrowly inquire into the facts, they will be found to be somewhat as follows:—In the outset of the disease, while there is yet pain, the patient cannot lie on the affected side on account of the pain, which that position exasperates; he lies therefore on the sound side, or on his back; sometimes he is obliged to sit up. At a more advanced period of the disease, when the pain has ceased, and considerable effusion has taken place, he cannot lie on the sound side, because of dyspnoea: the dilatation of the chest on that side would be impeded by such a posture; and what is more, the effusion, lying uppermost, would press upon the mediastinum, and so further tend to restrain the expansion of the sound lung. But he is no longer prevented by pain from lying on the diseased side, and consequently he does, in some instances, take that position: but more commonly still, he lies in what Andral calls a *diagonal* posture; *i. e.* the patient is not on his back, nor on his side, but between the two; on his back, we may say, but inclining towards the affected side. Again, however the fact may be explained, it is certain that there are some few persons who lie indifferently on the back or on either side, without augmentation of the dyspnoea in any of these positions, though one side is choke-full of liquid.

Now, of the symptoms that we have hitherto been considering, the pain, the dyspnoea, the cough, the accommodation of position, there is not one which, taken alone, can be said to be strictly or absolutely pathognomonic; or which indicates in a positive and certain manner the existence of pleurisy, or of pleuritic effusion. Yet when all, or several of them, occur together, they afford a degree of probability on these points almost equivalent to certainty. There are yet some other, and more conclusive signs, which either in themselves, or taken in conjunction with those already mentioned, render the diagnosis of pleurisy easy and sure. These signs are furnished by the size of the thorax on the affected side; by its forms and motions; and above all, as you will have anticipated, by percussion and auscultation.

I have already stated that in some cases, that side of the chest which contains the effused fluid becomes evidently larger than the opposite side. The ribs and their cartilages present that position which they assume during a deep inspiration: the intercostal spaces are pushed outwards and brought up to the level of the ribs; and occasionally fluctuation may be perceived in those spaces, through the muscles. When these appearances are observable, no doubt (or *scarcely* a doubt) can remain concerning the nature of the disease. This dilatation of the thorax on the diseased side is more common in old chronic cases than in the earlier periods of acute pleurisy; yet it *may* take place in a very short time. Andral declares that he has known it sometimes reach a great degree by the fourth or fifth day of the acute disease. You may satisfy yourselves that the side is dilated by measuring it with a string. Carry the string round the chest, upon a level with the extremity of the xyphoid cartilage, then fold it upon itself, and you will find that the half of it will more than encompass the sound moiety of the chest, and will not reach round the diseased. The diseased side may measure an inch, or an inch and a half, or even sometimes two inches, more than the other. But this measurement by a string is seldom necessary. The eye takes a very accurate estimate of the comparative volume of the two sides; and the obliteration of the intercostal spaces can only be ascertained by seeing or feeling them. It is necessary to remember that, in most persons, the right side is naturally somewhat the larger of the two.

I say when this dilatation is noticed, *scarcely* a doubt can exist of the true nature

of the case. Some time ago I should have said *no* doubt: but having myself mistaken such a case, and seen others mistake it, I introduce this slight qualification, although it is a thousand to one against another such instance occurring to puzzle or mislead the observer. My colleague Dr. Hawkins had a patient in the hospital, in whom this dilatation of one side of the chest was exceedingly well marked. It was the left side that was enlarged; the heart was evidently pushed over to the right of the sternum. This is another circumstance strongly corroborating our conclusion in such cases. The intercostal spaces were effaced, and the whole of that side was perfectly dull on percussion. The poor fellow had a very unhealthy aspect; and he had, some time before, suffered amputation of a leg, for what was understood to have been scrofulous disease of the knee-joint. It was not unnatural therefore that every one who saw him should have come to the conclusion that this was a case of empyema; of fluid, and most likely of pus, collected in the pleura, and very probably the result of the extension of scrofulous disease from the lungs. Under these circumstances, and inasmuch as his dyspnoea was not urgent, it was not thought right to take any steps for evacuating the presumed fluid. The case was pointed out to the pupils as a capital example of empyema. At length the patient died; and when his body was examined we discovered—what think you? not pus, nor serum, but a large red *solid* mass, in the centre of which, when it was divided, was still a red, but softer, pulsatious, half-fluid substance. At first it was thought to be cancerous degeneration of the lung; but it was soon noticed that the solid part was arranged in concentric layers, like those which are often seen in aneurismal tumours; and further research showed that the effusion had indeed once been liquid, for it consisted entirely of blood, which had coagulated in the manner I have just described. And the source of the blood was detected. A portion of two of the ribs had been destroyed by ulceration, and one of the intercostal arteries had thus been laid open. The lung was found uninjured, but totally empty of air, and pressed flat up against the mediastinum.

No precaution could guard against such a source of fallacy; and you are not likely ever to meet with just such another case: yet I have thought it sufficiently interesting to relate, in illustration of the subject immediately before us.

It is unfortunate, so far as the diagnosis is concerned (but not in any other sense), that dilatation of the thorax is far from being a constant symptom, even in cases in which the effusion is very considerable.

There is still a condition of the thorax to be described, which is the very opposite to this. When the effused fluid begins to be reabsorbed—and when some cause or other, generally the formation of adventitious membranes, prevents the lung from re-expanding and approaching the ribs in proportion as the fluid is removed—then of course the ribs *must* sink in, and approach the lung, to prevent that void which would otherwise exist between the ribs and the lung. Consequently that side of the chest on which the fluid has existed becomes narrower than the sound side. And the actual difference between the two will be augmented by the circumstance that, in such cases, an amplification of the sound lung, and of the cavity in which it is lodged, a true compensatory hypertrophy, commonly takes place.

This partial or general retraction of one side of the chest is not so much a sign of disease actually in progress, as of disease gone by; and it may exist without evident disturbance of the health, of any kind.

Persons who are thus affected have the appearance of being inclined towards the diseased side, even when they endeavour to hold themselves upright: and the deformity, for such it is, becomes manifest to the eye when the chest is uncovered. You see that the side is narrowed and shrunken. All its dimensions are contracted. It measures less, in circumference, by an inch or more, than the other side. The shoulder is depressed; the hypochondrium is tucked up; and the ribs are drawn close together. A patient of mine, whose chest had been punctured (a remedial procedure to be spoken of presently), and who drew off daily, with a syphon, pus which did not otherwise find vent, had such difficulty at last in introducing the tube between his ribs, that excision of a piece of the bone was contemplated by the eminent surgeon who had performed the operation. The effect of the atmospheric pressure is sometimes so great as to crook the vertebral column, and produce lateral curvature of the spine. This I have myself witnessed. And as one of the unseen walls of the cavity,

viz. a part of the diaphragm, is carried permanently up under the ribs, so another of the unseen walls, the mediastinum, is liable to be influenced by the tendency to contraction. The heart, which, when the *left* pleura is *distended*, is apt to be *thrust* over, beyond the sternum on the right, may thus, when the *right* pleura is *contracted*, be *dragged* into the same position. In the former case, the dull sound given out by the diseased side when struck, will transgress the mesial line and encroach a little upon the healthy side: in the latter, the resonance yielded by the healthy will transgress the mesial line, and encroach a little upon the diseased side.

The difference of the two sides is so striking, that, at first sight, an observer supposes it to be even greater than it is actually found to be by admeasurement. Yet I have met with this deformity, as Laennec declares that he also had done, in persons who were not themselves aware of its existence. Some of them have not even known that they had suffered any previous thoracic disease.

The conditions I have just been describing are *physical* conditions; and the signs they furnish are *physical* signs. I have still to speak of the remaining physical signs which are also *auscultatory* signs. What I have already said upon this subject in the present lecture will, I trust, enable you almost to foresee the kind of information which these signs afford in actual practice.

As soon as even a slight amount of effusion commences in the pleura, it is announced by a diminution of the hollow sound which percussion elicits in the healthy state. In proportion as the effusion becomes more considerable, the chest, when struck, gives a sound more and more dull. At first this flat sound is rendered opposite the lowermost depending part only of the cavity; and this, as I showed you before, forms one ground of distinction between the dulness on percussion in pleurisy, and in pneumonia. However, at length, the effusion augmenting, the dead flat sound may proceed from the whole of the affected side; and this forms another ground of distinction: for it is very seldom that the whole lung becomes so solid in pneumonia as to yield a uniform dead sound over the whole of one side of the chest. Either the dull sound is universal on one side, or it is not. If universal, it is not likely to be the result of solidification by pneumonia; or, I may add, by tubercles: if not universal, the dull sound will (except in some rare cases) shift its place as the patient alters his posture.

I may mention another ground of diagnosis, which may be of great assistance when the case is seen from the beginning. The dulness comes on much more quickly in pleurisy than in pneumonia. It has been noticed within twelve hours from the invasion of the disease. In living animals, a considerable quantity of serous effusion has often been very rapidly produced by injecting some slightly irritant matter into the cavity of the pleura. In pneumonia, the dulness is commonly later in its appearance. The induration of the lung is gradual; and so is the pneumonic dulness on percussion: the effusion of serous fluid is early and rapid; and so also is the coming on of the pleuritic dulness. Moreover, as I have just shown you, pleurisy may displace the mediastinum, and cause the *whole* sternum to give a dull sound. A hepatized lung will render *one-half* only of it dull.

The intensity or completeness too of the dull sound is generally greater in pleurisy than in pneumonia. In two days, or even in twenty-four hours, the whole cavity of the pleura on one side may be filled quite full; and the whole of the corresponding surface of the chest, from its base to its summit, will yield a sound (to use one of Avenbrugger's strong expressions) *tanquam percussi femoris*. It is very uncommon for such total and universal *matité*, as the French call it, to result from inflammation of the lung.

Again, in the outset of the disease, while there is yet little or no effusion, but when the pain is acute, the vesicular breathing is heard more faintly and feebly on the painful side than on the other. On that side also the walls of the chest are less forcibly expanded. But percussion, when the pain will permit of its being practised, gives the same sound on each side. It is clear that the sharpness of the pain causes the patient instinctively to expand the chest on that side as little as possible: and, consequently, the quantity of air that penetrates the lung in a given time is diminished, and the respiratory murmur is feeble.

As soon as effusion commences, the vesicular rustle is heard still less plainly on the affected side; and in proportion as the fluid increases, that rustle or murmur becomes more and more faint: and at the same time it becomes more distinct and noisy than

natural—*puerile*, in fact—on the sound side. And while the respiratory murmur is disappearing on the diseased side, and the spongy lung is becoming empty of air from the pressure of the augmenting fluid, and the larger bronchi are surrounded by compressed lung and by incompressible liquid, the bronchial sounds begin to be heard, which I formerly described—the bronchial voice, the bronchial respiration. But the sounds are not exactly the same as those which are heard in pneumonia. They are modified by the nature of the substances through which they pass. The voice, for example, is still bronchial, still the voice of a person talking into a tube: but it has a superadded character; it is trembling, quivering, thrilling, cracked, discordant. I strive in vain to convey to you by these epithets a notion of this remarkable modification of the voice. Laennec's happy similitudes may enable you to form a more exact conception of it. It is like (he says) the bleating of a goat; or, happier still, it resembles the voice of Punch. But when once you have heard the sound you will never forget it again. I presume that this modification of bronchophony (for such it is, and such I would have you consider it) is caused by the rapid undulations communicated to the effused liquid by the vibrations of the bronchi and condensed pulmonary tissue. The sound is usually most distinctly heard near the inferior angle of the scapula, the patient being in a sitting position. It disappears, or merges into pure and distant bronchophony, when the liquid exceeds a certain amount, so as to compress the bronchial tubes themselves, and to *damp* their vibration.

I would have you recollect, therefore, that *ægophony*, which is the technical appellation of the sound I have just been describing (goat-voice),—*ægophony* is nothing more than a species or variety of bronchophony; and the two run each into the other by such fine gradations, that it is sometimes difficult to say which it is we are listening to. When the quivering is strongly marked we may be certain that it denotes effusion into the pleura; when bronchophony only is heard, we cannot be sure, from that sound alone, whether there be indurated lung between the ear and the bronchi; or a liquid, and a portion of compressed and condensed lung: but other phenomena complete the diagnosis.

Do not forget that, when any modification of the voice is heard, or thought to be heard, on the suspected side, the sound of the voice in the corresponding part of the other side of the chest must be ascertained also. It is only by a *comparison* of the two sides that we can come to any safe conclusion; and that *comparison* becomes often a striking and most instructive *contrast*.

LECTURE LIII.

Pleurisy continued. Recapitulation of Symptoms; of Diagnostic Signs. Causes of Pleurisy. Pneumothorax; its Conditions and Signs. Treatment of Pleurisy. Empyema. Paracentesis Thoracis.

IN the last lecture I enumerated the symptoms, general and physical, which are met with, more of them or fewer, in cases of acute pleurisy. I then considered them singly; it may be well to take a rapid recapitulatory view of them as they exist together or in succession, and compose the actual disease.

The outset, then, of pleurisy, is marked by sharp stabbing pain, most commonly situated beneath one of the breasts, and preceded or accompanied by rigors. These two signs, the stitch and the shivering, are sufficient of themselves to awaken a strong suspicion that pleuritis has set in. At the same time there are usually a dry cough; a dread of breathing; a check or catch in the inspiration, which is curbed, so to speak, by the pain; fever; often a comparatively feeble respiratory murmur on that side on which the pain is felt; and the patient cannot lie on that side. If no liquid effusion take place, these symptoms ordinarily disappear at the end of a few

days, and the patient recovers. The case has been a case of dry pleurisy; and the chances are much in favour of the lung having become permanently adherent to the ribs.

I should have said, with respect to the *fever*, that at the outset of the pleurisy it is often high. And it was matter of observation long before the method of auscultation was thought of, as well as since, that in the acute period of the disease the *pulse* is remarkable for its hardness, and forms a contrast with the softer pulse of pneumonia, and with the small and contracted pulse of inflammation of the serous membranes of the abdomen. Indeed the older physicians laid great stress upon the quality of the pulse, in their endeavours to distinguish pleurisy from pneumonia.

But to resume the description of the symptoms of pleuritis. Where effusion takes place (and it does so very early, so as to form a part of the complaint, just in the same sense in which expectoration forms a part of catarrh), the sound elicited by percussion becomes dull on the side on which the effusion exists. While the effusion is moderate, the dullness shifts its place according to the posture of the patient, and is heard only when the lowermost part of the chest is struck. But the fluid may soon increase so much as completely to fill the pleura; and then the whole of that side is dull. Meanwhile the murmur of respiration becomes feeble and faint, and at length, as the effusion augments, ceases altogether; while on the sound side it grows noisy and puerile. Tubular breathing, and that modification of the bronchial voice which medical men have agreed to call *ægophony*, become audible during the early periods of the effusion. *Ægophony* is heard, however, only so long as the quantity of liquid poured out observes a specific limit. There must be a certain amount of effusion—and there must not be more than a certain amount. I have sometimes thought that the discordant sound might depend upon the propagation of undulations through successive media of different densities. It certainly is somehow connected with the presence of a stratum of liquid between the lung and the ear. When the lung is strongly compressed, and especially when the cavity is stretched and distended by the enclosed fluid, the side is necessarily motionless; no tubular or other breathing can any longer be heard, or even occur: nor is the voice conducted, except perhaps very faintly, to the listening ear of the physician.

When the effusion is great, that side of the thorax on which it has taken place becomes, often, more or less dilated; and I should add, that the integuments on the same side are frequently *œdematous*. The patient now cannot lie on the sound side: and the most common posture is that which is intermediate between the supine position and the lateral; he lies *towards*, but not, in general, *upon*, the affected side.

I observed, in the last lecture, that this inability, after the effusion has reached a certain point, to lie on the sound side, might be accounted for in two ways. Partly it may be owing to the impediment which lying on the sound side offers to that side's expansion. The muscles which dilate the healthy side have then to lift, as it were, the weight of the body, and are, some of them, pressed upon and incumbered in their action, by that posture. But the inability in question is chiefly attributable to another circumstance, viz., the pressure exercised by the effused fluid downwards, through the mediastinum, upon the only lung that is left to perform the function of breathing. Now disputes, or differences of opinion, have arisen as to which of these two circumstances is the most efficient cause in this matter: and therefore it may not be amiss to provide you with the facts which prove that the last-mentioned cause is, in reality, the most operative—I mean the weight of the superincumbent liquid, in the supposed position, upon the mediastinum and upon the healthy lung below it. This is shown by the fact, that patients, to whom the decubitus on the sound side had previously been impossible, on account of dyspnœa, have been able to rest in that position *immediately after* the artificial evacuation of the fluid. Now in such a case the obstruction to the dilatation of the healthy side, produced by placing it under the weight of the body, would remain the same as before, or nearly so. A hospital patient of mine, named Coggs, could not breathe if he attempted to lie on his right side. His left pleura was distended by liquid effusion. I thought fit to have paracentesis performed: and the poor man was greatly delighted to find himself at once enabled by it to assume the posture which his weariness had long made him wish for, at which he had not been capable of enduring. We found, by percussion, that the diseased side was now filled with *air*; the compressed lung had not risen at all; so

that the necessity for the free expansion of the sound side was just as great as before the operation. You may find a precisely similar consequence of the same operation related in the fifth volume of the *Dublin Transactions*.

The œdema that is sometimes observed on the diseased side is more or less connected, probably, with the habitual position of the patient.

There is yet another sign of pleuritic effusion, which, as it is very simple, and readily perceived by even the least instructed observer, is too valuable to be neglected. In most persons, one's open hand, laid flat upon the surface of the chest, feels the vibration or thrill which the voice occasions when the person speaks. Now, in a case of pleurisy with effusion, you will generally find a remarkable contrast between the two sides in this respect: *i. e.*, the thrill is strong and evident on the sound side, and not perceptible at all on the other. Whereas, when the whole side is dull in consequence of the solidity of the lung, the thrill is much *augmented* on that side. But this thrill is not always present in the healthy state, and then we can infer nothing from its absence on the diseased side. Its presence, however, under such circumstances, would indicate consolidation of some kind.

After a while, when the fever has ceased, the liquid begins to be reabsorbed: but as, in many cases, the lung is more or less bound down by adhesions, or overlaid by a membranous stratum of lymph, it cannot expand in proportion as the liquid is removed: and the necessary consequence is *that* shrinking of the affected side in all its dimensions which I fully described yesterday.

Let me now briefly re-state the points of distinction between pleuritic effusion and pneumonic consolidation, when the one or the other of these two morbid conditions is proved to exist by dullness on percussion, extending over the whole of one side of the chest. The question is one which frequently arises; and it is one of much interest and importance.

First, then, we distinguish these different conditions, having some physical signs in common, by their *history*. In pleurisy, sharp pains and a dry cough, or perhaps no cough, precede the dullness: and we have not the crepitation, nor the rust-coloured sputa, which are antecedent to the dullness of pneumonia.

We cannot, however, always learn the previous history of a given case.

Secondly, a lung rendered solid by inflammation does not *distend* the cavity. Copious pleuritic effusion most frequently does. In the first case, therefore, we have not that separation of the ribs, that obliteration of the intercostal depressions, that protrusion of the corresponding hypochondrium, that mensurable enlargement of the side, that extension of the dull sound beyond the middle of the sternum, or that displacement of the heart, which are, some or all of them, apt to result from a collection of liquid in the pleura.

Thirdly, the solid lung transmits the voice from the pervious bronchi to the surface of the thorax; and if any motion of the affected side remain, it transmits also the sound made by the passage of the air through them. These phenomena are wanting when the pleura is so stretched by its liquid contents as to make the side everywhere dull to percussion.

Fourthly, the vibration of the thoracic parietes, caused by the patient's voice, is augmented by consolidation of the lung; prevented when it is strongly compressed by imprisoned liquid. The increase of this thrill can be felt therefore in the one case; its diminution in the other.

This simple test fails to be applicable when, from the feebleness or the high pitch, of the person's voice, no thrill is perceptible on either side in the healthy state. Unless, indeed, disease should generate a vibration, which, in such a case, would certify solidity.

Fifthly, a patient having one lung solid, is generally indifferent as to posture. A patient having one pleura quite full of liquid, lies (usually) on or towards that side; and is distressed and suffers dyspnoea if he attempt to lie on the other.

It is of more importance that we should inquire into the *exciting causes* of pleurisy than into those which give rise to pneumonia. They are more numerous and complicated, and have a more direct bearing upon the prognosis and treatment in the one case than in the other. I do not desire to refine too much; and therefore I shall

restrict myself to those causes which are obvious, and which you are likely to meet with in practice.

I merely say of exposure to cold, as an exciting cause of pleurisy, that it is a very common—the *most common*—cause. You know already all that I can tell you of the circumstances that are likely to render that cause effectual in producing internal inflammations, and, among the rest, pleuritic inflammation.

But pleurisy is often occasioned by mechanical violence; or by the accidental extension of disease from other parts; and the course, and the event, of the disease, are liable to be considerably modified by the nature of its cause in such cases.

Pleurisy may be excited by the splintered ends of a broken rib; and if the pulmonary pleura be wounded in that manner, air may get into the pleural cavity, as well as into the areolar tissue beneath the skin; constituting the true and genuine *emphysema* of our forefathers. Pleurisy may be determined also by a penetrating wound of the thorax; or by a perforating ulcer of the pulmonary pleura, the extension of a tubercular excavation. In the one case air will enter from without, if the aperture be sufficiently large; in the other, air will pass from the lung into the cavity of the pleura. In all of these cases of air finding its way into this serous sac while in a state of inflammation, the event of that inflammation is much more likely to be the effusion of *pus*, than when no communication exists between the inflamed membrane and the atmosphere. This I have mentioned, and offered some explanation of, before. But another very curious consequence results from the admission of the air, and its coexistence with puriform or other liquids in the sac of the pleura. New auscultatory signs arise, very easily appreciated, very instructive, and therefore very necessary for you to be acquainted with.

You must know that when the pleura contains air alone, the patient is said to have *pneumothorax*; and when (what is infinitely more common) the air is there in company with liquid, he is said to have *pneumothorax with effusion*. This is the name given to that condition of the chest by Laennec; and it serves its purpose sufficiently well. I shall take leave to employ the simple term *pneumothorax*, in speaking of either condition; whether there be liquid also in the pleural cavity or not. *Pneumothorax*, then, often proceeds from one or other of those causes of pleurisy just mentioned. It is sometimes produced too by the operation of *paracentesis thoracis*; by the opening made into the thorax with the trocar of the surgeon, in order to let out its fluid contents; in plain English, by *tapping* the chest.

The modifications of sound that result are particularly curious. Of course the air occupies the higher portion of the cavity and the liquid the lower, in whatever position the patient may be placed. And this being the case, percussion will give a remarkably hollow sound when made upon the uppermost part, and a totally dull and flat sound when made upon the lowermost part: and the change from the hollow to the dull sound will often take place quite abruptly, so that you may trace out the exact level at which the surface of the effused liquid stands. And if you reverse the posture of the patient, the resonant and the dull sounds will interchange their respective places: the uppermost part always yielding the clear, and the undermost the flat sound. This is just what you would expect. The result of the experiment is the same whether you make it upon the human thorax, or upon a beer-barrel. The resonant part, you are to observe, will be much *more* resonant than it would be in health—more resonant (you have always the other lung to test it by) than the corresponding portion of the opposite side of the chest—*tympanitic*, drum-like; for the air is not involved in spongy lung, but contained in a free space: and the sound is not damped, as in a healthy chest it is damped somewhat, by the *presence* of the lung. Moreover no respiratory murmur can be heard where this *tympanitic* resonance occurs: nor can any thrill be felt. Now I say all this is no more than you must have foreseen. But the sounds detected in this new condition of things by the ear applied to the chest, as the patient *breathes* or *speaks*, or *coughs*, you would *not*, I think, have anticipated. You hear then a sound which I must endeavour to describe in words, but which you will scarcely form a right conception of till you have heard it, and then all further verbal description will be needless. I can describe it by similitudes only. The patient's breathing is like the noise produced by blowing obliquely into an empty flask; and so the French have given the sound the somewhat magnificent title of "*amphoric resonance*." I have heard, fifty times over, exactly the sound in ques-

tion when I have been out shooting in a gusty day, and the wind has blown sideways into the gun-barrel. It is a ringing, metallic sound. When this is present during the *breathing*, the *voice* also has, even *more strongly* in general, this metallic character; and so has the cough; and each of them is apt to be succeeded by a tinkling echo. The voice, and cough, resemble those of a person who speaks or coughs into a deep well: or with his head bent over an empty copper boiler. The same ringing quality is often heard when one speaks in a large vaulted room; or beneath the arch of a stone bridge. You may perhaps now have some idea of what these metallic sounds are. They are very singular: and they are perfectly decisive (as far as my experience has gone) of the presence of air in a considerable cavity, within the thorax; which cavity mostly contains liquid also: and of the presence of air and liquid in the cavity of the *pleura* in particular. I do not know that the liquid is essential: I do not believe it is; but commonly there is some liquid, and a good deal of air. Almost always, too—but *that* is not indispensable—the cavity communicates with the external air, either through the walls of the chest, or through the bronchi. Neither is it necessary that the cavity should be in the *pleura*, for it may be in the lung: and when we come to speak of phthisis, I shall point out the circumstances which may enable you to determine whether the sounds proceed from a tubercular cavity, or from the sac of the *pleura*. What you will please to remember is that, in actual practice, in ninety-nine cases out of a hundred, these sounds will be found to denote the presence of both air and liquid in the cavity of the *pleura*; and the probable existence of some passage of communication between that cavity and the *external* air: in a single word, they will reveal the existence of pneumothorax. The voice reverberates in the little cavern just as it does in a large empty room with a stone roof; and this is the best explanation I can give you of the phenomenon. Sometimes, as you are listening, especially if the patient have recently changed his posture, you will hear a sound just like that occasioned by dropping a pin's head into a glass vase, or into a metal basin: and to this sound the name of *metallic tinkling* has been given. It really often closely resembles the distant tinkling of a sheep-bell. This is supposed to result from the dropping of the liquid from the upper part of the cavity; or sometimes from the bursting of a bubble on the surface of the liquid during respiration. You may succeed now and then in hearing a species of the same metallic tinkling by applying the stethoscope over the stomach, when percussion has already taught you that it is distended with gas, and by getting the patient to swallow some drink in successive teaspoonfuls.

Another auscultatory sound, arising out of the same condition, viz., the presence of both air and liquid in the cavity of the *pleura*, and known even as early as the time of Hippocrates, is rendered audible by *succussion* of the patient's body. You lay your ear upon his side, and get him to give his body a sudden jerk or jog: or you get some one else to take him by the shoulders and shake him; and you hear the liquid splashing within: just as you hear it when you shake a cask that is neither full nor empty of water. This is an unequivocal indication of pneumothorax; and demonstrates beyond a doubt that there is both air and liquid in the pleural sac; for no sound would arise if there were liquid only. A moderate quantity of liquid will make a greater squash than a large quantity. Unequivocal I say it is, because one could scarcely be misled by the splashing which may sometimes arise from wind and water mingling in the *stomach*. I wish that a patient, who was under my observation for some months last year in the Middlesex Hospital, and could produce this splashing noise at will, were there now; for he was not a little proud of his fatal gift, and I should have brought him down here to-day, and given you an opportunity of hearing this sound for yourselves, worth a dozen descriptions of it.

It is surprising how long this state of things within the thorax may last, without any great declension of the patient's general health and strength, even when the disease is (as it mostly is) incurable. Two men, patients of mine, both of whom had well-marked pneumothorax in connexion with tubercular phthisis, remained in the hospital for several months; till, in fact, I could conscientiously keep them there no longer: and each of them went away in very tolerable plight. I was unable to trace them afterwards, for they returned to their homes, the one in Ireland, the other in the north of Scotland.

In June, 1853, I was consulted upon the case of a lady in whose right chest, near

the angles of the ribs, just below the scapula, the physical signs of pneumothorax were strongly pronounced; amphoric breathing, amphoric voice, metallic tinkling, and a splash following succussion. This last sound I and others often heard, while standing near her, without any application, direct or mediate, of the ear to her chest. It was audible by herself when she was jolted in a carriage, and when she cantered on horseback. She first heard the internal splashing, while cantering, in June, 1852. In November of the same year she had borne a child. Under cod's liver oil she grew strong and stout, and could take a good deal of horse exercise. She died, rather suddenly, at a distance from London, in January, 1855.

In this instance pneumothorax existed at least two years and a half; probably longer. The patient began to cough in 1851.

You see, then, that the conditions of pleurisy, and the symptoms of those conditions, may be modified by its causes. All those causes that imply the introduction of air into the cavity of the pleura, imply also a more serious state of disease than results from most other causes. The perforation of the pleura by the extension of a vomica, I have mentioned as one of those causes. But tubercles in the lungs are frequently, very frequently, the cause of pleurisy, when no such perforation has taken place. A tubercle, or a group of tubercles, approaches the surface of the lung, but does not break through. Generally the pleurisy so produced is slight and partial, and ends in the formation of adhesions: it is *dry* pleurisy. And this very common occurrence of adhesions between the costal and pulmonary pleuræ, in the course of tubercular disease of the lungs, is, in truth, one reason why *perforation* of the pleura, and pneumothorax from that cause, is comparatively so rare. The part where the perforation is likely to take place has generally, though not always, been secured and clouted, as it were, by previous adhesion. So that even here we find that inflammation has a conservative tendency, and helps to postpone the fatal ending of the specific disease.

Pleurisy may terminate in resolution and complete recovery; or in adhesion, which is its next best termination, and which obtains for the patient, at the expense of some trifling embarrassment of his breathing, complete security for the future against the dangers of pleuritic effusion. Again, acute pleurisy may end in chronic disease of the pleural cavity: *i. e.*, in a shrinking inwards of the walls of the chest, attended with total uselessness, or a very imperfect and limited use, of the corresponding lung. Lastly, pleurisy may terminate in death. It may cause effusion so copious, that the patient will die of actual suffocation, unless the fluid be removed by art. On the other hand, he may die worn out and exhausted by the disease, especially if it be attended with suppuration. In that case he will suffer hectic fever, and all its wasting and mournful accompaniments; and death ultimately by asthenia. It is seldom that simple idiopathic pleurisy proves fatal.

As the matter from a tubercular cavity may break *in* upon the pleural sac, and lead to the admission of air, and the establishment of pneumothorax; so the puriform fluid which has resulted from inflammation of the pleura, and was for some time imprisoned in its sac, may also break *out*, and the result will still be the admission of air, and pneumothorax. This is not a very frequent result of pleurisy, however. When it occurs, an abscess forms externally, generally in front of the chest; and either the abscess bursts, or it is opened by the surgeon, and then it is found to communicate with the cavity of the pleura.

Sometimes air is effused into the sac of the pleura, in consequence of the rupture of dilated air-cells on the surface of the lung; of this accident of disease I may refer you to an instance related by Dr. Lloyd, in a paper upon pneumothorax, contributed by Dr. Hughes to the eighth volume of the *Guy's Hospital Reports*. Sometimes gas is generated within the sac, from the decomposition of effused liquids, or of a gangrenous lung; and in such cases the gas has a strong odour, like that of sulphuretted hydrogen: sometimes, again, gas is said to be secreted from the membrane itself. All these events are, however, uncommon. When air, from whatever source, is shut up in the cavity of the pleura, and goes on accumulating there, it will compress the lung, just as certainly and effectually as if there were a liquid extravasated. And such compression, if suddenly brought about, may cause speedy death by apnoea: and this is more apt to occur from a scratch of the pulmonary pleura by the rough edge of a fractured rib, than from any other cause.

As to the *treatment* of pleurisy, you will have anticipated that in the outset of the disease we must have recourse to the lancet. I have stated, more than once, that blood-letting *tells* more, and is better borne, in inflammation of serous membranes, than in any other case. If you see the patient while the stitch in the side, and the restrained and cautious respiration are present, you will bleed him, in the upright posture, from a large orifice, until the pain is relieved, and he can draw a full breath again with ease and satisfaction; or until he is about to faint. And if the pain and catch in the breathing should return, and the pulse continue firm and hard, you will repeat the blood-letting; or cover the painful side with leeches; or abstract blood by the cupping-glass and scarificator. It is best to bleed fearlessly at first; and in proportion as you do so, the chance will be diminished of a repetition of the blood-letting being needed. The blood drawn in pleuritis is always deeply buffed and cupped.

Observe that I am here speaking of acute pleurisy, occurring in a person previously sound and healthy. If there have been any chest symptoms before, if there be any suspicion of tubercle, even if the patient be delicate, or of a consumptive family, or very young, or very old, you will do well to restrict yourselves to *leeches*.

Tartar emetic, which is so useful when the mucous membrane of the air-passages is inflamed, is *not* adapted to inflammation of the pleura. On the other hand, mercury, from its well-known power to check the effusion of coagulable lymph, is *especially* indicated. Of course it is to be given with a view to its specific effect on the system: *i. e.*, in equal doses, repeated at frequent and equal intervals, and guarded by a small quantity of opium. And in very severe cases, or when the internal employment of mercury is in any way contra-indicated, recourse must be had to inunction of the linimentum hydrargyri, or of the strong mercurial ointment.

By the early and vigorous adoption of these measures, the inflammation may generally be subdued in no long time. If, though the fever diminish, there still be pain in any part of the chest, leeches may be again applied, or the part may be covered with a blister. I do not think a blister does any good,—on the contrary, it is likely, by the additional irritation it causes, to do harm—while the inflammation is yet recent and active.

But though pain may have ceased, and no fever remains, and the patient is not conscious of much dyspnoea, there may be, and there often *will* be evidence, not to be mistaken, of effusion into the cavity of the pleura. Dulness, I mean, on percussion, bronchial respiration, ægophony; and the object of our treatment is now to get rid of the fluid. We seek to do so by keeping the patient on low diet. The more (says Broussais, with some quaintness), the more a patient eats, the sooner he will die. We pursue the same object by keeping his gums tender with mercury; by applying blisters one after another to the affected side; and by purgatives and diuretics. By keeping the vessels empty we facilitate, as much as in us lies, the absorption of the liquid contents of the pleura. A very good form of diuretic for this state of matters is a combination of squills, digitalis, and mercury. Half a grain of digitalis, one grain of squills, and three or five grains of blue pill, repeated and continued according to the state of the mouth.

Under this kind of treatment the effused fluid will often be completely removed; and the chest restored to its former state. I last week dismissed a lad from the hospital in whom all this was accomplished.

But in other cases, though the fever and the inflammation are at an end, and absorption of the liquid takes place, the parts within the thorax do *not* revert to their original condition. This we know by that shrinking of its dimensions on the side affected, which was described in the last lecture. This shrinking and narrowing is the *necessary consequence* of the absorption of the liquid, *unless* the compressed lung dilates again in proportion as the fluid is taken up. In many cases of this kind the lung *cannot* rise; being bound down by thick and firm false membranes: and then the deformity is irremediable, and lasts for life. If the lung be completely emptied of air, and enveloped by strong bands of lymph, so that it is permanently unable to admit air again—in that case, as the bony framework of the thorax can yield to a certain extent only, there will always remain, I presume, some liquid in the pleural cavity. If, again, the lung recover a part of its lost volume, and *meet* the contracting parietes of the chest, adhesion may take place; and the cavity of the pleura be obliterated by thick layers of false membrane. And other changes are apt to arise in

the lymph which is adherent to the pleura in these cases of imperfect repair. Sometimes tubercles form in it. Sometimes ossific matter is deposited. I show you a fine specimen of this kind of ossification of the pleura. There is yet another supposable case: the investing adventitious membrane may be thin and weak, and yielding; and though the lung may not expand to its full dimensions at first, it may gradually force its way against the binding power of the coagulable lymph, and then the external configuration of the chest may be restored, and the symmetry between the two sides return. That this sometimes takes place I cannot doubt: but I have only met with two cases in which the dwindling of the side was *entirely* recovered from. In May, 1834, I was asked to see a child four years old, who had had cough, and had wasted to mere skin and bone, after scarlet fever. I found the whole of the right side of the chest perfectly dull on percussion, and no respiration could be heard on that side. He was taken by his parents into the country, and I did not see him again for some weeks. He then had ceased to cough, and, in a great measure, had regained his strength; but he presented on the side which had been dull, as marked and complete an example as I ever saw of that sinking in of the ribs, with flattening and contraction of the chest, and depression of the shoulder, which denotes bygone pleurisy, and diminished bulk of the lung. About a year from the occurrence of the original disease his father brought him to my house, that I might see the change which had again taken place. The boy was plump and rosy, and in perfect health; the right side of the chest was as full and round as the other: the symmetry of the two sides was completely restored; the breathing natural and perfect; and the sound on percussion hollow. His father, to whom the former shrunk state of the side had been pointed out, told me that he had watched, with deep interest, the process of recovery, and that it had been very *gradual*. The only other example of perfectly regained symmetry that I have seen, occurred in the person of an adult man. I shall tell you some particulars of this remarkable case presently.

There are yet other cases in which the effusion continues and increases, and the side, instead of shrinking, enlarges; the functions of the lung on that side are entirely abolished; nay, the use of the remaining lung is greatly interfered with, by the pushing over of the mediastinum; and the patient is in imminent danger of suffocation. In such cases, whether the effusion has taken place rapidly or slowly—whether the disease has been acute or chronic pleurisy—we must relieve the oppressed lung by *letting the fluid out*—by tapping the thorax; and the sooner that is done, when such a state of things exists, the better.

The operation is not difficult, nor formidable; but a mistake in the diagnosis may be *very* formidable. I have heard of two instances, one in Scotland, and one in this town, in which the operation of paracentesis thoracis was determined on, to relieve the oppression caused by empyema: but the opening was made on the wrong side; and the patient in three minutes was, in each case, a corpse. There was effusion, which had already put a stop to the play of one lung; and upon air being freely admitted to the surface of the other, it collapsed also, and immediate suffocation took place. I do not mention these mishaps to deter you from performing the operation. They both took place some years ago. Such a mistake would be unpardonable now. But I mention them to show the necessity of our being sure of our ground before we proceed to open the thorax of a living person. A surgeon once told me that, with the sanction, and at the suggestion of a physician, who understands auscultation exceedingly well I believe, he passed a trocar into the chest of a patient; but no fluid followed, to the no small mortification of the physician. This proved to be a case of malignant disease of the lung; and fluid was let out afterwards by puncturing the thorax in another place, and much relief afforded; although of course the disease proved ultimately fatal. The surgeon informed me that he had suspected the true nature of the case, from observing a livid protrusion in front; which was, in fact, the specific disease making its way through.

You will take care, then, to survey the chest narrowly before you plunge a trocar into it. If you see by your eye, and ascertain by measurement, that one side is larger than the other; if the intercostal depressions be effaced on that side; if the whole surface afford a dull sound when percussed; if the side do not move at all, or scarcely move during respiration; if no vibration can be felt on that side when the patient speaks; if no breathing can be heard in the corresponding lung; if the heart be

found beating in an unnatural place, down towards the left hypochondrium, or in the other direction on the right of the sternum; and if, at the same time, the other side of the chest shall move freely, sound resonantly, communicate a thrill to the hand while the patient converses, and be full of *puerile* respiration; then you may be sure that the larger side is distended with fluid.

But it does not follow that you should, therefore, open that side. The propriety of doing so will depend upon circumstances.

There are two objects for which the operation of paracentesis thoracis may be contemplated; and these it is well to keep distinct. It may be curative in its intention, or merely palliative. In cases of simple pleurisy it may be adopted with the view of saving life, and restoring health. Again, when pleuritic effusion is complicated with other and mortal changes, the operation may sometimes be resorted to for the purpose of relieving urgent distress, and of prolonging, it may be, a doomed existence.

In simple pleurisy it ought never, in my judgment, to be performed unless the life of the patient is, or seems to be, in jeopardy, from the continued presence of the liquid within the thorax.

Now life is plainly in jeopardy when the vital functions of the lungs, or of the heart, are greatly hindered; when symptoms present themselves of approaching death by apnoea, or by syncope. If we discover no cause for those symptoms, except the increasing pressure of liquid, or of air, pent up in the pleura, we are warranted in ascribing them to such pressure, and bound to act upon that persuasion. Whenever, with the physical signs of abundant effusion, we have great hurry and distress of breathing; an anxious and livid aspect; a tendency to delirium—or extreme faintness, and a vanishing pulse—there is no time to be lost: it is our duty to propose and to urge the mechanical removal of the pressure which must else be fatal.

Again, when the patient, without suffering much dyspnoea while he lies quiet, is yet evidently losing ground from day to day, and early death by asthenia appears to be inevitable, without the operation; and when all other means for getting rid of the imprisoned liquid have failed; and when no other condition of disease, or of advanced age, exists to account for the progressive sinking; then, also, in my opinion, the patient should not be denied the *chance* which the operation may afford.

Thirdly, whenever (no matter how we ascertain the fact) the effused liquid consists of *pus*, it should be let out.

In either of these three predicaments of simple pleurisy, and in no other, should we be justified (as I think) in making an opening into the living thorax.

But I wish to be understood as giving you simply the impression which my own experience has made upon my own mind. I know that some practitioners recommend the early employment of the trocar; while (they say) the false membranes, which are apt to prevent the compressed lung from expanding again, are yet tender and unorganized. But surely we should risk much, and gain nothing, by admitting air into the pleura while the inflammation is still in progress. Most cases of mere pleurisy with effusion do well. The mortality from uncomplicated pleurisy is exceedingly small. It would, I fear, be vastly augmented if every patient having manifest effusion were to be tapped. The danger of the operation is this;—that it may, and probably will, induce suppuration, or cause the effused liquid to become putrid. Generally the effusion consists of serous fluid, which is at length spontaneously reabsorbed; the lung expands again, or the walls of the chest shrink inwards: and the ultimate state of such a patient is as good as it probably would have been after a successful tapping.

To make assurance doubly sure, it is always right, before proceeding to the operation of paracentesis, to adopt the expedient first suggested and used, I believe, by Dr. Thomas Davies, of trying the chest by means of a grooved needle; making a tentative exploration of the nature of its contents in that manner. The passage of this little instrument—like the dismissal of a pilot balloon—affords information which is useful in guiding the particulars of the subsequent process. As a mere diagnostic measure it is highly valuable. It not only ascertains that there really is liquid within the pleura, but it discovers the kind and quality, and exact place of the liquid. If it be serous, it will flow readily along the groove, and trickle down the patient's side. If it be puriform and thick, it will not exude so freely, but a drop or two will probably be visible at the external orifice: and when the needle is withdrawn, its groove

will be found to contain pus. In the former case it is possible that there may be no false membranes; in the latter they are likely to be thick. You would use a larger trocar to evacuate the thicker fluid.

The puncture thus made is quite harmless; and inflicts very trifling pain. Dr. Davies gives this useful piece of advice in respect to the trocar, that its point should be *sharp*: for otherwise, after the serous membrane has been penetrated, if there happen to be thick tough layers of coagulable lymph, not very closely attached to the costal pleura, they may be driven before the instrument, and so the liquid will not be reached, but the operator will be perplexed and baffled.

Connected with the operation itself there are some questions concerning which medical opinions and medical practice are not yet settled. I do not pretend to decide these questions: yet I cannot pass them by. I must point them out to you; and I shall, at the same time, state what my own observation has suggested in regard to them.

1st. Should all the liquid be let out at once?

Some say yes: some say no. If we appeal to experience on this point, we obtain no satisfactory answer. I have known patients get rapidly and perfectly well, after as complete an evacuation of the liquid as was possible. On the other hand, I have heard of speedy recovery when, by a sort of accident, very little had been withdrawn: enough to relieve the pressing distress: but much less than the operator intended.

We must try the matter, therefore, by our reason.

I think it very probable that when the serous membrane is *stretched* by the pressure of its contents, its natural absorbing power may be lessened. But we have no reason to suppose that the mere relief of this tension will *often* suffice to renew the process of absorption, and to enable the flattened lung to re-expand.

The theoretic objection to the thorough emptying of the thorax in such cases is (I conceive), that the introduction of air is likely to be hurtful, by converting the adhesive into the suppurative form of inflammation, and by promoting decomposition of the extravasated fluids. No doubt there is this risk; but, in general, if you wish to empty the pleura, it cannot be avoided. Unless the lung rises freely at once, the liquid *cannot* all, nor even much of it, come out, without air getting in. But the mere admission of air into the pleura does not necessarily *create* inflammation of the membrane. This we know from what happens sometimes in emphysema produced by a fractured rib. In the only instance of *pure* pneumothorax which I ever saw, the sac of the pleura had become half filled with air, through a very minute opening in the pulmonary membrane, communicating with the air-passages. There was *no* inflammation of the pleura in that case. Except that it was preternaturally *dry*, it seemed perfectly healthy. Neither does the access of air necessarily superinduce suppuration in the membrane already inflamed. Certainly, if pus follow the passage of the instrument, as much should be removed as we can get. And, for my own part, I should take away as much as would come, whenever the inclosed liquid proved to be serous. If much be left behind, a repetition of the operation will commonly be required. Air disappears spontaneously with far greater readiness than serum; and opposes less resistance, while it remains, to the gradual re-expansion of the compressed lung. Should you desire however to evacuate, as some advise, so much of the liquid, and no more, as the expansion of the lung, and the elastic resilience of the thoracic parietes suffice to press out, without admitting air—that object may be insured by adopting a very simple contrivance of Professor Schuh's, of Vienna, which was recently shown me by Mr. Spencer Wells. It consists of a sort of small trough, which is readily fitted to the end of the canula after the trocar has been withdrawn, and which is provided with a valve that prevents any reflux of liquid, or passage of air, from the trough into the canula. In this way the entrance of air into the pleural sac may be effectually obviated.

2ndly. Is the orifice to be healed up, or to be kept open?

Here, also, practical men differ. I should say, if pus come out, by all means make the aperture large, and keep it open; and inasmuch as detention of the pus would be injurious, and the depending point is difficult to hit, and the orifice is apt to clog, I would do more than leave it open: I would draw the puriform fluid off twice a day by a syphon.

If serum be let out, by all means close and heal the wound. Then, if all go on well, our object is achieved. But should the condition of the patient fail to improve; should hectic fever, after a day or two, set in or even continue; should much constitutional distress or disturbance arise;—under such circumstances I would reopen the wound. There *was* mere serum, or liquor sanguinis; there now *is*, in all probability, puriform matter pent up in the pleura; and even stinking and poisonous gases.

On six occasions I have myself witnessed the evacuation, by puncture, from the human pleura, of a clear transparent liquid. Some of the patients were under my own charge, some under the charge of others. Of these six patients one died the day after the operation; I can scarcely say why. She was an extremely timid and susceptible young woman; and I am inclined to attribute her death to the shock produced, by apprehension of the operation, upon her sensitive nervous system. Two others recovered forthwith, and perfectly. The wound presently healed in the three remaining cases also; but in one of the three it soon broke out again, and a quantity of healthy pus was discharged daily. After some time, the expedient of keeping the cavity free from accumulated pus by the use of a syphon was resorted to. Under this plan the discharge became gradually less and less, and at the end of many months it finally ceased. The side at one time was so shrunk in, and the ribs were drawn so closely together, that the introduction of the syphon became difficult. Ultimately this deformity was removed, and the symmetry of the chest restored. When the patient presented himself to me, after two years' residence upon the Continent, I found both sides of his thorax alike in shape and dimensions, and the pulmonary expansion everywhere audible and natural. This gentleman continues now (1857) in the enjoyment of perfect and vigorous health. This was the case to which I just now referred as forming the second example that I have seen of the complete re-expansion of a side contracted after pleuritic effusion. When the operation was performed, eighteen years ago, the patient was gasping, livid, faint, and on the brink of perishing by suffocation; it gave him instant and lasting relief. I have been told of a man who, for the last fifteen years, has had a similar thoracic fistula; and who has, nevertheless, during nearly the whole of that period, been actively engaged in the various labours of a farm-servant.

I have still two of the six patients to account for. They were both much relieved by the operation for a while; but after a few days they again fell off; and after many more days of gradual sinking and distress, they died. The cavity of the pleura contained, in both cases, much puriform liquid, and a quantity of most offensive gas, consisting in great part, as I judged from its odour, of sulphuretted hydrogen. I have since thought that both these patients would have had a much better chance for life, if this corrupt and corrupting mass had been duly removed.

Again, I have twice seen *pus* let out, by the *primary* puncture of the chest. One of these two patients sank, exhausted, some months after the opening, which never healed, was made. The empyema of the other had been occasioned by fracture of a rib. The discharge continued for a short time, then ceased, the orifice closed, and the lad got well.

This constitutes the amount, or nearly so, of my personal experience of the operation of paracentesis thoracis. You will see, in the statement I have been making, the grounds of those opinions which I have formed and expressed respecting it. A full and final solution of the grave and difficult questions that it involves would require a much wider field of observation than any one individual is likely to command. Dr. Thomas Davies has published a tabular account of the several cases of operation which he had then superintended. In sixteen cases of empyema, so treated, there were twelve recoveries; that is, the operation was successful in three-fourths of the whole number of cases: a very encouraging result. In three of the less fortunate cases, the lung could not expand after the evacuation of the fluid, in consequence of the thickness of the false membranes covering it.

The value of Dr. Davies' table would have been greater, if it had shown in each case the time, after the commencement of the disease, at which the operation was performed; the symptoms that called for its performance; the nature of the liquid evacuated; and whether the orifice made by the trocar was closed or not.

The quantity of liquid which the distended pleura is capable of holding is enor-

mous. I have seen upwards of a gallon let out at once. Dr. Townsend mentions the case of a patient of Dr. Croker's, in Dublin, from whose left pleura Mr. Crampton drew off the almost incredible quantity of fourteen imperial pints of pus. Of course this could not have accumulated there without making injurious pressure in all directions: upon the ribs, upon the heart and mediastinum, upon the diaphragm, and the abdominal viscera beneath it. It is interesting to know with what rapidity the capacity of the diseased side of the thorax may, in favourable cases, diminish. The same writer gives the history of a boy, twelve years old, in whom the circumference of the diseased side was sixteen inches and six lines, while that of the sound side was fourteen inches and one line. Nine days after the operation the circumference of the diseased side had decreased nearly three inches: it measured thirteen inches and nine lines; that is, rather less than the circumference of the healthy side. The side had shrunk somewhat within its natural size. This is common in such cases.

There is yet a third question of some importance. Whereabouts should the opening be made?

If any soft inelastic tumour have appeared, marking a tendency in the effused liquid to make its own way outwards, that tumour should be punctured without loss of time; for there will then be *no* chance of the reabsorption of the pus; and if the swelling be left to itself, troublesome, burrowing sinuses will be apt to form in the thoracic and abdominal parietes. As we have no choice in such a case about the place where the aperture is to be made, authors have termed the operation the *operation of necessity*; and they distinguish the case in which the surgeon is at liberty to introduce his trocar wherever he pleases; they say that then the *operation of election* takes place. Now the question is, what spot is the best for this operation of election?

If there be any part of the surface which is resonant on percussion, or which affords any sound of respiration, that part must be avoided. It is probable that the lung, in that place, is fastened by adhesions to the costal pleura. Of course you would not thrust in a trocar where you saw or felt that the heart was beating.

The object to be kept in view is that of making the opening in the situation which will allow the most free and perfect vent for the liquid. The intercostal space between the sixth and seventh true ribs, where the digitations of the serratus major meet those of the obliquus externus muscle, is the place usually recommended. Laennec prefers the space between the fifth and sixth ribs. He observes that, on the right side, an enlarged liver frequently reaches as high as the sixth, or even as the fifth rib. When the diaphragm is pushed as high as this (and I believe that Dr. Edwin Harrison, who had paid much attention to this point, could have told you that it is often pushed up even higher) there is an obvious risk of penetrating it with the trocar. In fact, Laennec committed that error himself. After making an incision between the fifth and sixth ribs, he thrust the instrument, as he supposed, into the thorax; and was a good deal surprised to find that no gush of liquid followed its introduction. The patient died; and dissection showed that the trocar had entered the cavity of the abdomen after transfixing the diaphragm, which, having been forced upwards by a large liver, had contracted firm adhesions to the seventh rib. I have myself witnessed a similar mischance, on the other side of the chest. The integuments of the side were œdematous; and it was thought that a little serum issued upon the passage of the grooved needle. The serum must have come from the infiltrated areolar tissue. No liquid was evacuated by the trocar. The patient died a day or two afterwards of peritonitis. The instrument had perforated the diaphragm, and entered the spleen, which was unusually large.

I am tempted to relate the particulars of one of the prosperous cases that I briefly adverted to before. It occurred in a lad of nineteen; a patient of my colleague's, Dr. Wilson. On his admission into the hospital he bore all the marks of copious effusion into the left pleura; the side enlarged, and motionless, and dull on percussion; the intercostal spaces tense, and level with the ribs; the heart beating to the right of the sternum: respiration puerile on the right side, inaudible on the left; urgent dyspnoea; a tendency to coma, marked by drowsiness and blueness of the cheeks and lips. In short, the boy was on the very verge of suffocation. He had been ill about a month; and had been bled, and cupped, and brought under the specific influence of mercury. Dr. Wilson judiciously directed that the liquid should be let out.

A grooved needle was first passed between the fifth and sixth ribs; and some serum

following the puncture, a trocar was then introduced by Mr. Tuson, and nine pints of a clear fluid were drawn off. During the operation the patient became faintish at times, and then the orifice of the canula was stopped for a moment by the finger. The immediate effect of the tapping was most interesting and gratifying. Even while the liquid was flowing, the heart was observed gradually to move over from beneath the right mamma towards its natural situation; and his difficulty of breathing was signally relieved. At the beginning of the operation he respired fifty times in a minute; at its conclusion thirty-eight times only. A good deal of air entered while the liquid was escaping: and for some days after the operation a splashing sound was audible on succussion of the chest; and one part of that side was unnaturally resonant, when struck, and another part unnaturally dull; and whatever was the posture of the patient, the hollow sound was uppermost, and the dull sound undermost; and when he sat up and spoke, or coughed, a brazen resonance was heard by the ear applied to the scapular region. This lad got quite well, without the recurrence of a single bad symptom. He afterwards presented himself at the hospital; and I understand that the left side was found to be in a very slight degree smaller than the right.

The liquid evacuated in this case was clear and transparent. It separated, on cooling, into three parts; one of quite watery consistence, one more viscid, and a third which constituted a soft, transparent, jelly-like mass of fibrin.

In this instance no injurious consequences resulted from the free admission of air. It may sometimes be necessary to puncture the cavity for *mere* pneumothorax: when, for instance, the pulmonary pleura has been pricked by a fractured rib, and air passes from the lung into the pleural sac faster than it can be absorbed; fast enough to compress the lung, and to threaten death by apnœa. The diagnosis of such a state cannot be difficult. The existence of the fracture, the tympanitic sound yielded by the chest on the injured side, the absence of respiratory murmur in the tympanitic part, and the increasing dyspnœa, all point to the same conclusion. Now a trocar of the smallest size—or even an acupuncture needle—may suffice to give vent to the imprisoned air, which will escape with an audible hissing noise. In some cases it must have existed in very large quantity, for the stream of issuing air has been strong enough to blow out a candle several times in succession; the flame being each time immediately re-lighted.

The same necessity for puncturing the cavity of the pleura from without may arise in cases of pneumothorax depending on specific disease in the lungs; but we cannot regard the operation as *curative* in such cases. Its value is very different from that which experience has shown to belong to it in empyema from acute or chronic (but simple) pleurisy. Yet if it save life for the time, if it prevent impending suffocation, and relieve existing distress, and postpone the fatal event, it is not *without* its value: and it has many times been done, and been followed by very gratifying results; but it has never, that I know of, been followed by entire recovery. Dr. Davies had superintended the operation in nine instances of pneumothorax with effusion; and *all* the patients died from tubercular complications.

There are, indeed, on record, examples of recovery after the operation, when pneumothorax had existed, and under very unpromising circumstances. I should have stated before, that as the pus, in empyema, sometimes finds its way outwardly, penetrating between the ribs, and forming an external swelling, which, if not opened by the scalpel, will at length burst; so it also, sometimes, escapes by making a road into some part of the air-passages, and being expectorated. Now the operation of paracentesis, in such a case, *there being no tubercular disease*, has been successful. Le Dran relates an instance in which he operated for empyema, where “the injection of a small quantity of mel rosarum and barley-water through the wound excited coughing, and part of it was coughed up through the mouth, mixed with pus;” thus clearly proving the existence of a fistulous passage through the lung; notwithstanding which the patient recovered completely. The effusion was probably circumscribed. But you will find other cases of a similar kind referred to by Dr. Townsend, in the *Cyclopædia of Practical Medicine*: and I may briefly mention one which has fallen within my own experience.

A gentleman, twenty-two years of age, had the ordinary symptoms of pleuritic inflammation. There had been no previous evidence of pulmonary disease. After a while he began to expectorate pus of a very offensive odour. The physicians in

attendance upon him came to the conclusion that a circumscribed collection of pus in the pleural cavity had found its way, by ulceration, into the air-passages. The patient was gradually wasting away. Auscultation long failed to disclose the exact seat of the presumed collection of matter. At length, in a small spot between the scapula and the spine, on the affected side, the mingling of air with a liquid was heard during inspiration. As under the existing circumstances life seemed drawing inevitably towards its close, it was determined, in a consultation at which I was asked to assist, that an attempt should be made to let the matter out by puncturing the chest. The place at which the operation was of necessity performed rendered it a difficult one; but Mr. Stanley succeeded in reaching the abscess, and in evacuating nearly two pints of pus. The wound was kept open. From that time the offensive expectoration by the mouth ceased. Gradually the discharge from the back became less, until there was none. The opening healed in three months after the operation, which was performed in May, 1850, seven years ago. The patient recovered perfectly, and is now a healthy man.

Since these lectures were last submitted to the press, I have witnessed several other cases in which paracentesis thoracis was deemed advisable. The subject has also been expressly treated of by Dr. Hughes and by Dr. Hamilton Roe. To those gentlemen the profession is much indebted for having shown with what facility, and with how little risk and pain, the operation may be performed. They have not convinced me of its frequent necessity. In so far as simple pleurisy and its consequences are concerned, my own opinion, after careful reconsideration, remains unchanged. The operation seems more extensively applicable, however, than I had formerly supposed, to other cases, where its object and effect are to alleviate present suffering, and to prolong life which it is unequal to save: cases of pneumothorax, and of serous or puriform effusion connected with malignant or tubercular disease of the lungs. But in mere pleuritic effusion I would not puncture the chest, however devoid of hazard that procedure may be, unless I knew that the contained fluid was pus, or unless there was no prospect of recovery without it. I except here the minor diagnostic puncture with the grooved needle. In one very interesting case, related by Dr. Hughes in the 8th volume of the *Guy's Hospital Reports*, the pneumothorax ceased after—it may be fairly said was cured by—the second evacuation of the affected pleura by tapping the chest. The air must have escaped from a small cavity in the lung, which afterwards rose and became adherent to the costal pleura. The patient died from rapid tubercular disease of the other (the left) lung. The right being removed, and submerged in water, not a bubble of air could be made to escape from it by inflation. Air had been absent from the pleural cavity from July 24th to May the 22d of the following year.

This concludes what I have to say, not only of pleurisy, but also of pneumothorax, and of empyema, which are often treated of as separate and independent disorders. They are more frequently connected with pleurisy than with any other form of disease, and they are almost always *consequences* of disease or of injury. But I believe I have omitted nothing of importance in respect to either of them.

LECTURE LIV.

Pulmonary Hæmorrhage: its varieties; its connexion with pulmonary consumption, and with disease of the heart. Pulmonary Apoplexy. Prognosis in Hæmoptysis. Symptoms. Treatment.

HAVING gone over the *inflammatory* affections of the organ of respiration; having brought before you inflammation of the membrane which *lines the air-passages*, or *bronchitis*; inflammation of the membrane which *invests the lungs*, or *pleurisy*; and inflammation of the *whole substance* of those organs, or *pneumonia*; I proceed next to the subject of *pulmonary hæmorrhage*.

You may remember that, in an early part of the course, I drew your attention to some general facts respecting internal hæmorrhages. I showed you that the blood does sometimes proceed from a ruptured vein or artery, but that it is much oftener poured forth through unbroken surfaces, from a multitude of capillary vessels; and that hæmorrhage of this kind takes place from the *mucous membranes* far more frequently than from any other natural surface of the body. I observed also that such hæmorrhage is almost always preceded by congestion; either by active congestion, which is less common, or by passive and mechanical, which is extremely common: and we speak, accordingly, of active and passive hæmorrhage. Hæmorrhage is also sometimes primary, or idiopathic, and then constitutes the whole disease; while at other times it is merely a symptom, direct or indirect, of some other disorder, in which case we call it secondary.

Now in the lungs we find examples of all these varieties of internal bleeding; but pulmonary hæmorrhage is secondary much more often than it is primary.

In speaking, therefore, of some forms of pulmonary hæmorrhage, I must touch upon certain diseases of which the bleeding is a symptom: but I shall not go further into the consideration of those diseases at present, than may be necessary to elucidate the hæmorrhage. Bleeding from the lungs is a thing of most fearful interest; and it will be useful to take a general view of that phenomenon, whether it be a substantial disease in itself, or merely a sign of other pre-existing diseases.

The blood, then, in pulmonary, as in all other hæmorrhages, may issue through a breach in the walls of some considerable blood-vessel; or it may proceed from innumerable capillaries distributed to the mucous membrane of the lungs: and the latter mode of hæmorrhage is much the more common of the two, although it is the popular belief that the "breaking a blood-vessel in the lungs" is of very frequent occurrence.

The particular vessels injured in the first class of cases, and the nature and origin of the breach made in their sides, are matters of infinite variety. Sometimes the blood is extravasated through apertures, the results of a disorganizing process which has commenced in the coats of the vessels themselves; as when, for example, aneurisms of the thoracic aorta, or of its primary divisions, burst, and pour their contents into the air-tubes. Having pointed out this accidental and well-nigh hopeless form of pulmonary hæmorrhage, I shall postpone any further account of the disease that gives rise to it, to a future lecture.

Sometimes, again, a large blood-vessel is laid open by the encroachment and extension of disease from contiguous structures. Here is represented (Carswell, fasc. vi. plate iii. fig. 5) the perforation of a large branch of the pulmonary artery, and of a neighbouring bronchial tube, by the extension of tubercular ulceration. The blood escaped so abundantly in this case, that the patient was dead in less than a quarter of an hour. And here I show you a preserved specimen of a similar opening made in the pulmonary vein.

It will be necessary that I should anticipate somewhat; and in order to include in one view all that relates to pulmonary hæmorrhage, that I should speak cursorily of its connexion with tubercular phthisis. No one here can be ignorant that in that terrible disease portions of the lung are liable to be hollowed out by the softening and expulsion of tubercular matter, into what are called *vomicæ*. Now seeing that

hæmoptysis occurs very frequently in persons labouring under consumption, and that the expectoration of blood is often copious, and takes place when it is evident that there are tubercular excavations in the lung, it would be very natural for you to suppose that the bleeding in such cases proceeded from large vessels which had been laid open during the softening of the tubercles, or by the subsequent extension of the ulcerating cavities. But in point of fact, this is very *rarely* the case. I shall explain to you hereafter how it happens that this hæmorrhage from the larger vessels is *generally* prevented; still it does *sometimes* happen.

But in a far greater number of instances the blood in hæmoptysis is *poured forth* from the mucous membrane that lines the air-passages. When this surface is examined in the dead body, and immediately after the occurrence of pulmonary hæmorrhage, it is very often found to be perfectly entire, from the commencement of the trachea to the remotest divisions of the bronchial tubes; as far, at least, as minute dissection can follow them. The membrane in these cases is usually red, as in simple bronchitis; but it is sometimes pale, or shows scarcely any traces of vascularity. The former of these appearances results from the continued turgescence of the submucous capillary vessels; the latter is the consequence of their having been completely emptied of blood by the last hæmorrhage. We shall meet with analogous conditions when we come to examine the hæmorrhages that proceed from *other* mucous surfaces; and especially from that of the alimentary canal.

When blood is thus discharged from the mucous membrane of the air-passages, the hæmorrhage may be strictly primary or idiopathic; *i. e.*, it may be independent of any discoverable alteration of texture, either in the mucous surface itself, or in any other part which, by reason of some intelligible connexion of structure or relation, seems capable of influencing the capillary circulation of the membrane. But the occurrence of pulmonary hæmorrhage strictly idiopathic has been more frequently affirmed than proved. Active hæmorrhage from the lungs is stated by systematic writers to be the hæmorrhage of adolescence, as epistaxis is that of childhood. I believe, however, that idiopathic active hæmorrhage from these organs is very rare indeed; unless we may consider as such, certain forms of *vicarious* bleeding, which I shall presently advert to. Andral tells us that in one instance only, in which hæmorrhage from the surface of the air-passages had been the immediate and apparently the sole cause of death, had he ever found the substance of the lungs free from *tubercles*, and perfectly *healthy*. He does not, however, state whether in this one instance the *heart* also was in its natural condition: an important omission, as we shall hereafter perceive. He relates, indeed, as an example of idiopathic hæmoptysis, the case of a young man who suffered profuse hæmorrhage from the lungs on four several occasions, between the ages of twelve and eighteen, without any apparent detriment to his health, which remained excellent. It is consistent, however, with much experience to suppose that crude tubercles might have been scattered through the lungs of this person, and have sufficed, on the application of some exciting cause, to determine the hæmorrhage, although as yet their presence was not indicated by any other sign. Almost every systematic writer quotes, as an example of idiopathic hæmorrhage from the lungs, the story of the Roman governor, mentioned by Pliny, who lived to the age of ninety, though he was afflicted with habitual hæmoptysis. Now the frequent citation of this supposed instance is of itself a sufficient proof that spontaneous pulmonary hæmorrhage is far from being common.

Cæteris paribus, the *disposition* to pulmonary hæmorrhage is increased by whatever tends to diminish the capacity of the thorax, and to compress the lungs, or the heart and great blood-vessels. The mechanical congestion thus produced may become a very intelligible cause of the exudation of blood from the mucous membrane. And it is partly on this principle that we may account for the frequency of hæmoptysis in persons with crooked spines; in tailors, who sit continually in a stooping posture; in young women who lace their stays too tightly; and even in those who labour under dropsy, or other cause of distension of the belly. Hæmoptysis accompanying ascites has been known to cease at once upon the performance of the operation of tapping, and to recur upon the reaccumulation of the dropsical fluid; and this not on one occasion only, but so often and regularly as to preclude all notion of accidental coincidence. There can be little doubt, however, that in this class of cases, or at least in a vast majority of them, the hæmoptysis is mainly to be ascribed to organic disease

of the heart or of the lungs; and that the pressure which precedes and determines the bleeding is simply a *concurrent* cause.

If we cannot properly rank *that* pulmonary hæmorrhage as idiopathic, which is constitutional and vicarious of some other natural or morbid discharge, — and most frequently of all of the menstrual discharge in females, — it may be considered as forming a link of connexion; as lying midway between secondary and primary hæmorrhages. There are a great number of very curious and well-authenticated facts upon record concerning this singular form of hæmorrhage by deviation. I will give you one history of the kind by way of sample; it is related by Pinel, who held that there was no supplemental hæmorrhage more common than the hæmoptysis that is vicarious of menstruation.

A female, 58 years old, born of healthy and robust parents, of strong constitution, of a sanguine and plethoric temperament, and of great sensibility, lived in the Salpêtrière, and was therefore under constant observation, from the age of 14. She enjoyed excellent health till she was 16 years old. In her 16th year the menstrual discharge commenced without mishap or difficulty; but this, her *first* menstruation, was suddenly suppressed, in consequence of the fright and agitation produced by the sight of an epileptic patient in strong convulsions. From that time the catamenia *never* reappeared, nor did any kind of discharge take place from the genital organs; but at the next period, when regular menstruation ought again to have come on, the girl was attacked with violent hæmoptysis. The hæmorrhage was preceded by vague pains, in the uterus and loins, and by other symptoms which frequently announce the catamenia. It lasted two days, during which time the girl expectorated nearly a quart of blood. With one interval only of exception, this female continued to menstruate *through her lungs* at each monthly period, from her 16th to her 58th year, *i. e.*, during 42 years of her life. The coming on of the hæmorrhage was sometimes a little accelerated by strong mental excitement; sometimes a little retarded by causes of a contrary nature. It was suspended during one whole year, without any serious impairment of the general health, or the occurrence of any other hæmorrhage: during this interval, however, the patient suffered most severe headaches. Occasionally the hæmoptysis was complicated with hæmatemesis. The symptoms by which the pulmonary hæmorrhage in this instance was generally preceded or accompanied were the following: — a sensation of weight and uneasiness in the loins and in the situation of the uterus, soon followed by chilliness of the surface, general lassitude, and a feeling of oppression and heat in the chest, with some dyspnœa. The face became red, and she had intense headache. Then she began to have a distinct sensation of pricking, and of a sort of bubbling, in the trachea and about the commencement of the bronchi; then followed sharp cough, and the expectoration of blood, often bright coloured and frothy, sometimes of a darker hue. The duration of the hæmoptysis was generally confined to a single day, and it never exceeded three days. It recurred with tolerable exactness at monthly periods; sometimes the interval was longer, and then the hæmorrhage continued longer, but was less abundant; and upon the whole, about the same quantity of blood was lost on each occasion. This woman continued plump, and otherwise healthy, though liable to some thickness of the breath upon unusual exertion.

Cases of this kind are not at all uncommon; although the vicarious hæmorrhage seldom persists so long and so steadily. They are not usually attended with any peril to life.

It is, however, a melancholy truth, that capillary hæmorrhage from the mucous membrane of the air-passages, is dependent, in a very large proportion of instances, upon incurable disease. The hæmorrhage is secondary; and the disease of which it is symptomatic is usually a fatal disease. And the complaint, of which hæmoptysis is by far the *most frequently* symptomatic, is tubercular phthisis. When the tubercles are found upon dissection to be yet crude and entire, and no breach can be detected in the membrane, then no doubt can be entertained about the source and manner of the bleeding; and even when cavities exist, especially if they are found to contain no blood, it is probable that, in most cases, the hæmorrhage has had a similar origin.

When hæmoptysis is thus actually symptomatic of tubercular disease of the lungs, it is liable to considerable variety in regard to the period of its first occurrence, and the symptoms by which it is succeeded. There are many persons in whom the first

attack of hæmoptysis precedes, even for years, the primary symptoms of unequivocal phthisis. There are others in whom the first attack of hæmoptysis is *immediately* followed by all the signs, which announce the presence of tubercles in the lungs. Many, again, do not spit blood until the tubercles have acquired a considerable degree of development, and the phthisical symptoms have been for some time clearly marked; and occasionally, in these cases, the first hæmorrhage proves fatal. Lastly, it is far from being an uncommon thing to see pulmonary consumption run its whole course, and terminate in death, without having been attended with *any* spitting of blood.

Andral gives the following statement as the result of his own observation, in regard to the relative frequency of these several modes of connexion between hæmoptysis and consumption.

Of the persons whom he had known to die of that disease, one in six never spat blood at all. Three in six (or one-half of the whole number) did not spit blood until the existence of tubercles in the lungs was already made certain by unequivocal symptoms. In the remaining two-sixths the hæmoptysis preceded the other symptoms of tubercular disease, and *seemed* to mark the period of its commencement.

By this comparative statement you will see how very frequently hæmoptysis occurs as one of the symptoms *connected* with tubercular phthisis. Under this physician's observation it happened in five cases out of six. In the experience, however, of M. Louis, the proportion, though very large, is not quite so great as Andral found it. Among eighty-seven instances of consumption, there were fifty-seven, or four in every six, in which hæmoptysis had been present.

It has, however, been made a question, whether the spitting of blood which thus occurs in *connexion* with tubercular phthisis, is always to be considered as indicative of the *existence* already of tubercles in the lungs; or whether it may not sometimes precede, and give occasion to, their formation in those organs. This question has evidently been suggested by those cases (constituting, according to Andral, one third of all that happen) in which the ordinary signs of phthisis *begin* to manifest themselves immediately upon the occurrence of the first hæmoptysis, or within a short time afterwards. Morton, who has noticed this kind of pulmonary hæmorrhage, includes among his *species* of phthisis, the "phthisis ab hæmoptœe;" and Cullen held that *spitting of blood* was *often* the *cause* of pulmonary consumption. It is a very important question, and I shall revert to it again hereafter.

Next to *tubercular* disorganization of the *lungs*, the most frequent source of pulmonary hæmorrhage is to be found in organic diseases of the *heart*. It has been stated by Chomel, Bouillaud, and others, both in this country and abroad, that the disease in these cases is most commonly situated in the *right chambers* of the heart. But certainly this is a mistake. The error has arisen from arguing upon erroneous analogies, instead of attending to matters of fact. However, the statement is just as little supported by reason as it is by the result of general experience. The only alteration in the right cavities of the heart which we could suppose likely *à priori* to cause pulmonary congestion, and thereby hæmoptysis, would be increased strength and thickness of their muscular parietes: hypertrophy: a morbid condition which is comparatively rare on that side of the heart, and which, perhaps, would not suffice for the production of hæmoptysis, even if it did oftener exist. The direct effect, on the other hand, of any *obstacle* to the free passage of the blood in the right chambers of the heart, would be to gorge the *liver*, and the system of the *vena portæ*; and to prevent the lungs from receiving their due proportion of blood. But any material obstruction existing in the *left auricle* or *ventricle* will impede the return of blood *from* the lungs, lead to its accumulation in those organs, give rise to mechanical congestion, and so dispose strongly to pulmonary hæmorrhage. And, in point of fact, we find that hæmoptysis is very frequently the result of disease in the left side of the heart; and this leads me to speak here of one very remarkable morbid condition of the lungs, which is often directly connected both with pulmonary hæmorrhage and with cardiac disease; though it is not *always*, or *necessarily*, associated with either.

The morbid state to which I allude is far from being infrequent; yet it had been scarcely noticed by or known to pathologists, until Laennec described it under the title of *pulmonary apoplexy*. It appears under two forms. In the one form we find

an uncertain number of hard knobs, or compact masses, situated here and there in the substance of the lungs, chiefly in their lower lobes, and towards their posterior surface. Their size varies from that of a marble to that of a hen's egg. When cut through they are seen to be very exactly circumscribed, the cut surface being more or less circular, of a uniform and very dark colour throughout, and exhibiting a strong contrast with the surrounding tissue. Careful examination shows that these masses are composed of blood that has coagulated in the pulmonary vesicles. Occasionally the pulmonary substance seems broken down, or torn, by the extravasated blood; and in these cases, perhaps, the resemblance between the injury done to the lung, and that which is inflicted on the substance of the brain in *cerebral hæmorrhage*, is tolerably close. Generally, however, there is no such laceration of the pulmonary tissues; but one, or more, of the *lobules* of the lungs, are gorged and crammed with blood, which has been poured out from the surface of the mucous membrane. These lobules, it is well known, have no direct communication with each other; but are isolated (except where they severally open into the bronchial tubes from which they spring) by a distinct investment of areolar tissue; and it is to this peculiarity in *their* structure and disposition, that the exact circumscription of the dark-red indurated masses is to be attributed.

FIG. 58.



Pulmonary apoplexy, occurring in a man aged 53. There were several apoplectic masses, exhibiting a deep purple, almost black hue, and causing an homogeneous solid appearance of the part affected, as shown in the section.

In the other form of pulmonary apoplexy, there are fewer of these solid spots; perhaps one only, large, diffused, occupying sometimes nearly the whole of one lobe, its limits obscurely defined, and its colour gradually deepening to the centre, which is obviously formed by little else than a black clot of blood.

Now the principal *symptom* attending the formation of these masses is *hæmoptysis*; and the principal though not the only *cause* is disease of the heart. The hæmorrhage is often severe and copious in the first or circumscribed form: sometimes slight and scanty, but commonly slow, oozing, and persistent, in the second or uncircumscribed form. The heart-disease is in its left chambers, and very often consists in contraction of the mitral orifice. No example of pulmonary apoplexy, or of pulmonary hæmorrhage, even *apparently* dependent upon hypertrophy of the *right* side of the heart, has ever fallen under my notice.

In truth that morbid condition of the lung which I am now speaking of, has been badly named. The application, by Laennec, of the term *apoplexy* to the *lungs* was singularly unfortunate: for it suggests an analogy between two things, which, though resembling each other in the appearances which they leave behind them in the organ affected, are yet, essentially, unlike. I have shown you, in a previous part of the course, that cerebral hæmorrhage depends almost always upon the giving way of a blood-vessel, in consequence of the morbid brittleness of its coats: while what is called pulmonary apoplexy can very seldom be so caused. The notions which I have been led to form upon this subject differ materially from those which you will find expressed in the works of almost every writer on pulmonary apoplexy. The opinions I entertain were stated several years ago, in some lectures which I was appointed to deliver before the College of Physicians; and I have constantly been in the habit of mentioning them to the pupils at the Middlesex Hospital, and to my medical friends. It is a matter of satisfaction to me to find that they are esteemed to be correct by so sound a pathologist as Sir Robert Carswell, who has alluded to them in one of his fasciculi on the *Elementary Forms of Disease*. Laennec speaks of the pulmonary apoplexy, as if it were the *cause* of the hæmoptysis. But this is surely a very incorrect view of the matter. The partial engorgement, and the hæmoptysis, are not mu-

tually connected with each other as cause and effect, but they are *concurrent* effects of the *same* cause; of that cause which gives rise to the extravasation of the blood in the first instance. A part of the blood so extravasated passes outwards by the trachea and mouth; while a part is forced in the contrary direction, into the ultimate divisions of the bronchi, so as to fill and block up the whole tissue of a single lobule, or of a bunch of contiguous lobules, and thus arises the *circumscribed* variety. Andral conceives that the sanguine effusion *takes place* in the ultimate air-cells; and he applies to this form of disease the term *pneumo-hæmorrhage*, to distinguish it from ordinary hæmoptysis, which he calls *broncho-hæmorrhage*; and this I believe to be the true pathology of the *uncircumscribed* variety. But it seems to me vastly more probable that in the other form of the complaint the seat of the effusion is in one or more of the larger branches of the air-tubes; and that the blood, a part of it at least, is driven backwards into certain of the pulmonary lobules, by the convulsive efforts to respire which the patient makes, when threatened with suffocation by the copious expulsion of blood, or by a paroxysm of cough and extreme dyspnoea: especially if the blood be poured out from the membrane while the chest is in the state of *expiration*. It is easy to understand how certain portions of the lungs, without undergoing any actual change of texture, may in this manner be so choked up, and crammed with blood, which afterwards coagulates, as to preclude any subsequent admission of *air*.

This view of the formation of circumscribed pulmonary apoplexy, affords an easy explanation of some of the phenomena attending it, which it would be difficult to account for on any other supposition. I mean, first, the occurrence of *several* of the clots, or masses of blood, in different, and sometimes in distant, parts of the lung at the same time; and secondly, the exact manner in which they are commonly bounded and limited to certain lobules. And if (as is sometimes, though seldom, the case) even the texture of the lung be lacerated, it is easier to conceive that this may happen in consequence of the violence of regurgitation during the struggle of impending suffocation, than that the mere impulsion of a thickened muscle at the centre of the circulation should be capable of driving the blood through the walls of an artery with sufficient force, to tear and break down the substance of the lung around it.

In the diffused or uncircumscribed form of pulmonary apoplexy, the congested lung is not relieved by a sudden and copious gush of hæmorrhage, but the impeded and stagnating blood oozes slowly from the vessels containing it into the neighbouring interstitial and vesicular tissues, clogs up a larger and larger space, and is partly expectorated in separate dark-red sputa, combined with a certain quantity of mucus. A similar condition of the lung sometimes occurs in *purpura hæmorrhagica*, without any mechanical impediment to the passage of the blood into and through the heart.

The belief that the dark-coloured, circumscribed spots seen in the lungs, and spoken of as pulmonary apoplexy, are often, if not always, produced in the manner I have just been describing, was suggested to me by the observation of a case, in which these appearances existed, and in which they *certainly* were so occasioned. I told you, when speaking of cynanche tonsillaris, that I had seen one person, and one only, die in consequence of that complaint; and that his death was occasioned by the laying open of the lingual branch of the carotid artery in the progress of ulceration. The phenomena attending the patient's dissolution were of deep, though of painful interest. I described them to you before. He had been taken out of bed, and laid upon a table in the ward, in the middle of the night, in order that Mr. Mayo might more conveniently place a ligature upon the carotid. Suddenly the bleeding burst forth afresh: and he expired, before our eyes, in the course of two minutes: not from syncope or exhaustion, but evidently suffocated. The blood entered and choked up the trachea, and he had not strength enough left to expel it by coughing. I felt his heart and the artery at his wrist pulsate firmly for some little time after the last attempt to dilate the chest had been made. This you know is what always happens when death takes place from the sudden denial of air to the lungs. We found the upper surface of the glottis covered by a clot of blood. There was blood also in the windpipe; and scattered through the substance of the lungs there were numerous hard, and dark, but not very large masses, precisely resembling those described by Laennec as constituting pulmonary apoplexy. This man had shown no symptoms of any pulmonary complaint; nor was there any morbid appearance in his lungs except

those which resulted from the presence of the blood that had been poured into them through the trachea, and rammed home into some of the air-cells, in his convulsive attempts to breathe. All that I have observed since this case happened, has tended to confirm my belief, that what has been erected into a distinct form of disease, under the objectionable name of *pulmonary apoplexy*, is simply an *accident of pulmonary hæmorrhage*. When hæmoptysis has occurred, to any amount, in *consumption*, it is by no means uncommon to find pulmonary apoplexy after death; and Dr. Latham has mentioned to me, in conversation, one remarkable instance of that disease, strongly corroborative of the doctrine I have been endeavouring to support. A young female patient of his, labouring under confirmed phthisis, was attacked, for the first time, with hæmoptysis. The bleeding was so profuse as to cause almost immediate death by suffocation. Her lungs were found riddled with small tubercular cavities; and each of these little cavities contained a little clot of blood. Surely it is more credible that the blood should have reached each cavity by regurgitation from the larger air-tubes, than that each should have been the seat of an independent hæmorrhage at the same moment. It is by a similar reflux of blood that the appearances are produced which characterize the circumscribed form of pulmonary apoplexy.

Upon the whole, the occurrence of hæmoptysis, considered in reference to the probable duration of life in those who are the subjects of it, is of melancholy omen.

I have long arrived at this conclusion:—that if from any given number of persons who have been known to spit blood, we subtract those in whom that symptom was connected with irregularity in the uterine functions, there will remain but few in whom the hæmoptysis did not depend upon disease, incurable and progressive in its nature, of the lungs, or of the heart; and that if we still further subtract those persons in whom the hæmorrhage was symptomatic of cardiac disease, there will be very few indeed left, in whose lungs the existence of tubercles may not be confidently predicated.

Among these few may be reckoned persons who have suffered hæmoptysis dependent upon the detachment and expulsion of “bronchial polypi;” and who exhibit no other indication of cardiac or of pulmonary disease.

You will, of course, understand that I do not include in this estimate of hæmoptysis as a prognostic symptom, those cases in which (as in simple bronchitis) the expectoration is merely *streaked* with blood:—nor those in which small quantities of blood are intimately combined and amalgamated with the bronchial mucus, and form the rust-coloured sputa so indicative of the presence of pneumonia:—nor those in which the hæmorrhage is a consequence of mechanical injury to the chest.

Of those individuals whom Andral had known to spit blood at some period or other of their lives, there was only one in five whom he did not also *know* to have tubercular phthisis. On the other hand, Louis states that for upwards of fifteen years he asked all the patients who came before him, in the practice of a large hospital, and who were *not* affected with phthisis, whether they had ever spat blood; and the answer was always in the negative, excepting only a few instances in which the patients had received violent blows upon the thorax; and the cases of females in whom the menstrual discharge had been suddenly suppressed.

The quantity of blood which is brought up in different cases of pulmonary hæmorrhage, is extremely variable. Sometimes it is so copious and overwhelming that either the patient dies suffocated, or he dies of syncope, outright; but this is not very common. Sometimes, on the other hand, a small quantity of blood finds its way into the mouth, the patient scarcely knows how. And between these two extremes there is every gradation in respect of quantity.

“When blood is thrown out by the mouth (says Cullen), it is not always easy to determine from what internal part it proceeds; whether from the internal surface of the mouth itself, from the fauces, or adjoining cavities of the nose, from the stomach, or from the lungs. It is, however, very necessary to distinguish the different cases.”

Now the diagnosis between hæmorrhage from the lungs and hæmorrhage from the stomach, in other words, between *hæmoptysis* and *hæmatemesis*, I shall not enter upon, until I have described the latter disease. And the diagnosis between hæmorrhage from the fauces or cavity of the mouth, and hæmorrhage from the lungs, can never be very difficult, if once the doubt suggest itself, and the necessary examination

of the mouth be made. And I would advise you not to omit that inspection. I could tell you of cases in which the neglect of this simple precaution has led to needless activity of treatment, and to the ultimate discredit and disadvantage of the practitioner. Blood may ooze into the mouth from spongy gums, or drip from the posterior nasal orifices, and be at length spat out in considerable quantity. Etymologically speaking, these are cases of *hæmoptysis*; but they do not constitute the particular disease or symptom to which alone nosologists have agreed to restrict that term. The sources of the bleeding are manifest as soon as they are carefully looked for.

Patients who are subject to *hæmoptysis* generally know by experience when it is about to happen. It is frequently preceded by some uneasy feeling within the thorax—pain, or a sense of weight, or of heat, or of pricking, beneath the sternum, with anxiety; and they tell you that they taste the blood in their mouths before it comes up, *i. e.* they perceive a saltish taste; and just before the blood appears, a tickling sensation is experienced about the top of the larynx. To relieve this sensation, the patient coughs or hawks a little, and a certain quantity of frothy and florid blood is expectorated.

In a person disposed to pulmonary hæmorrhage, the bleeding may be determined by a variety of causes; which ought to be pointed out to him, in order that he may avoid them. Anything which hurries the circulation will, of course, have a tendency to excite the hæmorrhage. Straining of any kind; great efforts of the body; active exercise; much talking; and more especially public speaking, or singing, or playing on wind instruments. A diminution in the superincumbent weight of the atmosphere is supposed to be, in some cases, sufficient to bring on hæmoptysis; and blood is said to have been forced even from sound lungs, in persons who have ascended very high mountains, where the atmosphere is rare, and where the pressure upon the surface of the body is sensibly lessened. Perhaps the labour of the ascent may have shared in the production of the hæmorrhage; for I am not aware that any such effect has ever occurred to persons who have much more rapidly reached a very great altitude in balloons.

Auscultation and percussion do not stand us in much stead in cases of hæmoptysis, so far as that *symptom itself* is concerned. Indeed, if they were capable of affording us information, it would in most cases be superfluous; for we *see* the blood, and we can generally satisfy ourselves that it comes from the *lungs*.

But pulmonary hæmorrhage may occur without hæmoptysis. In what is called pulmonary apoplexy there is extravasation of blood: and it is not always attended with the expulsion of a portion of the extravasated fluid through the mouth. Laennec and others pretend to say, that when there is blood in the bronchi, they can distinguish, by the peculiar character of the crepitation to which it gives rise, that it is blood, and not mucus; that the bubbles, passing through a thinner liquid, are larger, and break oftener, than those produced by the passage of air through viscid mucus. This distinction is too subtle for me. If, indeed, there have been hæmoptysis, and especially if the hæmoptysis have been sudden and copious, and if, after it, you hear large crepitation in one or more isolated parts of the lung, it will be reasonable to conclude that the air-tubes contain blood in those parts. Those lobules that are plugged up with blood, to the entire exclusion of air, will not, of course, be the seat of any sound during respiration; but this limited absence of sound will be scarcely appreciable unless the infarcted portion lies near the surface of the lung. Around the spot thus rendered solid the sound of crepitation may be audible.

Yet, although the method of auscultation furnishes but little help towards the *detection* of pulmonary hæmorrhage, it will often afford us most precise and valuable information respecting the disease of which the hæmorrhage is a consequence, and an index. Thus, it will frequently teach us, with absolute certainty, that the heart is diseased, or that the lungs are occupied by tubercles. The precise sounds, or deficiencies of sound, which supply the key to this knowledge, I shall describe when I come to those disorders.

Whatever may be the source and organic cause of the hæmoptysis, the bleeding should be stopped as soon as possible; not, however, merely by *suppressing* it, but by relieving the necessity on which it depends. The longer it is suffered to continue, the more likely is it to add to the damage, which already, in too many cases, exists in the lungs. If it lead to the formation of blocks of pulmonary apoplexy, the portions

of lung so filled up, are rendered useless for a long period, and probably for ever. Now as in most cases the hæmorrhage is a capillary hæmorrhage, and depends upon congestion, active or mechanical, we shall stay the hæmorrhage if we remove the congestion.

The congestion may be either mechanical, or active, when it results from the presence of tubercles in the lungs; it is almost always mechanical when it depends upon disease of the heart. The tubercles may press upon the blood-vessels and so lead to mechanical engorgement; or they may provoke by their presence an active determination of blood to those organs, just as we know that they often provoke inflammation, which is congestion and something more; and just as any foreign body lodged in the lung may cause either the one or the other of these conditions.

Frequently there is a distinct febrile movement accompanying the hæmorrhage: the heart beats with increased force and frequency, the cheeks are flushed, and the skin is hot; sometimes the pulse is quite hard, and full, and bounding, and people speak of such a pulse as a *hæmorrhagic* pulse. Now I mentioned in a former part of the course, that hæmorrhage occurring under such circumstances as these often works its own cure; but it is better, when an organ so vital and important as the lung is the seat of the effusion of blood, that *we* should cure the bleeding than that it should cure itself—that we should diminish the congestion with which it is linked, through the safer channels afforded by the veins of the arm, or by the capillary blood-vessels of the exterior of the chest. The patient is to be surrounded with cool fresh air. His head and shoulders should be elevated. He should be restricted to the most meagre diet; and be forbidden to exert himself, or to speak more than is absolutely necessary. His bowels should be freely purged, in the first instance, and then *kept* lax and open, both with the view of deriving (as it is called) from the thorax, and of preventing costiveness and straining. And, in conjunction with these measures, it may be requisite that he should lose blood from the arm, or from the surface of the thorax. The amount and the repetition of the bleeding must be determined by the circumstances of the case, *i. e.* by the cessation or continuance of the hæmorrhage, and especially by the condition of the *pulse*. It would be idle to attempt to lay down precise rules on this matter. We do not bleed, however, so resolutely and perseveringly in hæmoptysis as we are often obliged to do in acute inflammation.

A prejudice has been taken (such, at least, I deem it) against *local blood-letting* in pulmonary hæmorrhage. Inasmuch as leeches applied to the groins in amenorrhœa appear sometimes to restore the catamenia, so they have been thought likely, when applied over the surface of the chest, to attract the blood somehow to that part of the body, and even to *cause* hæmorrhage when none before existed. Now I have so many times taken blood from some part or other of the exterior of the thorax by leeches, or by cupping-glasses, without observing any such effect, of causing hæmoptysis, or of increasing it while already present, that I cannot help considering the objection a fanciful one. Nay, I am quite sure that very great relief to the congested and oppressed lungs may often be obtained by the rapid removal of blood from the peripheral vessels of the chest:—by cupping the patient freely between the shoulders, or upon the breast. In secondary hæmorrhage from the lungs, in that especially which is connected with tubercular disease, topical blood-letting thus performed is more effectual at the time, and far more safe in the end, than venæsection, repeated, as was once the fashion, from day to day, until the hæmoptysis ceases to recur.

When the fever and congestion are abated; or when there has been no constitutional disturbance, and the hæmorrhage has shown a passive character from the beginning, and when the further loss of blood, so far from being curative in its tendency, is likely to be injurious, then we are to employ those remedies which have been found efficacious in restraining and suppressing hæmorrhages.

Now, of the substances which are held to possess more or less of a specific virtue, when taken internally, in arresting the efflux of blood, the sugar of lead, the *plumbi acetate*, enjoys in this country the highest reputation. And it certainly is a very serviceable remedy. Dr. Paris speaks of it as one of the most valuable resources of physic: and says that in respect to its power over internal hæmorrhage there is nothing *simile aut secundum*. He states also that its use is equally safe and manageable. There is, in fact, no doubt of its efficacy: but most other writers use very cautious language in recommending its employment. Physicians have been deterred

from giving it by the fear of its poisonous qualities; by the dread of producing the disease called *colica pictorum*. Cullen observes, that the preparations of lead are certainly powerful in controlling hæmorrhage, but that they are otherwise of a character so pernicious as to forbid their use except in cases of the utmost danger. Of late years this drug has usually been administered in small doses, and guarded by opium; and it is to this combination that Dr. Paris refers when he declares it to be a safe and manageable remedy. More recently, however, a statement has been made by Dr. A. T. Thomson, which must be considered of much importance if general experience shall show it to be well founded. He was led, it seems, by some accidental circumstance, to suspect that lead acted as a poison upon the animal body, only in the shape of its *carbonate*. And the result of a series of experiments upon brutes satisfied him of the correctness of this notion. He holds, that when the acetate of lead produces the well-known symptoms of the painter's colic, it does so in consequence of its being somehow converted, after its reception into the body, *into* the carbonate: that this conversion may be obviated by a very simple expedient; and that the remedy may then be given with perfect safety in large and efficient doses. The expedient is merely that of administering the lead in draughts containing some dilute acetic acid, which prevents the decomposition of the acetate by any carbonic acid that may happen to be present in the intestinal canal. In this way he tells me he has given as much as fifteen grains daily for ten days together, without any inconvenience, and with most excellent effect upon the hæmorrhage. I have often exhibited lead in this manner; and I have never known it give rise to any unpleasant consequences. At any rate this method of administering it deserves further and careful inquiry.

[We have also administered acetate of lead very extensively in cases of hæmoptysis, and in tolerably large doses, continued, at short intervals, for one or two days, and have never known any injurious or even unpleasant effects to be produced by it. As a means of controlling the hæmorrhage from the lungs we know of no remedy so certain in its effects. In the disease under consideration, we have very generally combined a small portion of ipecacuanha and opium with the acetate of lead. — C.]

In slight cases of hæmoptysis, the mineral acids, with or without alum, are often sufficient; or, if there be feverishness, the saline draught with nitre and digitalis. Of the numberless other drugs which have been vaunted as specifics in hæmorrhage I have very little personal experience; at least in pulmonary hæmorrhage. You may sometimes be urged to give a celebrated quack medicine — *Ruspini's styptic*, which has obtained a high repute, and sells at a high price. This nostrum seems for a long while to have baffled analysis. The late Dr. Wollaston told Dr. Maton that it contained no metallic substance; Dr. Thomson has since announced that it mainly consists of a solution of gallic acid in alcohol, diluted with rose-water. But I believe that all the remedial agents which contain gallic acid are more effectual in another form of internal hæmorrhage, to be considered hereafter. Pure gallic acid itself is now easily procured; and it may be administered in doses of from three to six grains every six hours.

Of *mercury*, as a remedy for pulmonary hæmorrhage, my own experience is as yet too scanty to warrant my recommending it on that ground; but its remarkable efficacy has been strongly vouched to me by some excellent judges who have much employed it.

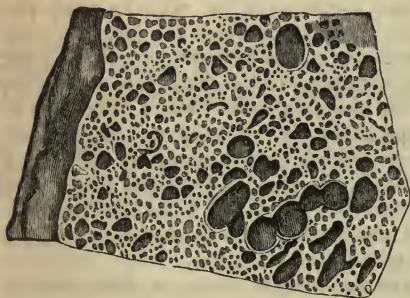
LECTURE LV.

Pulmonary Emphysema; vesicular and interlobular. Anatomical characters of vesicular emphysema; physical signs; general symptoms; causes; treatment. Interlobular Emphysema; its anatomical characters, symptoms, cause, and cure. Œdema of the lungs. Phthisis Pulmonalis.

I HAVE yet one or two morbid conditions of the lungs to consider and to describe, before I go to that which is the most common and most extensively fatal of all its morbid conditions — tubercular phthisis.

There is a state of the lung, or rather there are two or three different states, to which Laennec has applied the name *emphysema*. A very injudicious name it was for him so to impose. We are infinitely indebted to Laennec for the entirely new light which his able researches have thrown upon the morbid anatomy and the pathology of the lungs: but we have to regret that he should have employed, in several instances, a vicious nomenclature. Emphysema is a term that had long been familiar among medical men in a certain sense. It was used to express the inflation of the areolar tissue of the body with air: and surgeons still make much of it as an indication, in cases of fractured rib, that the bone has grazed the pleura, and allowed air to pass into the areolar tissue, and to diffuse itself over the chest and neck, and other parts; so that these parts, when pressed, convey a curious sense of crackling to the finger. But emphysema of the lung, as that term is employed by Laennec, includes dilatation of the air-cells of the lungs, and rupture of the partitions which separate them from each other; and also the infiltration of air into the interlobular areolar tissue, or into the subpleural areolar tissue. In strictness of language these last conditions alone should have been called emphysema of the lung. Laennec has distinguished the two species in this way. To the dilatation of the air-cells, with or without a breach of their partitions, he gives the name of *vesicular emphysema*: “the vesicular (I quote the words of Sir John Forbes’ translation), or pulmonary, properly so called.” Now in truth this is emphysema *improperly* so called. To the infiltration of the areolar tissue in or around the lung with air, *i. e.*, to emphysema of the lung in the old sense of that word, he applies the title of *interlobular emphysema*. We cannot change these denominations now. They have fastened themselves upon medical language. But it is very fit that you should be aware of their inconsistency with the ancient signification of the same word, and have clear notions of what in Laennec’s nomenclature they are intended to express.

FIG. 59.



Portion of emphysematous lung—the cavities are either formed by simply enlarged air-cells, or by the coalescence of groups of cells. —St. Bartholomew’s Museum, xiv. 11.

somewhat more minute attention to it, on those accounts.

Laennec was undoubtedly the first to put *emphysema pulmonum* upon the list of definite and cognizable diseases; to point out its frequency; and to collect its symptoms. But when he affirms that, before his time, the pulmonary change which con-

The change called vesicular emphysema was not *unknown*, as a mere morbid condition, before the time of Laennec: but it had been noticed by very few writers, and *practically* it was wholly *unattended to*. Yet it is extremely common: much more so than you would suppose: and when rightly studied it is of great interest too, in relation to the general pathology of the chest. But it is still so new, and it may so readily escape observation, both in the dead and in the living body, if it be not looked for, that I shall devote a

stitutes the disorder was misunderstood by nearly all those persons who had noticed it, he scarcely does them justice. "All of them (says he) seem to have thought that the derangement in question consisted in the infiltration of the cellular substance of the lungs with air." He inconsistently adds, "Ruysch and Valsalva are the only authors, as far as I know, who have observed in individual cases the dilation of the cells;" and with still greater inconsistency he proceeds to quote, from Morgagni, the following passage, in which this dilatation is very clearly described: "*Sinistri pulmonis lobus superior, quâ claviculam spectabat, vesiculas ex quibus constat mirum in modum auctas habebat; ut nonnullæ avellanæ magnitudinem æquarent; cæteræ multo minores erant.*" You will find the same change noted by Dr. Baillie, in his *Morbid Anatomy*: and by earlier writers than he.

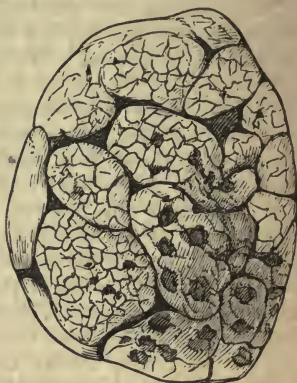
Vesicular emphysema, then (to adopt Laennec's phraseology), consists in dilatation of the air-cells. The enlarged cells become misshapen also in many cases. They vary in magnitude from that of a millet-seed to that of a swan-shot; nay, the cavities may even reach the size of a nutmeg or of a hen's egg: but when they are as big as this—and *a fortiori* when they are still bigger—the distension and vacuity are, no doubt, the result of the union of several air-cells, broken into one by the stretching or destruction of the partitions that naturally divide and isolate them. You may see the dilated vesicles very plainly through the pleura, if you carefully examine the surface of the lung. They appear to the naked eye as the healthy vesicles appear when seen through a magnifying glass. Sometimes all the vesicles belonging to one lobule are enlarged, while those of the adjoining lobules are of the natural size. In that case the emphysematous lobule is conspicuous both by its peculiar colour, and by its protrusion. The surface of the lung is often rendered quite irregular and uneven by projections of this kind. Sometimes one large globular prominence is seen, like a bubble on the water, or like a little bladder springing from a footstalk: but if you examine it closely you will generally find that the footstalk is merely a constriction at the surface, and that there is as large a cavity beyond it, in the lung, as there is without. These bullæ you cannot slip about, by pressure, from one part of the pleura to another.

The unevenness produced by vesicular emphysema upon the outside of the lung is manifest enough, when looked for; but the same condition of the air-cells exists also within, and *there it is not* so readily perceptible. The fluids which the lung contains, obscure all distinction of parts when the organ is cut. The best way of getting a fair view of the dilated cells, as they appear in the substance of the lung, is to inflate the emphysematous portion, by blowing air in at the bronchial trunk which belongs to it, and then tying that trunk to prevent the escape of the air. The inflated lung should be hung up in a current of wind, so that it may quickly dry; and during the drying process it should, from time to time, be re-inflated: for else the included air gets out somehow, and the piece of lung shrinks and shrivels up. When it is quite dry, if a section of it be made with a thin sharp knife, the altered state of the air-cells, some of which are *more* and some *less* dilated, will be very conspicuous.

No part of the lung is exempt from liability to these morbid changes: but generally they are limited to certain portions of the organ, and they are much more common and more pronounced at its loose anterior borders, and near its summit in front, than anywhere else. Both lungs appear to be alike obnoxious to the disease; which seldom affects the one without affecting, in a greater or less degree, the other also.

The parts that are emphysematous are usually paler than the rest, and sometimes they are quite white. In extreme cases the surface of the lung presents a sort of piebald appearance; large patches of it looking as if they had been bleached. This pale colour is oftenest seen towards the free edges of the lung. Sometimes those edges are rounded and thick; sometimes thinner, and folded back; while sometimes the

FIG. 60.



Vesicular emphysema.

margin is blown out, as it were, into an irregular fringe; some of the inflated portions remaining connected with the lung by slender pedicles, and thus forming *appendices* to it of a light yellow colour. I presume that what was thought and called a fringe of fat, garnishing the edges of the lung, in the body of King George IV., was of this kind. At least I have never seen, nor heard of, any other example of fat deposited in those organs. If you hold the emphysematous border between your eye and the light, you perceive that it is translucent: if you prick it with a pin, the puffy part surrounding the puncture sinks down; which shows that the dilated vesicles communicate together.

An emphysematous lung is not only paler, but drier also than ordinary: and for the same reason. It possesses fewer capillary blood-vessels, less blood, and consequently less moisture. It is dry and light, and floats high upon water, like a bladder filled with air.

If you take such a lung out of the body, having its surface embossed with irregular groups of enlarged air-cells—and if you inflate that lung, by blowing into the bronchi—the emphysematous portions will often *seem* to sink in, and flatten, and return to the ordinary level as the whole lung becomes distended. In point of fact, however, these portions remain permanently dilated, and the other parts near the surface, when they are sound and permeable, rise, as the air enters them, until the whole is smooth and even. Air is shut up in the emphysematous portions, which do not subside, as the adjoining portions do, when left to the agency of their proper elasticity. Hence you will see how it is that, when the vesicular emphysema is extensive, so as to occupy nearly the whole of the lung, the lung becomes apparently too big for the case in which it is contained. Not only does it *not subside* when the sternum is raised, and the pressure of the atmosphere is admitted to its external surface; but it even *protrudes*, the moment that the opening is made. When you handle such a lung, it gives a very different sensation to the fingers from that produced by pressing a healthy lung. It feels like a down pillow. It crepitates less; the air is less easily forced out of it, and escapes slowly, with a slight hissing noise.

The nature of the morbid state that I have been describing suggests at once the notion of some physical cause for it. But the mode, and the mechanical conditions, of its production, have been much misunderstood. I must confess that on previous occasions I have given you what I now know to have been an erroneous account of the generation and relations of vesicular emphysema. The permanent dilatation of the pulmonary vesicles is not attributable, as in common with others I formerly supposed, to the accumulation, imprisonment, and distending force of air shut up within the vesicles by obstructions in the air-tubes. Obstructions in the air-tubes cause an emptying of the portions of lung to which they lead—a collapse, and not a distension, of the vesicles. It is to Dr. William Gairdner that we are indebted for more correct views of these structural changes, so opposite in character; and of their mutual relation. He has shown that, in a vast majority of instances, pulmonary emphysema is a consequence of pulmonary collapse. I have always indeed been of opinion that emphysema does not occur as a primary and independent disease; that it is in every case a secondary change, the result of some pre-existing thoracic change. It is never met with alone: and according to Dr. Gairdner's testimony, pulmonary collapse is by far its most frequent concomitant. Take notice, if you please, that these two unnatural and opposite deviations from the healthy state have each its favourite place: collapse affecting chiefly the posterior portions, near the roots of the lungs, while emphysema occupies generally their fore-part, and above all, their anterior borders. It is found, too, that the tubes which belong to the emphysematous parts are rarely obstructed. Air blown into them reaches with facility the dilated vesicles. The tubes of the collapsed parts are clogged with viscid mucus. It can scarcely be doubted that collapse of certain portions of the lungs leads to emphysema of certain other portions. But it is not mere condensation and solidification of the pulmonary substance that produces emphysema elsewhere in the lung. Emphysema is not commonly found coexisting with hepatization alone, nor with solidity arising from the deposit of tubercular matter. What is there, then, peculiar to the condensation of collapse that should render it so fertile a source of subsequent emphysema? It is this—that collapse implies a diminution in the *bulk* of the lung, and that other modes of condensation are not necessarily attended with such diminution. For every portion of lung

closed by collapse there must be a proportionate loss of bulk. But since there can be no vacuum, it follows that, as the thorax expands, more than the standard quantity of air must enter those tubes and vesicles which are not involved in the collapse. And if the quantity exceed a certain amount, dilatation of the vesicles must needs ensue. There may indeed be atrophy and reduced bulk under other forms of condensation, and if so, there may arise a correspondent emphysema. The shrinking up of one part causes undue inflation of other parts, which also may thus be rendered permanently unsound. The old explanation was founded upon the supposed effect of pressure, made during strong expiratory efforts, upon air shut up in certain vesicles. But in truth no partial pressure can be so applied. It is when a portion of the lungs has lost its expansile power, and those organs rise in pursuit of the widening parietes of the chest in inspiration, that some of the permeable portions are apt to be stretched beyond their healthy dimensions by the force of the entering air.

Remember then that vesicular emphysema is a mechanical and complementary (though by no means a compensating) effect of the condensation and diminished bulk of other portions of the lungs; and not a substantial affection in itself. Disease of one kind in one part, brings about disease of an opposite kind in another part of the same organ.

The state of the lung, as discovered after death, being such as I have attempted to describe, you will naturally be inquisitive to know by what signs the existence of a condition so remarkable is revealed during life. First, then, when the emphysematous distension is considerable in amount, and extensive, it is associated with notable alterations in the shape and movements of the chest. The lung having lost much of its elasticity, does not subside as a healthy lung subsides. The act of expiration is arrested and incomplete. Consequently the thorax remains nearly in that position which it assumes after inspiring. It is prominent and rounder on the diseased side; or on both sides, if both lungs be affected; but it is apt to be irregularly prominent, and unsymmetrical; to bulge here and there in correspondence with the bulging of the lung within. The ribs lie less obliquely than they should do, and the chest is, therefore, more cylindrical, or barrel-shaped. The clavicles are ill-defined in such persons. They are so, as you know, in *fat* persons; wherefore this aid to the diagnosis is of most value in those who are *sparse*. In them it is a valuable sign, for it is simple and obvious. The distended lung presses upwards, as well as in other directions, and tends to efface the depressions which naturally exist both above and below the collar-bone. This symptom is the more to be depended on, if it present itself on one side only. It is readily distinguishable, by means to be mentioned immediately, from a similar fullness of the subclavian region produced by a large collection of liquid within the pleura. The manner of breathing is instructive also. The ribs, never receding within their proper limits after expiration, can move but little during inspiration: and the breathing is in a great measure abdominal.

Now all these signs are physical signs. But what, in the second place, are the *auscultatory* physical signs? Why, in the emphysematous regions, which commonly are also the most bulging, percussion yields an unnaturally clear and resonant sound; while auscultation discovers a very indistinct vesicular murmur. The two modes check and explain each the information afforded by the other. Percussion ascertains that there is air beneath the part struck: auscultation ascertains that there is little or no *air in motion* beneath that part. It follows, therefore, that there is stagnant air; air shut up in the enlarged air-cells, or air interposed between the ear and the lung in the cavity of the pleura: air at rest, in fact. I say the respiratory murmur is very feeble. This partly depends upon the limited play of the ribs, partly and chiefly upon the circumstance that the air is imprisoned in the dilated cells. When none enters or leaves them during respiration, no vesicular breathing can be heard: and then we must call in the aid of other considerations to determine whether the air be contained in the cavity of the pleura, or in the emphysematous lung.

Pure pneumothorax is extremely rare. Pneumothorax with liquid effusion is easily recognised by its proper signs. If these be absent, we conclude that the stagnant air occupies the lung, and not the pleura: and this conclusion is strengthened if the unduly resonant part be prominent also. Almost all writers on this subject follow Laennec in stating that *dry* crepitation may be heard in the emphysematous parts. It may be so; but if so, I cannot distinguish it. I mean that I know no crepitation but

that which is moist, and which proceeds from the formation and rupture of bubbles, as air passes through liquids in the bronchial tubes. But some people believe that they can hear a dry crackling, such as may be produced by inflating a portion of dry areolar tissue out of the body, or a dry bladder. You will try this by your own experience. I do not deny that such a sound occurs: I only say that if it do occur, I cannot tell when I hear that dry sound, and when I hear large moist crepitation. But I more than suspect that no such sound is given out by an emphysematous lung; and that the sound heard is really large crepitation in the neighbourhood of the dilated cells: for emphysema is very often accompanied by catarrh: and the sounds in question, authors agree, are not permanently audible.

So much for the physical signs of emphysema pulmonum. The *general signs*, when the change is extensive, are habitual shortness of breath, with occasional paroxysms of extreme dyspnœa; cough, which, however, is far from being a constant symptom; palpitation in most instances as the disease advances; and more or less œdema of the ankles. Usually the appetite remains unimpaired, and the patient does not lose flesh. The disorder is unattended with fever; and is essentially chronic.

The paroxysms of urgent dyspnœa are frequently concurrent with, and apparently excited by, attacks of smart bronchial catarrh; but they sometimes arise without any obvious cause. They are apt to come on suddenly, in the night, and the patient is obliged immediately to sit up, and even to open the doors and windows of his bedroom, that he may breathe more freely. In one word, he undergoes a paroxysm of *asthma*. These attacks become more frequent and more severe as the patient and the disorder grow older. They are attended with much wheezing; and in the lower posterior part of the lungs crepitation is generally audible. At first the palpitation of the heart, and œdema of the feet, abate and cease as the violent dyspnœa goes off: but at length these symptoms become permanent.

We see a reason in the physical condition of the thorax, why the breathing should be more oppressed, and why the paroxysms of orthopnœa should occur more frequently, *in the night*. Whenever the respiration is principally abdominal, it is apt to be embarrassed by the recumbent posture, which throws a part of the weight of the viscera of the belly upon the diaphragm. The horizontal position is always ill borne by these patients; and, for similar reasons, flatulence or fulness of the stomach, however caused, distresses them.

Cough, as I observed before, is an occasional, but by no means an essential accompaniment of vesicular emphysema: the expectoration, when there is any, is thinnish, like gum-water, and full of foam.

The disease of broken-winded horses is pulmonary emphysema; and Sir John Floyer, in his *Treatise of the Asthma*, published in 1698, sets forth, in the quaint language of that olden time, both the alteration which Laennec thought he had been the first to describe, and the manner in which he supposed it to arise. His observations respecting the lungs of horses are equally applicable—and he no doubt intended to apply them—to the human lungs. After speaking of “the broken wind, from the rupture or dilatation of the bladders of the lungs, by which the air is too much retained in the bladders, or their interstices, and thereby produces a permanent flatulent tumour”—and stating that “these horses wheeze much after filling their stomachs by water or food, because that keeps up the diaphragme”—he goes on thus:—“As it happens in external flatulent tumours, they at first go off and return, but at last fix in permanent flatulent tumours; so it is in the flatulent asthma, the frequent nervous inflations induce at last a constant windy tumour or inflation; and it ought to be considered how far holding the breath in hysteric fits, or the violent coughing in long catarrhs, or the great distension of the lungs by an inflammation in the peripneumonia, may strain the bladders and their muscular fibres, and thereby produce the same rupture or dilatation or hernia as happens in the broken-winded. This must be observed by the help of the microscope; and if the air blown into any lobe will not be expelled thence by the natural tone or muscle of the bladders, that the lobe may again subside of itself, ’tis certain some injury is done to the ventiducts; the bladders are either broken, and admit the air into the membranous interstices, or else they are over-distended like a hernia in the peritoneum; and this will produce an inflation of the whole substance of the lungs, and that a continual compression of the

air and blood-vessels, which will produce a constant asthma." Really this is a capital piece of pathology for the seventeenth century.

I have said that emphysema is always (in my opinion) a consequence of pre-existing disease or disorder of the chest. But I ought also to say that some persons believe it to be occasionally a congenital and idiopathic affection. You may often trace back the habitual shortness of breath to the period of infancy. Patients will tell you that from their earliest recollection they have been easily put out of breath: that they were never able to engage heartily, and to the same extent with their companions, in the active sports of childhood. It is a fact, too, that the complaint is traceable, from one generation to another, in certain families. Being thus often present at an early age in children born of emphysematous parents, the emphysema, in such cases, is set down as a vice of the original bodily formation. I am not convinced by this mode of reasoning. The facts upon which it rests show simply that the disorder may run in families, and that the tendency to its ordinary cause is sometimes inherited; that ordinary cause being partial collapse of the lungs. The same low degree of vitality or weakness of fabric, which favoured the production of such collapse in the parent, is repeated in the offspring, and imparts the same predisposition. Young children are moreover very liable to severe attacks of obstructive bronchitis, and therefore to lobular collapse and its consequences. We know that lobular collapse is also a frequent legacy of hooping-cough in the early periods of life. After the cough has ceased, however, the shortness of breath which it leaves behind is easily overlooked, until, with the increase of the emphysema, it forces itself into notice. For when once the morbid process has begun, it tends, slowly often, but surely, to its own augmentation. As the cells dilate, the capillary blood-vessels distributed over their parietes are gradually compressed and emptied: and many of them are, at length, completely obliterated. Hence, not only an exsanguine condition of the pulmonary tissue, but atrophy also of the intervesicular partitions, which become first thin, then tattered and imperfect. In dried specimens of emphysema you see very plainly the remains of the former walls of separation between the vesicles.

It is this interference with the nutrition of the lung which causes vesicular emphysema to be always a *progressive* disease. We see why it is that, speaking generally, the extent of the morbid change is proportioned to the age of the patient: why paroxysms of severe dyspnoea at length supervene; and become more and more frequent and trying. The function of the lung becomes year by year more limited; until it can no longer bear, without a struggle of distress, that further encroachment upon its office and capability which a slight catarrh, rapid movements of the body, a distended abdomen, or even the recumbent posture, may be sufficient to produce.

Vesicular emphysema may arise then, and receive increase, from various disordered conditions that precede or accompany it, and of which it is the effect: from any condition which renders a portion of the lung impermeable by air, and at the same time diminishes its bulk. On the other hand, it has been held to be, and I believe that it often is, itself the cause of subsequent disease, not merely in the lung, but in other parts; and above all, of disease in the right chambers of the heart. The smaller blood-vessels, as I have shown you, are gradually effaced as the dilatation of the air-cells proceeds; the emphysematous lung is evidently in a state of comparative anæmia, and incapable of admitting all the blood which is due to it from the pulmonary artery. In other words, the right side of the heart does not empty itself with its accustomed ease. Hence increased muscular contractions of the right ventricle: and a yielding of its walls to the augmenting pressure of the contained blood. And this embarrassment of the circulation in the right side of the heart is aggravated at those periods when the paroxysms of urgent dyspnoea occur. Now nothing is a more sure cause of anasarca than a permanent dilatation of the right cavities of the heart: and this influence of the unsound lung upon that organ is clearly seen in the palpitations to which such patients are liable, and in the œdema of the feet and ankles which often becomes manifest at the same time.

Dr. William Gairdner has, however, shown good reasons for believing, and has fortified those reasons by a large appeal to the morbid anatomy of the parts concerned, that the dilatation, with or without hypertrophy, of the right chambers of the heart, thus commonly accompanying pulmonary emphysema, is not always—is not even mainly—the consequential effect of that emphysema; but that the pulmonary and

the cardiac change are both of them effects of the same cause, namely, of diminished bulk of the lung, in whatsoever way that may have been brought about. The expansion of the chest in inspiration, which tends to dilate the air-vesicles of such a lung, tends also, and in the same way, to dilate the cavities of the heart.

The main conditions of the production of vesicular emphysema being those already set forth — that is to say, impermeability of part of the lung and reduction of its bulk—we see how that change *may* be produced by *pressure*, when the pressure implies condensation and atrophy of the pulmonary substance: by tumours, therefore, within the thorax, by an enlarged heart, by aneurism of the thoracic aorta, by deformity of the chest from a crooked spine, and even by tight lacing. Certain other alleged causes, enumerated by Laennec, because they fell in with his theory of the production of the disease, such as violent straining efforts, blowing on wind-instruments of music, and the like, have probably no such operation.

The relations between pulmonary emphysema and tubercles of the lungs require also a short notice, for they have been misunderstood. Louis, who has analysed with his accustomed care and exactness a considerable number of cases of emphysema, states that the disease is seldom complicated with tubercles. It has even been affirmed that the two disorders are antagonistic of each other, and incompatible: and this doctrine has led to the absurd project of attempting to prevent phthisis by creating emphysema. The latter exploit would probably be even more difficult to achieve than the former.

The simple truth appears to be that vesicular emphysema bears precisely the same relation to tubercle as to other structural diseases of the lung. The mere deposit of crude tubercular matter solidifies a portion of lung, and impairs its permeability, without reducing its bulk. The mere deposit of crude tubercular matter has, therefore, no tendency to generate emphysema: and when there is no other pulmonary defect, these two morbid conditions seldom meet in the same lung.

But tubercular phthisis is very often, most commonly indeed, attended in its progress with *cavities* in the lung; and the tissue surrounding these cavities is often in a state of atrophy. I am touching by anticipation upon the terrible disease which will next engage our attention. Such cavities may be increasing in size, and ill-defined, with weak and yielding walls; and then they, rather than the sound air-cells, will be likely to enlarge under the expanding force of inspiration, so as to balance the amount of atrophy. Large or numerous cavities, therefore, with flaccid walls, are unfavourable to the development of vesicular emphysema, and may be said to guard the lung against its occurrence. On the other hand, when the cavities are tending to heal and contract, and are surrounded (as under such circumstances they usually are) by dense fibrous tissue, and when they are small and few in number, and much of the lung is sound, the expansion during a full inspiration will then tell upon the air-cells, and favour the production of emphysema.

Once more — I shall have to show you that tubercular deposits may issue in a sort of cure, and leave in the lung depressed cicatrices, involving often cretaceous relics of the tubercles. Here we have manifest loss of bulk, while a large portion of the pulmonary tissue may be sound. Here we have the conditions of the generation of vesicular emphysema, and accordingly it is acknowledged by the best observers that vesicular emphysema is almost universally present in lungs that bear those marks which are considered to be evidences of recovery from phthisis.

Such are the views put forth by Dr. William Gairdner of the relations between tubercle and emphysema of the lung. They are novel, they are interesting, and to the best of my judgment they are true.

I must beg you to bear in mind that emphysema of the lung is one, and a very common, cause of *asthma*. The asthma so arising is less perilous than that which proceeds from certain other organic changes, to be described hereafter. Vesicular emphysema indeed, in its simple uncomplicated form, is seldom attended with much danger. When it proves fatal, it is so in consequence of the superaddition of some other disease. Laennec states very truly, that, of all the varieties of asthma, this is the one which affords the patient the best hope of a long life.

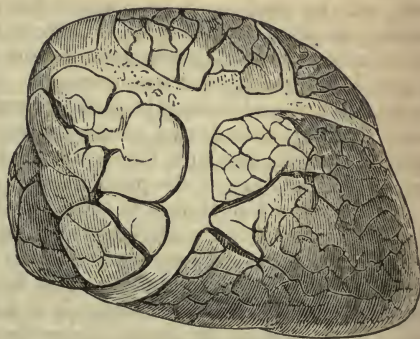
The condition that I have been describing, when once it has fairly been established, can scarcely admit of a cure. We shall do our patients good, not by any treatment addressed to the existing emphysema itself, but by guarding them against those cir-

circumstances which are likely to aggravate it; and by mitigating or removing those other disorders with which the emphysema is apt to be combined. Whatever is calculated to put the patient out of breath is bad for him. It is observed that they who, having emphysema, are obnoxious also to catarrhs, during which the dyspnoea is singularly increased, are much more free from such attacks in the warm weather of summer, than in the winter. This explains the beneficial influence of a judicious change of climate upon such persons, and it points to the necessity of warm clothing in the colder seasons for those who are obliged to remain in this country. The feet especially should be kept dry and warm; and the liability to catarrh may be sometimes diminished by the use of the cold shower-bath, in the way I formerly recommended. During the fits of extreme dyspnoea, you may hear the expiratory wheeze remarkably loud and protracted; and if, withal, you hear any small crepitation, indicative of pneumonic inflammation, you will do well to cup the patient moderately between his shoulders. This will always give relief to loaded lungs, whether there be inflammation present or not. But the great assuager of the dyspnoea in this disorder is opium: and especially opium combined with æther. Half a drachm of Hoffman's anodyne, with a third of a grain of the acetate or muriate of morphia, in camphor julep, will operate like a charm often, in quieting the whole system, and removing the difficulty of breathing. This circumstance would lead us to suppose that the access depended, in part at least, upon a spasmodic state of some of the muscles concerned in respiration. To this question I shall revert when I speak of asthma as a separate disease. At any rate you will find that some such formula as I have just mentioned will stand you in good stead when you have to deal with asthma engrafted on emphysema. And I may add, that this is a case of exception to a rule I formerly laid down. You need not be deterred from giving a full dose of opium by the blueness, which is temporary, of the patient's lips and countenance.

The *interlobular and sub-pleural* emphysema of the lungs is a species of *true* emphysema, the air being contained in the meshes of the common areolar tissue. When it appears on the surface of the lung, it may be distinguished from the bladder-like prominences, which sometimes form there by dilatation of the air-cells, in this way: the bullæ which are situated in the areolar tissue connecting the pleura with the lung, may be made to move hither and thither under pressure; whereas those which result from the protrusion of an enlarged cell or cells cannot be made thus to change their place. This sub-pleural effusion of air is sometimes enormous. I have seen it as large as a hen's egg. Bouillaud mentions a case in which the bladder or pouch was equal to the size of a stomach of ordinary dimensions. It proceeds, I presume, from the rupture of a superficial air-vesicle. Sometimes, as I mentioned before, the *pleura* also gives way, and air is poured into the cavity of the thorax. More commonly the membrane remains entire, and then these large bubbles of air may be seen upon the surface of the lung.

Of interlobular emphysema I can give you but little account except from the observations of others. I have never seen more than one well-marked example of it. The lobules of the lungs cohere together by means of areolar tissue, which is dense and close in the natural state, but which admits of considerable expansion when it is inflated with air. If the emphysema be slight in degree, the lozenge-shaped spaces visible on the surface of the lung are defined by little bubbles of air, that look like beads strung upon a thread. But in extreme cases the lobules are fairly blown asunder by the air; the partitions between them increase in width, and are said to be sometimes as much as an inch broad. They are broadest towards the surface of

FIG. 61.



Interlobular emphysema.

the lung, and narrower towards its roots; and exhibit somewhat of the arrangement seen in the section of an orange, the septa radiating and diverging from a centre. If the areolar tissue could be taken out, there would be left cracks and clefts in the lung. When the interlobular emphysema penetrates to the roots of the lung, the air readily passes into the areolar tissue of the mediastinum, and thence to the subcutaneous tissue of the neck and chest—and then we have the genuine emphysema of authors who wrote prior to Laennec.

There is this material difference between vesicular and interlobular emphysema; that the one is commonly slow and gradual in its formation, the other sudden. The permanent dilatation of the air-vesicles is the work of time. They yield, and lose their elasticity, and break into one another, only by degrees. The interlobular effusion of air may be effected in a few minutes or seconds. It is caused by *violent* straining efforts; such as those made by a woman in childbirth, or by any one who exerts himself to lift a weight which is too much for him. A deep inspiration is taken; then the glottis is voluntarily closed, and a strong expiratory effort is made. Some rupture must take place and form a communication between the air-vesicles and the areolar tissue; but such rupture has never been *traced*, nor is it likely that it should be.

They say that this form of emphysema is revealed also by large dry crepitation; why it should, is not evident. I can only say of that sound, as I said before: it may exist, and it may be distinguishable from large *moist* crepitation, but my ear is not delicate enough to distinguish it: and to speak the truth, I doubt exceedingly whether any such sound really occurs at any time. But do not let my doubts infect you. Try for yourselves; and till you have had opportunities of investigating this point, consider it as *adhuc sub judice*.

Again, they say that the noise of friction denotes the existence of interlobular and subpleural emphysema. On this point I can give you no information of my own knowledge. That you may sometimes hear the costal pleura rub against the pulmonary during inspiration and expiration I know. I have often heard that sound (as I mentioned to you before) when the membrane has been roughened by pleurisy. But that a soft, smooth, moist lung, though embossed by emphysema, will give rise to a rubbing sound, I do *not* know. It may be so, but it has never occurred to me to hear it.

We may be more certain that interlobular emphysema has arisen when, immediately after some violent straining effort, considerable dyspnoea and oppression ensue, and presently the subcutaneous areolar tissue becomes emphysematous. You will understand how rapidly the inflation of the areolar tissue may take place if you ever saw a butcher blow up that of a calf which he is in the act of skinning.

As interlobular emphysema differs from vesicular emphysema in its seat, in the suddenness of its formation, and in its cause, so does it differ in its curability. Under favourable circumstances it will soon cure itself—the air will be reabsorbed, and the dyspnoea cease. I do not know that we can do much by art to accelerate that process. If the dyspnoea be extreme, it will be relieved by blood-letting; and if the air make its appearance, and can be felt, crackling, beneath the skin, you may let it out by making a few punctures with a lancet, and the deeper-seated emphysema will be lessened as the air-escapes. I believe that this interlobular emphysema is more common in infancy than in any other part of life; on account, I suppose, of the greater delicacy and tenderness of *all* the tissues at that age.

The interstitial areolar tissue of the lungs, as well as the air-cells, is liable to be filled not only with air, but with serous fluid; and this constitutes *œdema of the lungs*; a condition which is by no means uncommon, and one of which you ought therefore to be aware; but it need not long occupy our attention at present. When a lung, or a portion of a lung, is anasarcaous (and you will often find that the œdema is limited to the most depending part of those organs), it is generally of a pale grey or yellowish tint; is heavier than healthy lung, and less crepitant; and pits more on pressure—is *doughy*. And if the œdema be extensive, the lung does not subside perfectly when the chest is laid open. If incisions are made into such a lung, a thin watery fluid flows out, more or less spumous; and if the lung be well squeezed, the whole of the liquid may be expressed; and then it will be obvious that the texture

of the organ is sound, but that it had previously contained less air than usual, in consequence of the presence of the watery fluid.

This condition of the lung seldom takes place except as a part of general anasarca : and we may discover its existence, first by noticing that there is dropsy of the areolar tissue in other parts ; secondly, that the patient has dyspnoea ; and thirdly, by hearing crepitation, produced by large bubbles, at the lowermost portions only of the lungs. Into those portions the liquid gravitates ; just as it sinks into the ankles when the patient sits up or walks about. There is still air in the oedematous portion ; so that percussion still gives a hollow sound ; as hollow at least on the one side as on the other. With the air there is also liquid, which transudes, I suppose, from the areolar tissue, or is exhaled from the surface of the membrane : and the liquid is from time to time coughed up and expectorated. Sometimes, however, there is but little expectoration. What does come up is chiefly aqueous, with occasionally a piece or two of mucus floating upon it ; and it is somewhat foamy also.

This oedema or anasarca of the lung is symptomatic of other disease ; generally of disease of the heart or great blood-vessels ; and it is capable of no other rational treatment than such as is suited to the original disorder : and therefore I have nothing further to say about it now.

I proceed, in the next place, to that most prevalent and lamentable disease of the lungs, which is well known to everybody, under the titles of *pulmonary consumption* and *tubercular phthisis*. And without pausing to make any general reflections, touching facts which must be familiar to you all—the fatal and almost hopeless character of the disease, and the havoc it produces among the young, the most gifted, and the most beautiful, of the human race—I shall commence by inquiring into the *morbid anatomy* of tubercular phthisis ; which will naturally introduce us to the consideration of its symptoms, causes, treatment, and general history.

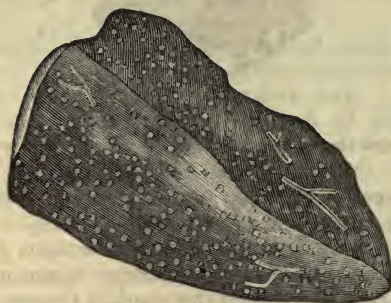
Phthisis, you know, means a wasting away, or a consuming ; but of late years the term has been restricted to that *species* of wasting disease, which consists in the occupation of the lungs by tubercular matter, and the changes which that matter *suffers* and *works*. But it would be an error to suppose that the disease is restricted to the *lungs* in these cases. The lung disease would be sufficient at length to destroy life ; but its mortal tendency is aided and accelerated, in most instances, by disease of a similar character, situated in other organs. “The *pulmonary* consumption (as Dr. Latham justly observes) is no more than a *fragment* of a great constitutional malady.” But that malady plays its part most conspicuously in the lungs. I shall notice its complications as I go on ; but I am desirous of cautioning you in the outset against supposing that tubercular phthisis is *exclusively* a pulmonary disease.

Before I proceed to a more particular description of the changes that, in the progress of consumption, are wrought in the lungs, I must briefly recall to your recollection certain points, relating to tubercular disease in general, which were brought before you in an earlier part of the course. The formation of tubercles is closely linked with the existence of the scrofulous diathesis. Tubercles themselves are composed of unorganized matter, deposited from the blood, of a yellowish colour, opaque, friable, and of about the firmness and consistence of cheese. This is what all pathologists agree in regarding as the true tubercular matter. It is most commonly deposited on the free surface of mucous membranes ; and not unfrequently amidst areolar tissue. You will remember that tubercles are not necessarily, as some have supposed, of a round shape. Their form depends upon the nature of the tissue in which the tubercular matter is deposited. Wherever it is laid down, it is liable to increase in quantity by the continual accretion of fresh matter of the same kind. Hence, when a speck of this peculiar matter is deposited in any soft uniform tissue—such as the brain, or the areolar membrane—there being no inequality of pressure from any quarter, it preserves a spherical or globular form as it grows larger. But taking the lung, with which we are at present chiefly concerned, the round form is sometimes real, sometimes apparent only. It is real when the tubercular matter fills up, or lines, and therefore takes the shape of, the pulmonary vesicles. So it is when a number of these, contiguous to each other, coalesce by the increase of deposit, and compose one large globular mass. And you may often catch the tubercles, if I may so say, in the process of forming these large rounded masses ; *i. e.* you may see them arranged in

circular groups or clusters, the interstices between them becoming gradually smaller and smaller. But when, as is often the case, the tubercular matter is laid down in the smaller ramifications of the bronchi, it assumes a cylindrical shape. This you may ascertain by carefully following the branching of the air-tubes: but in the manner in which the lung is usually divided by the scalpel, you *see* merely sections of these cylinders; and then the round form is apparent only. If the tubercular matter come to fill one of these smaller air-tubes, and also all the vesicles to which that tube conducts, then the new substance, when fairly displayed, represents a branch, with a cauliflower termination; like a twig with a bunch of leaves at its extremity. You may see these appearances delineated, from nature, in Sir R. Carswell's admirable lithographic drawings.

If this account of the formation of tubercles, as explained by Sir R. Carswell, be the true one; — of which I entertain little doubt; — it will follow, as a matter of necessity, that no alteration *can* take place in the tubercular matter, after it has once assumed the solid form, except through the agency of the parts around and in contact with it. No vital change can originate in the inorganic tubercle itself.

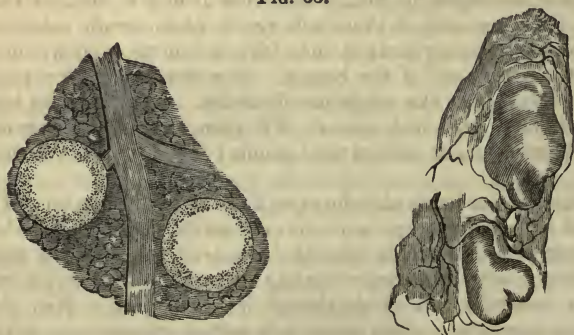
FIG. 62.



Miliary tubercle, scattered throughout the pulmonary tissue, forming translucent, greyish, and circular points, of the size of pins' heads.

Besides this true and undisputed species of tubercle, you will often find the lungs more or less thickly studded with a number of small granules, of firmer consistence, almost as hard as cartilage, semi-transparent, and of a bluish-gray colour. Respecting the nature of these granules — which are sometimes called *miliary tubercles*, some-

FIG. 63.



Microscopic appearance of miliary tubercle. The drawing to the left shows the site of the deposit and its relation to the pulmonary tissues; the effusion has taken place into the air-vesicle, which is distended into a globular ball. The drawing to the right exhibits a section of miliary tubercle without the bronchules, surrounded by congested vessels.

times the *granulations of Bayle*, who first described them — many different opinions are entertained. Laennec considered them to represent the incipient stage of the opaque yellow substance; and he calls them accordingly *nascent tubercles*. Andral

believes that they are simply some of the pulmonary vesicles rendered solid and hard by chronic inflammation. Sir R. Carswell explains their formation in this way. The membrane lining the air-passages secretes from the blood, not only the matter of tubercle, but its own proper fluid; whence it sometimes happens that a dull yellowish point of tubercular matter becomes enclosed and set, as it were, in a small pellet of grey, tough, semi-transparent mucus. Whatever may be the true theory respecting

FIG. 64.



Microscopic appearance of a mass of miliary tubercle, in close aggregation.

these little bodies, it is certain that they acknowledge some intimate connexion with the true cheesy tubercle. Both occur in the same persons, in the same lungs, in the same parts of the lung. One very seldom occurs without the other. They both belong essentially to the disease we are considering—pulmonary phthisis. Louis, a minute and faithful observer, states, that the granules present, at a certain period of their development, a central opacity. Upon the whole, it seems probable that Laennec's doctrine, in regard to the relation subsisting between the grey semi-transparent granule and the yellow opaque tubercle, was well founded.

He was wrong, however, in some other points, especially in his statement that the softening of tubercles begins in their *centres*. Sir R. Carswell has shown how the *appearance* of a central softening arises, first, in the smaller tubercles; secondly, in the larger agglomerated tubercular masses. The morbid secretion is deposited, principally, upon the mucous surface—upon the inner lining of the air-cells, and of the bronchial tubes communicating with them. Now it may so accumulate as to *fill* those cavities; and then, sections of them will represent the crude tubercles of Laennec. But it may only *line* the cavities: it may leave a central vacuity, containing mucus, or other secreted fluids; and if the lung be cut across under these circumstances, the divided air-cells will look like *rings* of tubercular matter grouped together; and so also will the divided bronchial tubes. We then have the *appearance* of tubercles with central depressions, or soft central points; and Laennec was deceived by those appearances.

But the larger masses begin also, he says, to soften at the centre. True: we do find the process of softening going on at several points within them at the same time. The masses, you must bear in mind, are formed by the growing together of many smaller tubercles; and the areolar tissue, with the other tissues which originally separated these tubercles, still exists, though it ceases to be visible. At length, under the augmenting pressure, or some other influence, it suppurates; and in this way the tubercular mass is broken down. Now this is the very process by which the tubercles are at length, often, expelled from the body. They increase till the surrounding parts take on inflammation, either from the increasing pressure, or from some accidental cause. The inflammation thus excited, occurring in scrofulous persons, has the scrofulous character. The thin pus which it throws out pervades and loosens the tubercular deposit; a process of ulceration goes on in the surrounding textures; and, at last, the softened scrofulous matter is gradually coughed up and expectorated.

This explanation of the mode in which the tubercles are formed, and increase, and

soften, and are removed, has been given to the world within the last few years, by Sir R. Carswell; and it is much the most simple and probable of any that I have seen. It is, moreover, perfectly consistent with the best ascertained facts concerning the progress of tubercular disease. You will find numberless theories broached by different authors on this subject, if you like to look for them; but I do not think you will find any more satisfactory than Sir R. Carswell's. And having recalled these

FIG. 65.

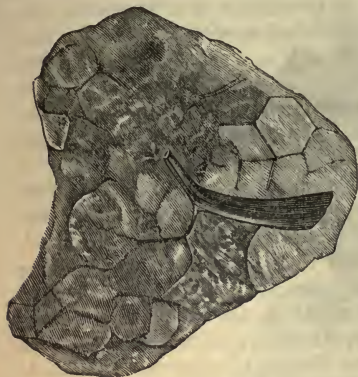


FIG. 66.



FIG. 67.

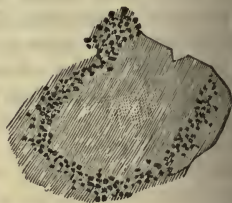


Fig. 65.—Hexagonal appearance caused by the mutual pressure of the air-cells, filled with yellow tubercular matter, with obliteration of the bronchules leading to the lobules. Magnified 60 diameters.

Fig. 66.—Microscopic appearance of minute vessels, surrounding air-vesicles in tubercular pneumonia, and miliary tubercle.

Fig. 67.—A section of an air-vesicle filled with yellow tubercles, and surrounded by exudation-corpuscles.

things to your memories (for it is some time now since I mentioned them before), we may go on to the further consideration of the morbid anatomy and pathology of tubercular phthisis.

It is a remarkable and very important fact, that tubercles, when they affect the lungs, are not deposited at random, or indifferently in all parts of those organs. It is in the upper lobes, and in the upper and back parts of those lobes, that, in nineteen cases out of twenty, and in more than that proportion, we meet with tubercles when they are few. It is in the same part that they are largest, and most numerous, when they are scattered throughout the whole lung. It is here, also, that they first ripen, and grow soft, and become ready for expulsion through the bronchi and trachea: consequently, it is here that we have the most frequent, the most numerous, and the largest excavations in the lung—what are technically called *vomicæ*. And the number and magnitude of the tubercles and of the *vomicæ* gradually diminish from the summit of the lungs downwards.

Now these are not merely *curious* facts: they have a most important bearing upon the diagnosis, in cases that might otherwise be doubtful. It is a rule which has but few exceptions—just enough to establish its claim to be a rule—that the favourite habitat of pulmonary tubercles is the upper part of the superior lobes of the lungs; and I may remind you that the converse of this is true (though with more numerous exceptions) of common inflammation of the lungs. Pneumonia affects by preference the lower lobes; it does sometimes commence in the upper, but that is comparatively rare. When it occupies the superior lobes, it generally has arrived there by travelling upwards from the inferior. You will at once perceive the practical advantage of knowing these points of contrast.

It is a curious fact also—less practically useful, perhaps, than the former, but still a valuable fact—that the left lung is much more obnoxious to tubercular disease than the right. Modern observers have collected numerical statements showing that this really is so. Why it should be so, I know not. Thus Louis, whose volume is the result of immense labour in observing, and is full of the most instructive matter, had

met with *seven* cases in which tubercles were confined to a single lung: in *two* of the seven cases it was the right lung that was thus exclusively affected, in *five* it was the left. Of 38 instances in which the upper lobe was totally disorganized by the disease on one side, 28 were of the left, and only 10 of the right. Eight times he had known the pleura perforated by the extension of tubercular disease; and seven times out of the eight the perforation happened on the left side of the chest. So also Reynaud met with 27 cases of pneumothorax on the left side, to 13 on the right. No less curious is it that here also the facts ascertained with respect to pneumonia are just the contrary of those which belong to phthisis. I mentioned, in a former lecture, Andral's conclusion, derived from the observation and collation of 210 examples, that pneumonia is more than twice as common on the right side as on the left. M. Lombard, of Geneva, found the ratio somewhat less than this, but still great. Of 868 instances of pneumonia, 413 occurred on the right side alone, 260 on the left alone, and 195 on both sides at once. That is, there were three on the right side alone, for every two on the left alone.

The tubercular matter, then, being deposited on the inner surface of the vesicles, and of the small bronchial tubes that conduct to them—groups of these diseased air-cells, lying contiguous to each other, become, more or fewer of them, amalgamated, or fused, as it were, into one large mass: and generally there are tubercles of various sizes, from that of a pin's head, to that of a pigeon's egg, in the same lung. And there is yet another disposition which the tubercular matter is apt to take: it sometimes is diffused uniformly over a considerable space, occupying all the vesicular and interstitial portions of the part affected, and having no distinct circumscribing boundary. The part looks as if fluid tubercular matter had been poured into it, and there had hardened. This is what the French call *tubercular infiltration* of the lung.

The tubercular matter, once deposited, may remain for a longer or shorter time in what is called the crude state; surrounded by perfectly healthy lung, undergoing no increase in quantity, and no alteration of consistence. But in a vast majority of cases, serofulous inflammation is sooner or later set up around the tubercles—or in the areolar tissue imprisoned within the agglomerated masses—and then the whole breaks down in the way I mentioned before; and the detritus is conveyed through one or more tubes into the primary divisions of the bronchi, and thence to the mouth, to be expectorated. Of course there is an excavation, cavity, cavern, or vomica, left. All these names are given to the void space which the tubercular matter previously occupied. Now there are some interesting particulars to be mentioned respecting these cavities; but I must postpone them till our next meeting.

FIG. 68.



The apex of a lung affected with tubercular pneumonia; the limitation of the disease was defined by a sharp line bounding the inflamed tissue, which surrounded the tubercular deposit.

LECTURE LVI.

Phthisis, continued. Vomica; adhesions of the pleura; ulceration of the larynx and trachea — of the intestines; fatty liver; waxy liver; auscultatory signs of a vomica; gurgling, cavernous respiration, pectoriloquy: general symptoms of phthisis; cough, expectoration, dyspnœa, pain, hectic fever, diarrhœa, wasting œdema, aphthæ.

WE were engaged, when we separated yesterday, in investigating the *morbid anatomy* of consumption. Bear in mind how and where the tubercular matter, which is the essence of that disease, is deposited in the lungs; that it occupies by preference their upper lobes, and the upper part of those lobes; invading gradually the lower lobes, from above downwards, as the disease advances. Both lungs are, commonly, affected at the same time, though in unequal degrees. Among one hundred and twenty-three instances of phthisis, Louis found that the tubercles were limited five

FIG. 69.



The apex of a lung containing numerous cavities, with tubercular deposit intervening. The large cavity, and several of the smaller ones, are lined with an adventitious membrane.

times to the left lung, and twice to the right. The tubercular matter, once deposited, may remain quiet and unchanged for some time; but in general it increases in quantity, until at length inflammation, of a low and scrofulous character, arises in the pulmonary substance in immediate contact with the tubercles — or in the areolar tissue involved in the larger agglomerated masses — in consequence of which inflammation, a sort of suppuration takes place: the tubercular matter becomes soft, and breaks down, and is ultimately expelled through the bronchi, trachea, and mouth. The vacuities left in the lung after this process of expulsion are

called cavities, excavations, caverns, vomicae. And I go on to consider certain points of practical interest connected with these vomicae.

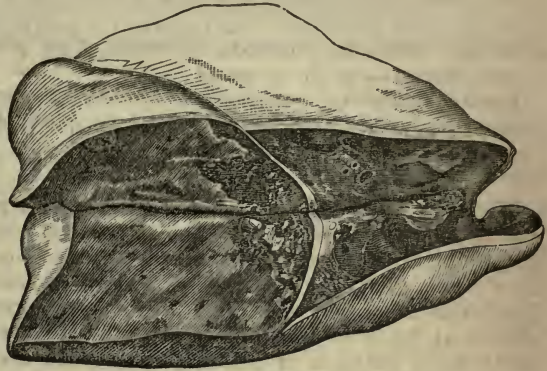
In the first place, as you may see by the specimens upon the table, they vary greatly in size. They may be no bigger than, or not so big as a pea: they may be large enough to contain a pint or more of fluid. Sometimes the whole of the upper lobe is converted into a bag of this kind. These large cavities are never met with in the lower lobes. They are formed by the union of several that are smaller; so that they are often of very irregular shape, and divided, as it were, into chambers, by imperfect partitions, or by bands which cross them in various directions. Opening into the cavity, there is always one, and there are generally several, pervious *bronchial tubes*, which seem as if they had been cut off just where they enter the cavity. But you never, or very seldom indeed, find a *blood-vessel* thus opening into the cavity. And the reason of these differences is plain enough. It is not, as some modern authors have fancied, that the arterial or venous tissue possesses a low degree of vitality, and therefore resists or avoids the destruction in which the surrounding tissues are involved. That principle may be applicable to other cases, but it is not applicable to this. The opinion I am now referring to proceeds on the supposition that the bands which sometimes cross the cavities are really blood vessels that have escaped the disorganizing process. Such seems to have been the notion entertained

by Bayle; and it has been more recently and more expressly advanced by Cruveilhier. But the truth is, that these bands rarely *contain* blood-vessels: and when they do contain them, the blood-vessels are mostly *impervious*. In one hundred and twenty-three cases, Louis found pervious blood-vessels in the bands no oftener than five times.

The true reason why bronchial tubes open into these cavities, and blood-vessels do not, is to be found in the natural differences between the two sets of vessels, in respect of their structure, and in respect of the fluids that pass through them. The blood-tubes yield readily to external pressure. Many of them are pushed aside and flattened by the progressive increase of the tubercular matter around them; some probably are obstructed by its gradual accumulation within. In either case the stagnant blood coagulates, and the vessel is obliterated to some distance from the place of the original obstacle; just as you know a clot forms, and seals up an artery, which has been tied during life, for some little way anterior to the ligature. But the bronchial tubes are neither so easily compressed, nor do they carry any coagulable fluid. In the agglomeration of the tubercular masses, by the softening of which the cavities are formed, the air-tubes included within the mass are filled up by it; and when the whole breaks down in scrofulous suppuration, they are expectorated with the rest of the detritus. Meanwhile their open mouths, on the hither side of the point where the tubercular matter stopped, remain, and afford a channel through which the same matter, after it has become soft, finds its way towards the trachea. Occasionally, indeed — but that, I repeat, is a rare occurrence — a considerable blood-vessel *does* get laid open during the formation of a vomica, and then copious and fatal hæmorrhage ensues. Occasionally also an oozing of small quantities of blood takes place from the inner surface of the cavity, tinging the matter expectorated. Hæmoptysis is certainly much more frequent before the softening and expulsion of tubercular matter than afterwards.

When the vomica is first formed by the expulsion of the tubercular matter, its inner surface is soft and ragged; and if other softening tubercles are in the immediate neighbourhood, the cavity goes on enlarging; that is, two or more vomicæ coalesce. If, however, there happen to be no more tubercles thereabouts, the cavity may remain

FIG. 70.



A lung, exhibiting extensive tubercular disorganization throughout its upper lobe, which is almost converted into one rugged cavity. The pleura is very much thickened; intimate adhesion has taken place between the upper and lower lobe, and the tubercular deposit is seen encroaching upon the latter.

FIG. 71.



Pulmonary caverns intersected by cord-like bodies. From a specimen in the cabinet of Dr. Gross.

If, however, there happen to be no more tubercles thereabouts, the cavity may remain

stationary. Its inner surface then becomes smoother : and something like a membrane forms upon it : and sometimes a puriform fluid is poured out by this surface, and sometimes not. Generally the pulmonary tissue around such a cavity is condensed and solidified ; partly perhaps by crude tubercular matter which it contains, partly in consequence of the inflammatory process of which it has been the seat during the softening of the tubercles. It is important to bear in mind this fact of the condensed, solid state of the lung immediately surrounding a vomica ; for it explains certain peculiarities met with in the symptoms.

There is another point of much interest connected with these vomicæ. When they occur singly, without other tubercles or cavities (which, though a rare thing, does sometimes happen) ; and when occurring thus singly they have been completely emptied of the tubercular matter ; they may gradually contract, and ultimately become obliterated. This approach of their sides leads to a puckering of the pleura on the surface of the lung : and, on the other hand, a puckering of the surface indicates that beneath it there is probably a shrunk, or an *obsolete* vomica. The central part of the diseased spot is converted into a substance resembling cartilage ; and the appearance it presents is called a cicatrix ; and really it deserves that name. The process which has gone on is a process of natural recovery : and the recovery would be complete and permanent, if no fresh deposit of tubercular matter took place. Too often, however, the tubercles multiply, until at length their number, or size, or effects, become incompatible with the further continuance of life.

FIG. 72.

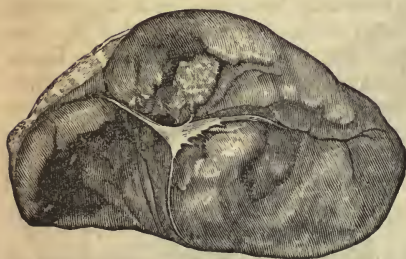


FIG. 73.



Fig. 72.—Cicatrix at the apex of a lung, resulting from the previous arrest of tubercular disease.

Fig. 73.—Internal section of the summit of the left lung, showing the stellate puckering at the apex, and another lower down. Corresponding with the former may be seen a cavity the size of an almond, in process of contraction, and surrounded by dense fibrous radii. Natural size. From Bennett.

This, then, is one way in which tubercular disease, *limited to one small* portion of the lung, *may* be eliminated, and the part which it occupied undergo a kind of repair. But the disease, *when so limited, may* cease in another way. The more watery parts of the morbid secretion may be absorbed : and the earthy salts it contains may concrete ; and the whole be converted into a shrivelled, hard, chalky mass, which sometimes is coughed up, sometimes, in favourable cases, remains for years in the lung, an inert and almost harmless body.

Let me state, while I think of it, that the expectoration of these chalk-like concretions, denoting, as it usually does, the existence of pulmonary consumption, marks at the same time the chronic character of the case. I am acquainted with a gentleman who, though delicate, enjoys a very fair share of health, and who has for years been coughing up, at intervals, little branching fragments, like bits of white coral, consisting principally of carbonate and phosphate of lime, and evidently moulded in the smaller bronchial tubes.

Take notice that depressions and puckerings on the surface of a lung do not always indicate the presence of a cicatrix, or of an obsolete tubercle, beneath them. It is obvious that they may, and in point of fact they often do, result from mere lobular collapse, or from circumscribed atrophy of a portion of the lung from any cause; the neighbouring tissue being sound, or perhaps emphysematous.

When the tubercles are numerous — or rather when they lie near to the surface of the lung, as, of course, they are likely to do when they *are* numerous — they very generally give rise to dry or adhesive pleurisy. So that, in a person dead of consumption, it is a very rare thing to find the lungs free from adhesions to the ribs. I mentioned before that this attachment of the lung to the walls of the chest affords a protection against a much more formidable condition; namely, perforation of the pulmonary pleura, and the escape of tubercular matter and air into the serous cavity: producing that worst kind of pleurisy which implies pneumothorax. That the pleurisy and adhesion are consequences of the presence and irritation of the tubercles, appears from this: — that, for the most part, the extent and the situation of the adhesions correspond with the extent and situation of the tubercular disease. Thus, a single spot of adhesion has been seen to unite the costal and pulmonary pleuræ exactly opposite a solitary tubercle which lay near the surface of the lung. As the summit and back part of the upper lobes are most thickly set with tubercles, so here also is the adhesion the most constant, and the most firm. You will often find the upper part of the lung invested with a thick cap of false membrane: and the connexion between the pleuræ so tough, that the lung breaks down in the attempt to separate them.

To show you that these statements — which have long been familiar to those much conversant with disease and with morbid anatomy — to convince you that they are strictly borne out by numerical or tabular facts, I may again have recourse to Louis. He tells us that in 112 cases which he himself examined of persons dead of consumption, and having therefore tubercles in their lungs, there was but one in which both lungs were free from adhesions. In eight cases the right lung was exempt from them; and in seven cases the left. Again, in twenty-five other instances, there were either no cavities, or very little ones; and the adhesions were accordingly of small extent, and easily broken down. In the remainder there were large vomicæ, and the adhesions were extensive, dense, and firm.

Such is a sketch of the changes which take place in the *lungs*, in consequence of the deposition of tubercular matter in them, and of the changes which that matter undergoes. But the air-passages that lead to the lungs are very liable to become implicated as the disease proceeds. The mucous membrane of the larynx and trachea ulcerate: and when the morbid condition of the larynx gives rise to prominent symptoms, and especially (as it is apt to do) to hoarseness and loss of voice, the disease is sometimes called *laryngeal phthisis*. But there is no such disease, that I know of, existing by itself. I mean, that *scrofulous* ulceration of the larynx and trachea occurs only when the lungs are affected with tubercles. It is curious that when ulcers are met with in the trachea, they are often found on one side of it only; on the side, viz., which corresponds with the diseased lung, or with that lung which is most diseased. In like manner, when some of the bronchi are found red internally, and even ulcerated, these appearances are confined to those bronchi which communicate with cavities; and do not occur in the bronchial canals leading to crude tubercles. It is towards the back part also of the trachea that the ulcers, especially when large, are most commonly observed; the floor of that channel as the patient lies supine. And when the epiglottis is involved in the mischief, the ulcers are situated, almost always, on its laryngeal surface alone. We have strong reasons therefore for believing that their formation is influenced by the contact of the matter which is expectorated, in its frequent passage over the mucous membrane. Moreover the little mucous glands

wherewith the membrane is provided, are most numerous at the posterior part of the trachea and bronchi; and these glands are especially prone to ulcerate.

In respect to these points also Louis has made comparative observations. Among 180 persons who died of some chronic disorder, *not* phthysical, he once only met with ulceration of the larynx; whereas of those who perished of consumption, about *one in every five* had ulceration of the epiglottis or larynx, and nearly *one in three* had ulceration of the windpipe. Hence it would appear that, if we except the effects of the syphilitic poison upon the larynx, ulceration of that part is almost peculiar to phthisis pulmonalis.

I have told you that consumption is not merely a lung disease. Its local ravages are most obvious indeed in the thorax; but it leaves in the abdomen also traces of its destructive agency not less definite, and scarcely less constant. You know that the surface of the intestinal canal is strewed, throughout, with separate mucous follicles; and that the lower portion of the ileum is furnished with other follicles, collected together in oval or circular groups. When I come to speak of continued fever I shall have much to say about the changes which these little glandular bodies undergo, in one form at least of that disease. These same glands are the frequent seat of tubercular deposit in phthisis. Here and there you may see a solitary yellow tumour, not larger than a hemp-seed, projecting from the surface of the bowel. In other places the ripened little tumour has burst, the tubercular matter is gone, and a ragged roundish ulcer remains. More frequently the aggregated glands are affected; and the ulceration, in them, varies much in form and extent. It often involves the whole patch, and then the shape of the ulcer is more or less elliptical. Louis met with ulceration of these glandulæ agminatæ in five-sixths of all the fatal cases of phthisis that he examined. Ulcers of greater magnitude, were very nearly as common in the large intestines. And it is worthy of notice that, the disorganizing process being in these cases slow, nature has time (if I may use such metaphorical language) to provide against the threatened perforation of the gut. The tissue that forms the base of the ulcer, whether it be the muscular or the peritoneal coat, is thickened and vamped; or the bowel becomes adherent to some contiguous surface. Once only in my life have I known scrofulous ulceration, in phthisis, penetrate the serous tunic, and allow the contents of the intestine to escape into the sac of the peritoneum.

You ought to be aware — although the facts possess, as yet, no practical value — that the stomach is often much enlarged and thinned in those who die of consumption; and that the liver is very apt to undergo remarkable changes, almost yet not altogether peculiar, I believe, to that disease. It, too, enlarges, and there are two varieties of its enlargement. In the first, the liver becomes full of adipous matter, greasing the hands and scalpel of the anatomist, and yielding when heated an oily substance, which makes a grease-spot on paper placed in contact with it. The whole gland partakes in the alteration, is of soft consistence, loses its natural red tint, and assumes a pale fawn colour. In three years Louis met with this fatty liver, as it is called, forty-nine times; and forty-seven of the patients died phthysical. It occurred in one-third of the whole number of the victims to consumption; whereas, among two hundred and twenty-three cases, not phthysical, there were two examples only of this hepatic change. It is more common in women than in men.

The second kind of enlargement is produced by the infiltration of the hepatic tissue with an albuminous material. The cut surface of a liver thus altered has a whitish and glistening appearance; hence it is called the *waxy* liver; and by the Germans, from its resemblance to bacon, the *lardaceous* liver. The waxy liver so far resembles the fatty, that it is frequently observed in connexion with scrofulous disease; and that it is large, sometimes exceedingly large, with a smooth surface, and a blunt lower edge. Both these forms of hepatic enlargement proceed insidiously, with little or no pain or tenderness. Their presence is revealed during life by no symptoms, except that the enlargement belonging to them may at length be ascertained by percussion and pressure with the fingers.

Let us now inquire what modifications of the healthy sounds arise from the altered conditions of the lungs in phthisis. Most of them are such as you would naturally expect. Whether a portion of lung be rendered solid by common inflammation, or by the presence of tubercles in it, the result, so far as the auscultatory signs are concerned, will be the same. In such a piece of lung, supposing the solidification com-

plete, no vesicular breathing can be heard; but bronchial breathing and bronchopony will be audible, in each case, if the solidified portion enclose a considerable bronchus, and come near the surface of the chest. And percussion will give a dull sound, whether the lung lying beneath the part struck be hepatized, or blocked up by tubercular matter. On these points, therefore, after what was said in a preceding lecture, I need not dwell. But the excavations—the empty or half-empty vomica—these are something new. We have hitherto met with no condition exactly similar to that of a large cavity. And accordingly I have to make you acquainted with two or three new sounds: or sounds which are modifications of those formerly described, and in most instances sufficiently distinct from them to have acquired peculiar names. You will remember that what we have called large crepitation depends upon the passage of air through liquids; the liquids being contained in tubes; those tubes the bronchi and their ramifications. But when pus or vitiated mucus, or liquid of any kind, is collected in a *vomica*, which communicates freely with the trachea through pervious bronchi, the bubbles produced by the entrance and exit of air will be still more numerous and large; and a sound is then produced which the word *gurgling* expresses well. Laennec calls it *gargouillement*. This sound is heard, too, in a circumscribed space; and not diffused, as large crepitation usually is. Wherever, therefore, we hear gurgling during respiration, or during the act of coughing, there, we conclude, exists a *cavity*. But the cavity is not necessarily a *vomica*. In ninety-nine cases out of a hundred it will be so; but in the hundredth case perhaps it will not. Bear in mind what was formerly stated of dilatation of the bronchi: how sometimes they terminate in a considerable globular expansion; sometimes belly out and contract again several times alternately: and you will see that cavities containing liquid, or liable to contain liquid, belonging equally to the one condition and to the other, and the sound in question depending solely on the intermixture and agitation of air with liquid in a cavity, we cannot be sure from mere gurgling respiration, or gurgling cough, that we have a *tubercular* excavation beneath our ear; or that the case is one of consumption. Gurgling may also proceed from that very rare morbid condition, abscess, the result of common inflammation of the lung. These constitute the only sources of fallacy in the matter. The fallacy seldom interposes; but it does sometimes interpose; and therefore it must *qualify* our conclusion from this symptom of gurgling, with respect to cases otherwise doubtful.

Again, the vomica may be empty of liquid; and then we hear, as the patient respires, not vesicular breathing of course, nor yet exactly bronchial breathing; it is something more than that when the cavity is large, something different in character from it when the cavity is small: but whatever the character of the sound, as we believe it to take place in a vomica or cavern, we call it *cavernous respiration*. It is a hollow sound, especially when the cavity is of considerable size; an exaggeration of mere bronchial respiration. But the cavity may be small. The moment a portion of tubercular matter is separated and discharged through a neighbouring bronchial tube, the cavity has commenced; and the sound produced in these little cavities during the breathing may be of various kinds. It may be, and it often is, a click, like the opening and shutting of a valve; or a chirp; or a creaking; or like many other well-known sounds; but, as all these sounds, under certain circumstances, denote the formation of a vomica, it is best, for simplicity's sake, to call them all by the same name—*cavernous respiration*.

Dr. Latham explains in a few words the causes of these differences. "The varieties of cavernous breathing are doubtless owing to different sizes, and forms, and situations of cavities, and to different conditions of the surrounding lung. A cavity may be very large or very small. Several bronchi may open into it, or only one. It may be a simple cavity, or it may have many chambers. Its sides may be condensed and equal, or rough and ragged. The lung around it may be solid and indurated, or pervious and vesicular. It may be near the ribs, or far from them: adherent to, or separate from the pleura. It is quite obvious that these different circumstances are calculated to modify the sound, which will, nevertheless, be always such as indicates a cavity."

A tubercular cavity may be so large, and of such a kind, as to yield the metallic sounds which are apt to be heard in pneumothorax. I show you a cavity in which those sounds were actually heard, most distinctly, by many persons, during the

patient's life, while he was under my care in the Middlesex Hospital. I was certain beforehand that these sounds proceeded, not from the cavity of the pleura, but from a tubercular excavation. Once subsequently, in a patient who was dying of phthisis and diabetes, I have heard the same sounds; also, I am sure, in a tubercular cavity. The patient insisted on leaving the hospital, and I lost sight of him before he died.

I promised, when speaking of these metallic sounds as arising (as they much more commonly do) from pneumothorax—a collection of air, or of air and a liquid together, in the sac of the pleura—I promised to point out the circumstances whereby you may tell which of the two conditions in question the sounds denote. Both of the conditions imply, in general, the existence of tubercular phthisis; and therefore the observation of the ordinary symptoms of phthisis will not help us much.

Now, in the cases seen by me, there were two circumstances that stamped them as being cases in which the sound proceeded from a cavity *in* the lung, and not *exterior* to it. One was the *situation* in which the sound was *invariably* heard. The other was, the absence of excessive resonance when that part was percussed. You know that when air is contained in the pleura itself, the sound yielded on percussing the chest in the corresponding spot is quite tympanitic, like that of a drum. But it is a well-ascertained fact, (though contrary perhaps to what you would suppose,) that the sound is duller over tubercular cavities, in nine cases out of ten, than over sound lung. The explanation of this fact is simple enough. It is that the layer of lung which still remains in such cases, thick or thin, is dense and solid, and damps the sound which the vomica might otherwise make resonant. But then again the situation of the metallic sounds was a guide. They occupied the upper part of the chest: the very part where vomicæ are wont to be the most common, and the largest: and moreover a part where pneumothorax seldom or never exists. The summit of the lung is generally covered in phthisis with a cap of false membrane, which binds it to the ribs: and this, as I observed before, is the main reason why perforation of the pleura pulmonalis is so rare in that disease; and it is also the reason why, when it does take place, it seldom takes place at or near the apex of the lung. In truth it is found by experience that (though the rupture of the pleura *may* happen in any part) the place where it usually occurs is in the lower and back part of the upper lobe of the lung, opposite the angle of the third or fourth rib; that is, just beneath the edge of the false membrane by which the summit of the lung is generally adherent. But the sound, in the cases I refer to, was *invariably* heard at the very top of the chest. It did not shift, as that of pneumothorax may often be made to shift, when the patient changed his posture. Attention to these points will always lead you to an exact diagnosis. You may say, perhaps, "The complaint being in each case a mortal one, what is the use of so much refinement?" Why, there is this utility in it. We may sometimes, as I stated before, give great relief to the patient, and save his life for a time, by tapping the chest in pneumothorax. Air may get in with each inspiration, and threaten immediate suffocation; and the thorax being punctured, it will issue in a blast. But no one would think of tapping a tubercular cavity.¹

To give out the amphoric resonance and the metallic sounds, the vomica must, I presume, be a *large* one. That which is before you, the only one I ever *saw* in which those sounds had been heard, is very large. Its inner surface is smooth: it adheres to the ribs externally by at least two-thirds of its circumference: and the medium of adhesion is very thin. Quite low down, a single bronchial tube, of about the third division, may be seen to enter it.

So much, then, for the modification, by a tubercular cavity, of the sounds heard during *respiration*. But the *voice* will also be modified, if the cavity be of considerable size, and near the surface, and have dense walls, and be empty. Then we hear, in that part, when the patient speaks, the sound which is called *pectoriloquy*: as if the voice proceeded from the chest. The words are distinctly articulated into the ear of the listener. But I need not trouble myself or you by attempting to *describe* pectoriloquy. You may any day hear the exact sound that word is intended to denote, by placing a stethoscope over the trachea of one of your friends, applying your ear to

¹ In this the author finds that he was mistaken. The operation has since been proposed and practised. That it can meet with much success, or favour, he does not yet believe.

the other end of it, and getting him to speak: just as you may obtain an exact notion of bronchial respiration by listening then to his breathing.

For some time after the first appearance of Laennec's great and original work on the diseases of the chest, pectoriloquy was deemed to be the pathognomonic and infallible sign of a vomica. "Oh," the young auscultator would say, "I detect pectoriloquy beneath the clavicle. There can be no further doubt about the nature of the disease. My patient has not only tubercles, but a cavity in his lung." So I long thought; and so some, I fancy, think still. Yet the evidence afforded by mere pectoriloquy of the presence of a vomica, or even of the presence of tubercles, is far from being certain or trustworthy. Experience had taught me this before I knew that many others, studying under the same schoolmaster, had learned the same truth. Among my hospital patients many years ago was a man who laboured under phthisis. Percussion gave a dull sound under his right collar-bone, and in the same spot loud and distinct pectoriloquy was audible. I well recollect inviting the particular attention of the pupils to this case, as affording an exquisite specimen of pectoriloquy; and I predicted very confidently that after the patient's death, which was obviously at hand, a large excavation would be found in the summit of his right lung. My prediction did me no credit. The *left* lung indeed was hollowed by cavities, but there was nothing like a cavity in the right. The upper part of the lung was thoroughly and uniformly solid; filled with hard, grey, tubercular matter. The large bronchial tubes were pervious, and the voice descending into them had been conducted by the solid lung with perfect and almost painful distinctness to the listener's ear. This was a useful lesson to me: and I mention it that it may be a lesson to you. Remember that solidification of the summit of the lung will modify the sound of the patient's voice, very much in the same manner as a large vomica there situated. It is stated, indeed, and perhaps truly, that a practised ear can discriminate between the loud, diffused, though articulate, resonance of the voice produced by solid lung, and the circumscribed, whiffing pectoriloquy of a cavity. But the distinction is too nice for the average of ears. Now since the pulmonary tissue may be rendered dense and solid by other causes than tubercles, pectoriloquy does not always indicate the existence of consumption. The fallacious condition does not often occur; for common inflammation is seldom limited to the upper part of the lung; and the whole of that part is seldom completely hardened by crude tubercles. But whenever it does occur, it is apt to mislead or puzzle. I was consulted last year about a gentleman in whom this phenomenon of pectoriloquy was strongly marked. Two excellent auscultators had been led, by this symptom, to the belief that a cavity existed in the lung. Remembering the case I have just mentioned, and learning that the patient had been ill for a few days only, and had not previously suffered cough, nor any apparent pectoral complaint, I was of opinion that the summit of his right lung had become hepatized by acute pneumonia. And it was so. The patient died; and the diagnosis I had formed was verified upon inspection of the lung. Dr. Latham relates one or two examples to the same purpose. Dr. Stokes goes so far as to consider pectoriloquy the least important and most fallacious of all the physical signs of phthisis. Taken alone (he says) it is absolutely without value. Sir John Forbes has come to similar conclusions. Certainly cavernous respiration is a much more alarming sound.

Wherever actual pectoriloquy from a cavity is heard, there also will be heard cavernous respiration. But the converse of this is not necessarily true. There may be, and there often is, cavernous respiration and a cavity, yet no pectoriloquy. The cavity is not large enough, or not near enough to the surface of the chest, or not of such a kind as to reverberate the voice.

Often when pectoriloquy is absent, and cavernous respiration is doubtful, and gurgling even cannot be heard (because the communication with the bronchi is not free), a slight splashing sound will occur when the patient coughs: nay, you may sometimes hear it, if he hold his breath, with every beat of his heart, which causes a little succussion in the cavity: but its contents must then be thin.

Now when the sounds I have been engaged in describing are well marked, they denote the existence of a vomica. The only source of fallacy is that which I formerly mentioned; the same sounds may arise from a cavity in the lung, whatever be its nature; and therefore they may arise when the bronchi are expanded into cavities

But I repeat, that this is a deceptive condition which you cannot calculate upon meeting with often.

When the sounds are not well marked, take time before you pronounce a decided opinion respecting them. Strong bronchophony comes very near to weak pectoriloquy: bronchial respiration may closely resemble some varieties of cavernous breathing: large crepitation, confined to a small spot, may simulate gurgling. It is better, when the sounds are thus equivocal, and when they may denote conditions so very different in their nature and tendency, to suspend one's judgment, and to give a guarded opinion. A little time in such cases will clear away the doubt.

I am afraid of being tedious about these sounds; but really they are of immense importance. Upon their exact appreciation, and correct interpretation, will depend the opinion you will be *called upon* to express; and that opinion will, in many cases, be a sentence of life or death with respect to the dearest friends of those who hear it. A correct diagnosis is also very important, in the early periods of the disease especially, for another reason. It is in those early periods alone that we can entertain much hope of arresting the progress of the complaint by art, or by change of climate.

I must now consider the *general* symptoms of this most afflicting disease: and while doing so, I shall point out how the physical signs confirm or confute their language, in cases which might otherwise be doubtful.

The general symptoms of phthisis are cough, dyspnoea, expectoration, hæmoptysis, wasting, hectic fever, hoarseness or loss of voice, diarrhoea; and there are some other symptoms which mark often some of its stages, and to which I shall incidentally advert. I shall speak of them all as briefly as is consistent with clearness.

Cough is one of the earliest symptoms of consumption; and it is that which commonly first attracts the attention, and awakens the fears, of the patient or the patient's friends. Generally at first it is slight, occasional, and dry: it occurs upon the patient's getting out of bed in the morning; or if he make any unusual exertion in the course of the day. It feels to him as if it were caused by irritation about his larynx. Sometimes it will cease for a while, as in the warm weather of summer, and recur in winter when the external temperature is lower. By degrees it begins to be troublesome in the night: and to be attended with more or less expectoration of mucus.

Now when such a cough steals upon a person gradually, and when no reason can be assigned for its occurring, that circumstance alone is enough to excite suspicion as to its true nature and cause. But chronic cough may exist without any tubercular disease of the lungs: as you well know. It may depend upon a disordered state of the *stomach*; the *pneumogastric* nerve may be irritated *there*. It may be the cough of *chronic catarrh*; it may result from disease of the *heart*; it may be the nervous, barking, importunate cough which I formerly mentioned as of frequent occurrence in *hysterical* girls. And bearing these circumstances in your mind, you will inquire, and you will generally make out without much difficulty, whether there be any unnatural or deranged state of the digestive organs; or chronic catarrh; or cardiac disease; or hysteria. These are points on which I need not further insist.

I may observe, here, that as chronic cough may exist when there is no consumption; so consumption may sometimes exist, and even prove fatal, and large portions of the lungs may be disorganized, without there having been any cough; or at least without the occurrence of enough cough to draw the notice of the patient or of his friends to it. This is not common, however: cough is usually present, more or less, during all the stages of phthisis, and it is often that symptom which most distresses and harasses both the patient and his family.

Great attention used to be paid to the *expectoration* in cases of suspected phthisis. It was thought that if a patient spat pus, he was in a state of confirmed consumption: and whole volumes have been written, and prizes awarded to their authors, respecting the means of distinguishing pus from mucus. But we now know that, so far as the diagnosis of phthisis is concerned, this is a very idle inquiry. The inflamed bronchial membrane may secrete pus; so that the presence or absence of pus in the sputa is no test at all of the presence or absence of tubercles in the lungs. If you are, nevertheless, curious to know how pus may be identified, one easy criterion is that proposed by the late Dr. Young. You are aware that pus, like the blood, con-

tains globules; and these globules, when examined through transmitted light, exhibit prismatic colours; appear surrounded by rings of colours, somewhat resembling those of the rainbow, but differently arranged, and often beautifully brilliant. Mucus, having no such globules, affords no such coloured rings. The way to make the examination is, to put a minute quantity of the fluid between two small pieces of plate glass; to hold the glass close to the eye; and to look through it at a distant candle, having a dark object behind it. A yet readier, and I believe a better test, is furnished by the *Liquor Potassæ*, which converts pus into a viscid stringy mass, while it liquefies mucus.

For those who possess, or have access to, a good microscope, all other means of discrimination become superfluous. The globules peculiar to pus may be recognised, by an instructed eye, at a moment's inspection.

Whether the expectoration be puriform or not, has ceased, however, to be a question of much importance as regards the diagnosis of phthisis. A portion of the matter expectorated comes from the surface of the bronchi, and consists of altered mucus: and therefore, the sputa brought up in phthisis, and the sputa brought up in bronchitis, are, in a great degree, the same. These are partly composed of a ropy transparent fluid, in which opaque masses of a yellow or greenish colour are seen to float; and intermixed also with which there may be a good deal of froth. The froth is a measure of the difficulty with which the mucus is brought up: and it is usually less abundant and conspicuous in phthisis than in bronchitis. The heavy, sage-leaf sputa that we sometimes see, belong to both diseases.

You may occasionally find portions of tubercular matter in the expectoration; a circumstance quite decisive, when we are sure of it, of the nature of the case: dull yellow streaks, or little curd-like fragments involved in the mucus. But small opaque specks of that character are sometimes formed in the follicles of the tonsils; and this makes the appearance more equivocal. The sputa *most* characteristic of tubercular disease consist of globular, grey, flocculent masses, which look like little portions of wool more than anything else. *Nummular* sputa the French call these, because when spat into a vessel not containing water, they assume a flat circular form, like a piece of money, and remain separate and distinct from each other. When they are spat into a glass of water, you perceive that some of them subside to the bottom—some float on the top, suspended, apparently, by healthier mucus in which they are entangled, or by bubbles of air—and some remain stationary at different depths. When stirred and agitated in the water, they render it slightly milky. This kind of expectoration commonly marks a confirmed and advanced state of the disease; but it will continue for weeks sometimes. It is not *perfectly* pathognomonic, but *nearly* so. On one occasion I found expectoration of this nature from a man whom I did not very diligently examine by my ear; and I set the case down as one of phthisis chiefly on the observation of that symptom. The patient evidently had not long to live. Our apothecary at the hospital, Dr. Corfe, had more time to explore the condition of the chest: and he came to the conclusion that the disease was not tubercular phthisis, but extensive chronic bronchitis: and sure enough he was right. When we came to examine the lungs after the patient's death, not a tubercle could be found. I am satisfied that there is no kind of expectoration which indicates phthisis with *perfect* certainty: but that which I have just been describing very *seldom* occurs unless there is phthisis. Louis appears to have noticed these round, separated, woolly masses twice only unconnected with tubercles: and once the same thing has occurred to Chomel: so that, when the other symptoms are obscure and doubtful, this will materially augment the gravity of the prognosis. Flies appear to be more attracted by the sputa of phthisis than by any other sputa.

Practised microscopic observers are sometimes able, I believe, to discriminate "tubercular corpuscles," and portions of broken down pulmonary tissue, in the expectoration of phthical patients. In this method of diagnosis I must confess that I am inexpert.

Hæmoptysis is a kind of expectoration; the expectoration of *blood*. I have already spoken of this symptom as connected with phthisis, and have stated my belief on that subject; viz., that if a person spit blood who has received no injury of the chest, in whom the uterine functions are healthy and right, and who has no disease of the heart, the odds that there are tubercles in the lungs of that person are fearfully high.

Excluding cases of amenorrhœa, and of mechanical injury to the thorax, Louis did not meet with a single example of hæmoptysis among twelve hundred patients, except in such as were phthisical.

I touched at the same time upon the question, whether hæmoptysis, which sometimes precedes for a while the manifestation of any other symptoms of consumption, is ever really the *cause* of it, as the old authors maintained. You will understand my persuasion to be that, occurring in connexion with tubercles, pulmonary hæmorrhage is always the *consequence*, and never the *cause*, of their presence in the lung. Andral relates a curious case, from which the contrary opinion might be argued. "A man, ill of chronic peritonitis, had been for nearly two months in La Charité, and had never presented any morbid symptom which had relation to the organs of respiration. He had no cough, and he breathed easily. One evening, for the first time, he suffered some dyspnœa; and in the course of that night he spat up a large quantity of florid and frothy blood. For the five following days the hæmoptysis continued abundant, then it diminished by degrees, and at length stopped. But the patient continued to cough, and to breathe with difficulty, and at length he died. In the right lung there were found several masses of a brownish red colour, exactly circumscribed, and constituting, in short, that condition which Laennec has called 'pulmonary apoplexy.' One of these masses contained a considerable number of granulations of a yellowish white colour, and having all the characters of minute tubercles in an early state. Two others of the red masses contained each a very small number of these white granules; and in the remaining masses no tubercles at all could be discovered, nor was there any trace of them in other parts of the lungs; but they were numerous in the false membranes of the peritoneum."

Andral argues, that in this case the partial collections of blood that were found in the lung could not have been *occasioned* by the presence of tubercles, because in the majority of these masses no traces of tubercular matter could be perceived. On the other hand, their existence appears *connected* with that of the apoplectic masses, because, except in the midst of some of these, no pulmonary tubercles could be seen. But such a case as this hardly bears out the conclusion that pulmonary hæmorrhage is ever the cause of tubercles. There were tubercles in the abdomen before; therefore, the disposition to tubercular disease pre-existed in this individual; and then tubercular matter was deposited in the places where blood was extravasated; just as we know it is deposited in the blood itself, in the spleen sometimes; or, what I think more probable still, the cluster of granulations provoked the hæmorrhage from the spot they occupied, and other lobules of the same lung became blocked up by the reflux of blood, in the manner formerly explained.

Prior to the age of fifteen, hæmoptysis, even in phthisical children, is extremely uncommon.

Dyspnœa is not a very important symptom in phthisis. It is seldom extreme till towards the termination of the disease, and not always then. Patients who fear, and yet are unwilling to believe, that they are consumptive, will fetch a deep breath, and bid you remark how thoroughly they can distend their lungs; and they expect you to say that there can be no disease in those organs. I have been told that the late Dr. Baillie died of pulmonary phthisis; and that even he was accustomed to delude himself by this test. However, if phthisical persons do not in general suffer much from dyspnœa, their breathing, although they may not be aware of it, or may not choose to acknowledge it, is generally, in some degree or other, short, or hurried. You may wonder that a disorder in which so large a portion of the breathing apparatus is so often effectually spoiled, should be attended by so little distress in respiration; so little dyspnœa: but your surprise will be diminished if you consider the insufficient manner in which consumptive patients are nourished, in consequence of abdominal disease; and the extent to which their blood is wasted by diarrhœa, and by perspiration. The mass of blood is thus kept down to that measure which, passing through the still pervious portions of the lungs, is capable of being arterialized without any great deviation from the ordinary mode and frequency of breathing.

Neither is *pain* of the chest a very important symptom in consumption. In some cases severe pains are complained of, resembling those of rheumatism; in the sides, or beneath the clavicles. In others, no pain at all is experienced. When sharp pain

occurs, it may be supposed that the pleura is inflamed and beginning to adhere in the painful part.

There is, however, one contingency of which the two symptoms last mentioned are sometimes very significant. When, during the progress of phthisis, violent pain of the side, and extreme dyspnoea and anxiety, set in *suddenly*, they denote, with much certainty, perforation of the pleura, and its serious consequences.

The *hectic fever* which accompanies phthisis is of much greater moment. It often creeps upon the patient insidiously. He feels chilly perhaps, towards evening; and in the night his hands and feet are dry and burning; and in the morning he perspires. The most marked symptoms of the hectic are to be found in the perspiration, and in the state of the pulse. The perspiration is usually out of all proportion to the previous chilliness and dry heat. It seems to have a close connexion with the *sleep* of the patient: it seldom comes on while he continues to be awake; but after sleeping he wakes, and finds that he is sweating. The perspiration is generally most copious on the upper part of the body, the chest and head. Sometimes it is moderate; sometimes the patient is drenched and drowned in it. There is a good deal of uncertainty about this symptom, and of obscurity as to its cause. Generally speaking, it belongs to the more advanced stages of phthisis; but occasionally it accompanies its early periods. It will cease without any apparent cause: and recur again with the same capriciousness. A poor friend of mine, who died of phthisis, and was particularly harassed by the nocturnal perspirations, took it into his head that *posture* had something to do with them; and slept for several nights in succession *sitting* in an easy chair: and during those nights he certainly did not sweat, though he had been doing so, profusely, before. Louis found that one patient in ten escaped this symptom.

This is a symptom which is often very distressing to the patient, making him even dread to go to sleep; it tends also to the rapid exhaustion of his strength; and betokens, it is believed, when copious or persistent, a short duration of the disease.

Frequency of *pulse* is a symptom so generally present in tubercular phthisis, that too much importance has been ascribed to it as a diagnostic sign. I mean, it has been too much the opinion that the lungs are safe, when the pulse does not rise above its natural standard. Sometimes it remains steady to that standard nearly up to the period of dissolution. Such cases are, I believe, generally slow in their progress. Very recently I lost a friend whose lungs were full of cavities and crude tubercles. He had been a valetudinarian for years; but the pulmonary disorder had been manifested by decided symptoms during a few months only. At no period did his pulse exceed sixty-eight beats in the minute. Commonly, however, the pulse is continually above ninety; and often it is much higher. When there is nothing to account for this increased frequency of pulse it is a suspicious symptom.

Diarrhœa is a common and an ugly symptom in phthisis. When it occurs early, as it sometimes does; when a patient having habitually costive bowels, becomes habitually relaxed; and you *suspect* only, from other causes, that he may have incipient phthisis; this change often sets its seal upon the nature of his disorder. Usually, however, diarrhœa does not become urgent until the disease is far advanced, and has already declared itself by other and unequivocal symptoms. When it so occurs, it is apt to harass the patient exceedingly; and rapidly to waste his strength and flesh. He appears to melt away under the influence of the purging; which is therefore said to be *colliquative*. It used to be held that the diarrhœa and the perspiration bore an inverse ratio to each other: that when one of them abated, the other always increased. But the more exact observations of Louis and of others have proved that this is not so: that neither in phthisis, nor in other diseases, have these symptoms any such regular reciprocal relation. One reason, perhaps, for this error, may be found in the circumstance, that acids, which have the effect often of checking the perspiration, tend also, in some persons, to produce diarrhœa. Louis found that this symptom began early in the disease, and continued through its whole course, in one out of every eight patients; and in one case only in every twenty-two was it wholly wanting. It depends most commonly, if not always, upon serofulous ulceration in the small intestines and in the colon. In Louis' experience, there were, invariably, *large* ulcers, whenever the diarrhœa had been chronic and abiding, and the stools had been numerous. In the small intestines the ulceration evidently commences in the mucous follicles; the glandulæ solitariae, or the glandulæ agminatæ; and sometimes, though not often, the ulcer perforates the bowel. It is probable that in the large intestine ulceration begins

in the same way, by the deposit of tubercular matter (which is subsequently removed) in the solitary glands: but when once begun, the ulcerating process extends itself indefinitely to the surrounding mucous membrane.

I should have stated before that, with this disease of the intestinal canal, there is often found enlargement of the corresponding glands of the mesentery, which are frequently filled also with tubercular matter.

Several of the symptoms that I have been mentioning — the state of the digestive organs, which interferes with the due assimilation of the food; the drain implied in the profuse sweats, and in the habitual diarrhœa; — conduce to cause another constant accompaniment of progressive phthisis: and that is *emaciation*. You know that the wasting in this complaint, when it is not cut short by some accidental complication before it has reached what may be called its natural termination, is extreme. It often is one of the earliest, as it is one of the most alarming, of the symptoms which the patient presents: and it frequently becomes excessive before any perspiration or purging have occurred to account for it. If, without any apparent cause, a person grow thin and weak, and his pulse be quick, and his breath at all short — these are intimations which seldom prove unfaithful, that tubercular disease is at work in the lungs, and in the abdomen.

Œdema of the ankles, and even some puffiness of the hands and face, are circumstances which seldom fail to appear in pulmonary consumption: but they are among the latest of the symptoms. *Œdema* does not tell us what the disease is in such cases. We have been satisfied as to that some time before. But (unless there is some marked disease of the heart) it tells us that the disease is about to terminate. It is worth attention as a prognostic symptom merely.

And the same may be said of *aphthæ*. This is one of the last of the symptoms: but in some cases it does not happen at all. I have lately described this morbid condition of the mucous membrane of the mouth and tongue, and have nothing more to say of it at present. It bears the same relation to phthisis as to other chronic disorders: and marks, for the most part, the approach of their fatal termination.

It is always interesting to couple changes of structure with their appropriate signs. I will therefore take this opportunity of telling you what Louis has observed of this relation, in respect to the larynx and windpipe.

Ulceration of the epiglottis was often latent; gave no appreciable signal of its existence. The symptoms that belong to it are, a raw, or pricking, or burning sensation, at the upper part of the thyreoid cartilage, with occasional dysphagia, and the rejection of liquids through the nose, while the tonsils and pharynx present no visible alteration.

Ulceration of the interior of the larynx is marked, when slight, by trivial pain in that part, and some variation from the natural voice; when deep, by severer pain, and abiding aphonia.

Ulceration of the trachea is seldom revealed by any symptom. And this is worth remembering; for patients are continually persuaded by medical men who know no better, that their symptoms are all *tracheal*.

There are still a few other circumstances which, when they occur, accumulate conviction as to the nature of the disorder. The catamenia are suspended in women: and the hair falls off. There are certain physical peculiarities too, which are strongly indicative of a tendency to consumption; or perhaps I should say of the scrofulous diathesis. Largeness of the pupil, with a sluggish iris — in other words, a not very sensible retina — constitutes one of these. A clubbed state of the ends of the fingers, with convex and adunque nails, forms another. Yet this last is not peculiar to tubercular consumption. I have heard of one case in which it was strongly marked: the patient died after a long illness — chronic puriform discharge from the pleura after paracentesis thoracis: but there were no tubercles. And I have recently (1857) been consulted by a gentleman whose right chest I found flattened and much contracted, and marked at its lower part in front by the scar of a puncture through which matter was discharged four or five years ago. He was still coughing up thick and puriform mucus. The ends of his fingers were remarkably bulbous, and their nails very convex. They had naturally (he assured me) been delicate and taper: and he had watched the change in their shape, which commenced with the commencement of his chest symptoms. This peculiarity seems therefore a sign of present disease, rather than of a diathesis.

LECTURE LVII.

Phthisis, continued. Diagnosis. Forms and varieties of Phthisis. Ordinary duration. Age at which it is most frequently fatal. Influence of sex; and of occupation. Question of Contagion. Treatment.

IN a former lecture, the twelfth of this course, I entered somewhat fully into the *pathology* of scrofulous and tubercular diseases in general. I pointed out the fact, that though such diseases affect vast numbers of persons, and are most extensively fatal, yet that they affect almost exclusively certain *classes* of persons. That while some are so prone to tubercular disease, as to fall into it upon the operation of the slightest external causes, or even spontaneously — nay, in spite of every care to the contrary — others again, who are constantly exposed to influences likely to call scrofulous disease into action, either do not suffer therefrom; or if they do become scrofulous, it is only when the external circumstances most favourable to the production of such disease have been intense in degree, and protracted in their application.

At the same time I showed you how commonly the *disposition* to scrofula descends in families: and I told you what observation has collected in respect to the *causes* which may *excite* scrofulous disease in persons *hereditarily disposed* to it. I shall not, therefore, go over that ground again. What I then said of scrofulous disease in general is true of tubercular consumption in particular. I will merely remind you that these exciting causes are essentially causes of *debility*. Whatever tends to depress the vital powers, and permanently to weaken the body, tends also, in a predisposed frame, to engender or to call forth this fearful and most destructive malady.

With respect to the detection of tubercular disease in the lungs, it is sometimes very easy, sometimes extremely difficult. It is easy when the tubercles are numerous, large, or far advanced: difficult, sometimes, when they are crude, scanty in number, and thinly scattered, and individually small. In the latter case they may not cause any appreciable deviation from the natural resonance of the chest on percussion, or from the natural smooth, equable rustle of the breathing. It would be tedious to travel over again all the auscultatory and other symptoms, with the view of pointing out their bearing upon the diagnosis. I touched upon that point incidentally, when discussing the individual symptoms, in the last lecture. Many of the symptoms tell their story so plainly that any attempt to expound or interpret them would be quite superfluous. One or two cardinal points, however, which have rather been hinted at before than expressed, I may just advert to.

The fact that tubercles occupy the *upper part* of the lung by preference, is of great moment in relation to the diagnosis. When the symptoms are equivocal; when, so far as *they* are concerned, the case may either be one of chronic bronchitis, or of tubercular consumption; a careful examination of the superior regions of the chest will often decide the anxious question. The sound resulting from the first gentle tap upon or beneath the clavicle, often rings in the physician's ear the knell of his unfortunate patient. Even unusual distinctness of the sound of *expiration*, if heard at the summit of the lung, and *à fortiori* if at the summit of one lung only, warrants the terrible suspicion that tubercles are breeding in that lung. It may, indeed, be laid down as a rule, which very few exceptions diversify, that if you find dulness on percussion; or indistinct breathing; or coarse inspiration; or loud and prolonged expiration; or undue resonance of the voice; or a click or morbid noise of some sort when the patient respires, or speaks, or coughs; if you find this day after day, and always between the clavicle and the mamma in front, or between the clavicle and upper edge of the scapula, over the top of the shoulder, and nowhere else; and more especially if these deviations from the healthy sounds be limited to one side, or greater on one side than on the other, or different in quality on the two sides; you may set the case down as a case of tubercular phthisis. On the other hand, if in the same parts you still distinguish all the natural sounds of respiration, and can still obtain a clear sound on percussion, you are not to condemn the case, nor to despair of recovery, whatever

its other circumstances may be. The worst symptom certainly, when auscultatory signs are wanting, is hæmoptysis.

Incipient consumption is most liable to be confounded with chronic bronchitis. Yet the leading features of the two are well contrasted. The morbid sounds belonging to chronic bronchitis are chiefly audible in the lower lobes of the lungs: those of phthisis in the upper. Some degree of expectoration attends the cough of bronchitis from the first: the cough of phthisis is often, for a long while, dry. Simple bronchitis is not accompanied by hæmoptysis. The pain that occurs in bronchitis is felt beneath the sternum: in phthisis pain most commonly affects the sides, and the space between the shoulders. It is enough, I trust, to have drawn your attention to these points, without dwelling upon them longer.

Dr. Latham, in the little work which I have several times referred to, has laid down certain distinctions most deserving of your notice, with respect to the various forms of phthisis. This portion of his book is quite original. The facts indeed have long been known: but they have never before, that I am aware of, been made so instructive, by being clearly disposed, and exhibited in their proper bearings.

He first divides phthisis generally into two forms, which he calls *mixed* phthisis and *unmixed* phthisis. And he illustrates what he means by those terms very simply and skilfully. He takes the case of an absorbent gland in the neck, affected with scrofulous disease. The changes which are liable to take place in it are wrought before our eyes: we have the privilege of watching them. Now such a gland will sometimes enlarge, in consequence of the deposition of tubercular matter in its substance: it will grow large and hard without there being any pain, or heat, or redness, observable; and it may remain in that state for weeks, or months, or years.

But in the majority of instances the absorbent gland, after remaining for a certain time in this condition, will undergo, and cause, other changes. Pain, heat, and redness will ensue; the hard gland will soften; the integuments will grow thin, and at length give way; the softened tubercular matter, mingled with pus, will escape; and then the pain and heat and redness—the inflammation, in short—will disappear; and the abscess will heal, leaving behind it nothing more than a slight scar. This process may happen to one such gland; or to more than one *simultaneously*; or to several in *succession*.

In this case there has been no more inflammation than was just enough to accomplish its purpose of removing from the body the tubercular matter. The inflammation has not transgressed what Dr. Latham has called its specific limit.

But again, it may go beyond that limit; it may be both more severe and more extensive than is necessary for the removal of the tubercular matter in the diseased gland. It may pervade the whole neck, giving rise to diffused redness, and swelling, and pain: and the whole of the subcutaneous areolar tissue between the angle of the jaw and the clavicle may be loaded with effused serum and pus.

All this you may see almost any day in the wards or waiting-rooms of a hospital. And Dr. Latham has happily chosen this affection of the cervical glands to elucidate what happens when the tubercular matter is deposited in the *lungs*, where one cannot see the changes it is suffering or producing.

Tubercles in the lungs may remain for an indefinite period of time, in their crude state; never softening at all, or softening only at a very late period. Or they may give rise to just so much of inflammation, and no more, in the pulmonary tissue surrounding them as is sufficient to bring about their own softening and subsequent expulsion. Or, lastly, the tubercles may excite much more inflammation of the lung around them than is requisite for their elimination: inflammation of every degree, and of any extent.

Now to the first two cases, when they occur, he gives the name of *unmixed* phthisis: the third he calls, on the other hand, *mixed* phthisis. We learn from auscultation whether the case be one of mixed or unmixed consumption; *i. e.*, we hear, in the unmixed form, the sounds or the modifications of sound which result from the presence of tubercles or of vomicæ; and we hear these morbid sounds only: in every part of the lung where they are *not* audible, we hear the vesicular murmur of health. But in the mixed form we also hear these sounds. True, and we hear other morbid sounds besides. The tubercular disease is mixed with common inflammation; and we hear the sounds that denote common inflammation of the mucous membrane, or

of the substance of the lung — sibilus, or large or small crepitation — we hear these sounds *mixing* themselves with the sounds which belong to the tubercular affection.

This distinction is of considerable importance, for it concerns the *treatment* of the malady. The tubercular disease, when established, is beyond our power. The inflammation which is incidental to it we may hope to alleviate or to remove. It is in the stage of vomicae that the disease commonly assumes the mixed character; and sometimes the bronchial or vesicular effusion upon which the added sounds depend, may be got rid of by the seasonable application of a few leeches, or of cupping-glasses, or of a blister, or by a moderate bleeding from the arm, and the disease be brought back again, for a time at least, within its specific limits: and the patient be relieved from much distress, and imminent danger. It is upon this principle that Dr. Latham explains the fact that most consumptive patients improve considerably, soon after their admission into the wards of a hospital. The poor are necessarily much exposed to those causes which tend to complicate the tubercular disease. The tubercular disease may as yet be slight and limited; but the superadded mischief, the bronchial and vesicular effusion, may be immense; and this being submitted, often for the first time, to treatment, upon their admission into a hospital, is for a while removed.

Now if we had not the advantage of the method of auscultation, we could not ascertain these differences, nor detect them when they existed. You will perceive, I am sure, their practical importance.

Of course the more ready the surrounding lung is to take on inflammation — in other words, the stronger the disposition in the complaint to assume the mixed character — the more rapidly fatal is it likely to be.

But of the unmixed form of phthisis Dr. Latham has made two interesting varieties: and the truth of the distinctions drawn by him will be more manifest to you, the more you see of this terrible disease. In one of these varieties the lungs are apparently tenanted by a multitude of tubercles, which remain crude and unaltered for a considerable length of time. In the other, successive *crops* of tubercles appear to form: or at any rate the tubercles ripen and are expelled in successive crops: and there may be long intervals between each crop and the next.

Dr. Latham thus describes the former of these two varieties: — “An individual loses the complexion of health, and becomes thin: he coughs a little; but perhaps he has no notable fever, and no constant acceleration of pulse.” Upon auscultation of his chest it is found that there is dulness beneath one or both clavicles, or about one or both scapulæ, and an indistinct respiratory murmur in those parts; but the vesicular breathing is free and perfect in every other part of the lung. Here we have tubercles, crude, and in the upper lobe alone: and this state of things may endure for years, without variation; the patient remaining always a great valetudinarian. “To such a patient (says Dr. Latham) it is a continual puzzle why he does not get well. He consults an infinite number of medical men: and it is remarkable that he gets no comfort or satisfaction from those who understand his disease the best, and the greatest comfort and satisfaction from those who understand nothing about it. Those who know what it is, out of kindness do not tell him the truth, and they cannot asseverate a falsehood stoutly enough to carry any weight with it: whereas they who know nothing about it affirm boldly and unhesitatingly that *it is all stomach*; really believing that the whole and sole disorder is in the stomach, and that it is within the reach of an easy cure.”

But at length — perhaps after a very long period — vomicae are formed; and then the patient sinks rapidly, and his lungs after death are found riddled by cavities and stuffed with tubercles; but every part of them not occupied by tubercles or vomicae is crepitant and healthy. In these cases, disease lingers long in the crude stage; new tubercles are added, probably, year after year: but none of them soften. They do not excite inflammation in the lung around them. You recognise the presence of the tubercular matter by the ear; but there are no vomicae. At last vomicae are formed, many at the same time or in rapid succession, and the patient presently succumbs.

The other variety of unmixed phthisis may be just as protracted as this; but its character and progress differ materially. In the former case the patient's condition was one of invariable ill health; in the one I am about to mention he has fits of ill

health, and fits of comparative good health. He spits for a time considerable quantities of puriform matter, and then ceases from expectorating altogether. He has hectic fever, and then throws it off, and then suffers it again: wastes, and recovers his flesh, and again loses it. You will find such cases common enough; and in these cases the morbid sounds will be correspondent to the symptoms. During the fits of illness you will hear gurgling respiration or gurgling cough at the apex of one or both lungs; and during the fits of good health you will hear cavernous respiration or pectoriloquy in the same parts; but everywhere else you will hear a clear sound of vesicular breathing. Here the tubercular matter excites just enough inflammation around it to achieve its own expulsion, and no more. The lung is destroyed bit by bit. Fresh portions of tubercular matter are deposited; these ripen and soften, and are expectorated, and a vomica is the result; and then there is a period of quiet. And there being still a large portion of each lung to breathe with, the patient regains more health and strength in the intervals of his attacks, than the former patient possessed habitually.

But in this form of unmixed pulmonary consumption, a period at length arrives when the patient does not revert to the former state of apparent health. The quantity of lung that has now been destroyed forbids it. You hear the sounds proper to tubercular disease over a large space, between the clavicle and the mamma, or anywhere about the scapula, on one or both sides; yet still that part of the lung which is free from tubercles and vomicae is pervious and healthy: but the hectic continues, the emaciation increases, and the strength declines; and the fatal consummation arrives.

Of these two varieties of genuine and unmixed consumption, the first is the most hopeless. The tubercles are numerous; they probably go on increasing in number though they do not soften; there is not, and there cannot be, any, even temporary return to health, either real or apparent.

Whereas, where the tubercles come singly, or in successive crops, and rapidly soften, and are expectorated; and where some long time interposes between the crops; the health and strength return, and there is just a chance that no more tubercles may form. It is in this variety of unmixed phthisis that a natural cure, by the contraction and cicatrization of a vomica, may by possibility take place. We cannot expect, we scarcely dare permit ourselves to encourage hope, that the disease *will* cease in that manner: but if it cease in any form of the malady, it is in this.

[In regard to the curability of pulmonary consumption, Dr. Wood, in his Treatise on the Practice of Medicine, remarks, as follows:—

“I am not one of those who believe that phthisis is in all cases necessarily fatal. On the contrary, I believe that, in one stage or another, it is occasionally cured, or at least ends in perfect recovery. It is no very unfrequent event to see threatening symptoms of phthisis give way under suitable treatment. It cannot be proved, with absolute certainty, that these symptoms were tuberculous; because the evidence of dissection is wanting, and the physical signs are not sufficiently positive, in mild cases of early phthisis, to authorize a certain conclusion. But they are undistinguishable from symptoms, which, in other cases, are the forerunners of confirmed phthisis; and we have abundant evidence from dissection, that tubercles are capable of undergoing favourable modifications. The probability is, that the tuberculous matter is absorbed, and sometimes, as shown by dissection, replaced by calcareous matter; and, if the diathesis be so far subdued as to prevent the deposition of other tubercles, before these have completely run their course, the disease may be said to be cured. The circumstance that such remains of tubercles are not unfrequently found in the lungs of old persons, who have died of other diseases, would seem to show, that these cures are sometimes permanent.

“But this is not all. It occasionally happens, that consumptive symptoms disappear entirely even in the second stage of the disease, after the formation of a cavity. This event, it is true, is comparatively rare; but some such cases have probably fallen within the notice of almost every practitioner of extensive experience. Even should the disease ultimately return, still, the case may be said to have been cured; as the occurrence of a second attack of pneumonia is certainly no proof that the first was not cured. But there have been cases in which no return of the symptoms has taken place during the residue of life, even though considerably protracted. Two instances of this kind have occurred in medical men of this city. One of the patients was

affected, when a young man, with all the symptoms of phthisis, including frequent attacks of hæmoptysis, severe cough, hectic fever, &c., from which he completely recovered, and continued exempt up to the time of his death, which occurred many years afterwards of typhoid fever. (See *N. Am. Med. and Surg. Journ.*, viii. 277). The other was my preceptor and friend, the late Dr. Joseph Parrish, who in early life laboured for a time under the symptoms of phthisis, and after his death, at an advanced age, was found to have several cicatrices of the upper part of one lung, which were obviously the remains of tuberculous cavities. (See *Am. Jour. of Med. Sci.*, xxvi. 256). The probabilities upon the whole are, that each tubercle has a tendency towards health, and, if alone, would in time end in perfect recovery; so that the great fatality in phthisis consists in the continued predisposition which causes the constant or frequently repeated deposition of other tubercles, before those first deposited have had time to run a favourable course. We may, therefore, always entertain some hope, if applied to in the early stage, in cases of no great severity, of seeing a cure effected; and, even in the second stage, when the diathesis is not very strong, or the local disease extensive, there is no reason for absolute despair. Even in cases which appear to offer no chance of ultimate recovery, we may hope to be able very much to prolong the duration of the complaint, and sometimes even to add years to a valuable life. There is an individual now pursuing an active business in Philadelphia, though with a cavity in his lung, who eight years since was under my care with severe cough, copious purulent expectoration, night-sweats, hectic paroxysms, and great emaciation and debility. When the second stage is clearly established, with severe constitutional symptoms, and the physical signs of extensive disease in the lungs, there is scarcely any ground for hope. If to the ordinary symptoms, in such instances, are superadded continued vomiting and diarrhœa, the case may be considered as quite desperate; and speedy death may be expected." — C.]

This grouping of the different characters under which pulmonary consumption may appear, has been performed by Dr. Latham with perfect fidelity. There is nothing overstrained or fanciful about his sketch; it is after nature; and it is by the hand of a master. And there is something very refreshing in original views of this kind. Vastly more instructive too they are, than those presented by a dull compilation. I therefore again recommend you to study his little volume. I am sure that I have derived much useful and usable knowledge from it: and so also may you.

There is another form still of tubercular consumption which Dr. Latham has not omitted to notice; but his observations on this form are not so new. It is a striking, but not very common form; and it is sure to arrest the attention of the practitioner when it does occur. I have met with three or four examples of it. The phenomena are of this kind. The patient has difficulty of breathing, cough, hæmoptysis perhaps, night-sweats, and much hectic fever; the symptoms in short which constitute the acute phthisis of some authors. But if you listen to the chest, you do not hear the sounds that are peculiar to phthisis: you do not find dulness confined to the upper lobes, or pectoriloquy, or gurgling respiration: but you rather find the superadded sounds which accompany mixed phthisis; small crepitation all over the lungs, succeeded by an absence or deficiency of the proper breathing everywhere. Meanwhile there will be none of the expectoration which is characteristic of phthisis. In short, you would not suppose that the disease was phthisis at all. Yet it must be called such, for after death you find the lungs thickly bestrewed everywhere with what I spoke of before as the granulations of Bayle; nascent tubercles, myriads of them, grey and minute: what many persons call miliar tubercles. The tubercular matter, from some cause whereof we know nothing, is thickly and uniformly sown over the whole of the air-passages, or throughout the entire extent of the lungs, and its sudden presence there in such abundance excites inflammation, which masks and conceals the specific disease; and the true nature of the case is not suspected till after the patient's death. All the instances that I have seen — three or four only in number — were supposed by me to be cases of extensive inflammation of the lungs; and so indeed they were, but they were something more. The tubercles, doubtless, were the cause of the inflammation; and not the inflammation the cause of the tubercles.

From what I have been stating you will perceive how difficult it is to say what is the ordinary *duration* of phthisis; concerning which a question was put to me at the

close of the last lecture. The disease may be present for some time without declaring itself by any marked or unequivocal symptoms; and therefore without attracting attention. But taking the cases as they occur, and estimating the duration of the malady from the time when it first manifests itself in a decided form, we find there is quite enough of variation to warrant the distinction that has been made by authors between chronic and acute phthisis; or, to use the more popular and more expressive phraseology, between slow and galloping consumption. The following tabular statement of the results observed by Bayle and Louis, will give you a somewhat more precise notion of the general progress and duration of the disease. The whole number of cases noted was 314. Of these, 24 died within three months; 69 between three and six months; 69 also between six and nine months; 32 between nine and twelve months; 43 between twelve and eighteen months; 30 within from eighteen months to two years; 12 between two and three years; 11 between three and four years; 5 between four and five years; 1 between five and six years; 3 between six and seven years; 1 between seven and eight years; 3 between eight and ten years; and 11 between ten and forty years.

You will remark that, so far as this account goes, more than one-half of the whole number died within nine months from the time when the disease first became manifest. This agrees with the experience of the late Dr. Gregory, of Edinburgh. He used to state that the ordinary duration of phthisis was about six months; that sometimes it lasted only two or three months; and that he had seen one case which proved fatal on the seventeenth day after the symptoms were first observed. On the other hand, he had known one man who was at least 72 years old when he died, in whom symptoms of phthisis first appeared at the age of 18, and who was never free from them during all the intervening period; being often hectic, and frequently spitting blood. It has been my melancholy task to watch the long decline, and the death at last, of a statesman who served his country well and strenuously, yet of whose years and health a precisely similar description to this would be true. The *average* or *mean* duration of consumption has been computed to be about two years. This is a different thing, you will please to observe, from its *ordinary* duration.

There are many other points in the statistical history of phthisis well worthy of attention and inquiry; but I have not time to go into them in any other than a summary manner. This part of the subject is very well worked up in Sir James Clark's lucid and sensible book upon Consumption: but you will have perceived, from the references I have so frequently had occasion to make to M. Louis, that *his* work is the great storehouse or treasury of tabular information, with respect to the facts of tubercular phthisis.

It is an interesting question to determine at what period of human life consumption numbers the most victims. There are two short tables, one drawn up by Louis, containing observations relative to 123 cases, and the other by Bayle, respecting 100 — which throw some light on the question. The two tables agree, in the main, very closely. Thus, from the age of 15 to that of 20, Louis met with 11 deaths from phthisis, Bayle with 10; from 20 to 30, Louis met with 39, and Bayle 23; from 30 to 40, Louis 33, Bayle 23; from 40 to 50, Louis 23, Bayle 21; from 50 to 60, Louis 12, Bayle 15; from 60 to 70, Louis 5, Bayle 8. You see from this account how erroneous the common notion is, that consumption does not occur at an advanced period of life: that a person who has reached his thirtieth or fortieth year is thenceforth safe from that disease. From these two tables, and others collected by Sir James Clark, it appears that, taking decennial periods, the greatest number of deaths from phthisis happens between the ages of 20 and 30: the next greatest number from 30 to 40: the next from 40 to 50: and that, after these, it is a doubtful matter whether more perish of consumption between 50 and 60, or between 15 and 20, which last is only one-half of a decennial period. These calculations refer, as you will remark, to human life after the age of puberty. Before that age, tubercular disease is fearfully common, especially in infancy and childhood. Among 920 children (532 girls, and 388 boys) who died from the age of 2 to that of 15 years, no less than 538 (nearly three-fifths of the whole) were affected, Dr. Papavoine tells us, with tubercles.

In a report made by the physicians to the Hospital for Consumption at Brompton, the decennial periods are calculated from a different point. Their tables, which com-

prehend the cases of 2679 males and 1679 females, lead to the conclusion that, in both sexes, the "*liability to consumption* is greatest" between 25 and 35 years of age.

From the same tables, as well as from those of the Registrar-General, it appears that in *this metropolis* more men than women die of consumption. In the provinces, according to the Registrar-General's returns, there are more deaths from phthisis among women than among men.

Statistical researches are of still greater interest, perhaps, when they elucidate the influence of different trades and occupations in calling phthisis into existence. Sir James Clark has brought together much curious information on this point. There are certain occupations which appear to provoke pulmonary consumption by the direct application of local irritants to the lungs themselves: and there are others which tend indirectly to bring on phthisis, by lowering the tone of the general health: by producing debility and cachexia. But these two causes often go together: and it is difficult to estimate with accuracy their separate effect. The workmen whose employments have a directly irritating operation upon the respiratory organs, are stone-masons, miners, coal-heavers, flax dressers, brass and steel-polishers, metal-grinders, needle-pointers; and many others who of necessity inhale during their labour an atmosphere loaded with irritating particles of matter. But, then, most of these men work also in towns, and remain for many hours day after day in a constrained position, in crowded or in close apartments. Moreover, some of these occupations, being sedentary, and requiring no great muscular power, are unfortunately selected, for that reason, by persons who are naturally of feeble or delicate constitution. On the other hand, butchers, fishermen, and their families, and farm-servants, are said to be comparatively free from phthisis. Beddoes ascribed this exemption to the use of animal food by these classes: but much of their better health is due, no doubt, to their habits of active exercise in the open air; and to the circumstance that such employments demand a certain amount of bodily strength and energy, and therefore are not likely to be adopted by weak and scrofulous individuals. It is obvious that the whole inquiry is beset with sources of fallacy. We know, however, on the evidence of undoubted facts, that certain occupations do tend to induce pectoral complaints, and to shorten life. Dr. Knight, of Sheffield, informs us that fork-grinders, who are what are called dry grinders, die there of the *grinder's asthma* or *grinder's rot*, before they are thirty-two years old. Razor-grinders, who grind wet and dry, live a little longer: the moisture diminishes, of course, the number of floating particles of metal. Table-knife grinders work on wet stones, and survive till they are between forty and fifty. I must refer you to Sir James Clark's book for similar facts with respect to the inhalation of silex, of the dust of mines, and so forth. Without pretending to assign to each alleged injurious influence its precise contribution of mischievous effect, we must be content, at present, with the practical inference, that such employments should, if possible, be avoided by all those who show any tendency to scrofulous disease.

Is phthisis *contagious*? No: I verily believe it is not. A diathesis is not communicable from person to person. Neither can the disease be easily (if at all) generated in a sound constitution. Nor is it ever imparted, in my opinion, even by one scrofulous individual to another. Yet in Italy a consumptive patient could not be more dreaded and shunned if he had the plague. And in this country the suspicion will now and then arise that the disease may be infectious. A girl dying of phthisis is nursed by her sister, who afterwards droops and dies of the same complaint. Here the presence of the peculiar diathesis is strongly presumable. But the parties may be different in blood. A wife watches the death-bed of her consumptive husband; and presently sinks herself under consumption: and there may be no traceable or acknowledged example of scrofula in her pedigree. Yet even here the latent diathesis may fairly be presumed to have existed. Very few families are perfectly pure from the strumous intermixture. The predisposition may be slight; it may be dormant for a generation; or, like other inherited peculiarities, it may alight capriciously on some individuals only of the kindred. In both the supposed cases there have been other influences at work, more authentic than the alleged contagious property, in calling forth the fatal malady. Watching, the want of rest, confinement in the unwholesome air of a sick chamber; and, above all, protracted mental anxiety, than which no single cause perhaps has more power to foster and forward the inbred

tendency to phthisis. The disorder, I am satisfied, does not spread by contagion. Nevertheless, if consulted on the subject, I should, for obvious reasons, dissuade the occupation of the same bed, or even of the same sleeping apartment, by two persons, one of whom was known to labour under pulmonary consumption.

The *treatment* to be adopted, and the plan of regimen to be observed, in respect of tubercular phthisis, resolve themselves into the methods of *prevention* when the disease is *likely to occur*; of *arresting its progress* when that disease is *incipient or limited in extent*; and of *alleviating the most distressing symptoms*, when no hope remains of stopping its course, or of averting its fatal close.

With regard to the *prevention* of the disease, in those, who, by inheritance, or by circumstances, are predisposed to it, a great deal might be said; but the subject belongs rather to the head of medical police, or hygiene, than to the practice of physic. We deem that a person *has* that predisposition, which is almost a necessary condition of the development of tubercular disease, when we observe those marks of the scrofulous diathesis which I pointed out in an earlier part of the course: or when we know that the parents possess that peculiarity of constitution: or when brothers or sisters have displayed it. It would be well indeed for society if the multiplication, and diffusion, of the strumous diathesis could be checked, by a prudent avoidance of ill-assorted marriages. But we cannot say — no legislature could say — to a scrofulous man or woman, you shall not marry, and propagate scrofula. It is reasonable, however, to conclude, and the conclusion is amply borne out by the observation of facts, that where both parents are strumous, the child will, in all probability, be doubly so; or that, at any rate, its chance of escaping the scrofulous disposition will be small. It is very desirable, therefore, that correct notions on these subjects should be generally prevalent: and that persons who are conscious that scrofula in any of its shapes exists in their family, and *à fortiori* they who know that it exists in their own corporal frame, should avoid allying themselves with persons who are in the same predicament: and this prudence might be enforced if they could be made to foresee the suffering and misery its neglect is calculated to inflict upon their offspring. Intermarriages of persons of the same family, when that family is subject to tubercular disease, are earnestly to be deprecated. But on these points our advice is seldom asked.

We are liable, however, to be consulted respecting the mode of *warding off* scrofulous disease in those who have derived a hereditary tendency to it from their ancestors. Now the first and most effectual prophylactic in such cases, is residence in a mild, and dry, and equable climate: and next to that is the avoidance of all causes likely to foster the morbid tendency. I need not repeat what I formerly told you on this subject. Pure air; nourishing, but unstimulating food; moderate exercise; early hours; cleanliness; warm clothing; and abstinence from excessive study, from severe bodily toil, from occupations in their nature unwholesome, from such callings also and employments as are fertile of care and anxiety, and from vicious and exhausting indulgences of all kinds: these are the topics upon which we must insist, when our advice is sought for respecting the means of preventing consumption in children or others, who are in danger of contracting it.

But when the disease is *present* — when tubercles actually exist, and are ascertained to exist in the lungs — may the progress of the disorder be ever suspended by a change of climate? Indeed I believe it may: but only in certain cases, and in certain stages of the disease. When phthisis occurs in either of its slow and unmixed forms, the question of a change of sky will be worth entertaining. In that form in which tubercles remain long in the crude state, I believe life may be preserved or lengthened by leaving this country, and residing under a higher and more equable temperature, provided that no softening of the tubercular matter has yet taken place. And in the other form — when a vomica or vomicae have occurred, and the strength is apparently restored, and the remainder of the lungs gives out the sounds of health — in that case also I would recommend a voyage to a milder and less changeable climate to those persons who could afford to migrate, and to whom it was a matter of importance that they should prolong their earthly existence. I believe there is no place to which such persons could go with more hope of benefit than to Madeira. Pau in the Pyrennees, Malaga on the east of Spain, Egypt, have each their advantages for phthisical patients. Madeira may be regarded as the type of the moister

and more *soothing*—Egypt of the drier and more *bracing* climates. There are, however, places on our own coast that offer no ineffectual substitute for warmer lands beyond the sea, to those who cannot so conveniently expatriate themselves. Hastings, for instance; the Isle of Wight; Clifton; and more especially Torquay, on the coast of Devonshire; and Penzance, or its neighbourhood, in Cornwall. In those southern and sheltered spots the patient may sometimes pass the colder weather of our winter and spring months in comparative security. If, however, the lungs are already in a state of rapid disorganization, no benefit, but on the contrary much inconvenience and useless expense, will result from change of place, unless that place in which the patient is residing be notoriously unhealthy. When I am asked about removal, either to another country, or to some distant part of our own, and the state of the patient is such as I have just alluded to, I always advise that he should *not* forego the comforts of his home—and leave his family and friends—to seek advantages which he will not find, among strangers, and amid the discomforts of a lodging perhaps, or an incommodious dwelling. I think it wrong, and cruel, to send people away merely to die: and that many are so sent to this place and to that, in the almost certain prospect of their never returning, no one, I think, can doubt.

[It is well remarked by Dr. C. G. Comegys, in a very sensible paper on the Etiology and Curability of Phthisis Pulmonalis, communicated to the Ohio State Medical Society, at its session of 1854, that phthisis pulmonalis is a common disease of all civilized nations; and the remark may be extended, almost as fully, to embrace the people, whether savage or civilized, in all parts of the habitable globe. It does not, however, prevail to the same extent in all countries, and the most secure residences for the predisposed would seem to be those so far north as to escape the greatest vicissitudes of the seasons. Thus in Stockholm, the deaths from pulmonary consumption only amount to one-fifteenth of the entire mortality, while in London, Paris, and Berlin, they amount to almost one-fifth. In some of the German cities, the proportion is also much larger than in St. Petersburg.]

Many writers still hold out the idea that warm climates are comparatively exempt from pulmonary phthisis; but since accurate statistical research has been directed to the investigation of the question, the deaths from the disease have been found to be as common as in higher latitudes. Thus, for the eastern continent, the rate is almost as great at Rome, Naples, Madrid, Lisbon, Marseilles, Malta, the Ionian Isles, and the north of Africa, as it is in England.

From the researches made on our own continent, the same result is established. In fact, according to the statistics collected by the late Dr. Drake, from the reports of the British Army, and the sanitary publications of some of the cities on our seaboard, the proportion of deaths from phthisis is greater in the south than in the north; and we may add that the same general fact is also established by the medical reports of the Army of the United States.

These statistics relate to the West Indies, as well as to the British possessions further north. Statistics collected in Brazil and Martinique, by Drs. Russ, Jobins, and Sigaud, show that consumption of the lungs is as destructive to life in those places as in the United States. In the Island of Cuba, the deaths from phthisis for the City of Havana constitute considerably more than one-fourth of the entire mortality, and for the rural districts upwards of twelve per cent. In the City of New Orleans, La., the deaths from phthisis constitute 9.51 per cent. of the entire mortality. In neither the Island of Cuba nor in New Orleans, are the deaths from the disease chiefly confined to strangers who visit these places with a view to the recovery of their health, but occur in a very large proportion among the native population.

According to Dr. A. K. Johnson (*Geographical Distribution of Health and Disease*), consumption is rare in the Arctic regions, in Siberia, Iceland, the Faroe Islands, the Orkneys, Shetlands, and Hebrides. And, in confirmation of the opinion that it decreases with decrease of temperature, Fuchs shows, from extensive data, that in Northern Europe it is most prevalent at the level of the sea, and it decreases with elevation to a certain point. At Marseilles, Oldenburg, and Hamburg, near the seaboard, the mortality from this cause is about 25 per cent.; at Eschwege, 496 feet above the sea, it is only 12 per cent.; and at Brotterode, 1800 feet above the sea, only 0.9 per cent.

The subject of the effects of climate on tuberculous disease is very ably examined by Dr. Edwin Lee, of London, in a dissertation which obtained the Fishe Fund prize from the Rhode Island Medical Society. (See *Amer. Journ. Med. Sciences for Apr.*, 1857.) As the general conclusions of the author have a more or less direct bearing upon the subject before us, we present them here entire.

1. Tuberculous disease of the lungs is curable in an indefinite proportion of cases, which proportion would doubtless be greatly increased by the more general employment of climate, and the other hygienic and remedial means, to which recourse has been had up to the present time, only in exceptional cases, frequently when the disease has arrived at too advanced a stage to derive permanent amelioration from the use of any means. Even when a cure is not practicable, the progress of the organic lesion may often be arrested or retarded by the suitable employment of these agents.

2. The formation of tubercle depends, most probably, upon an impoverishment of the blood, characterized more especially by a diminution of the normal amount of its globules, together with an alteration in its composition; occasioned chiefly by deficient activity of the skin—considered as an excrementitious organ—whence substances are retained in the blood which ought to be eliminated from it, some of which, chemical analysis has detected the existence of in tuberculous matter—and also as a supplementary organ to the lungs in the function of respiration.

3. Statistical documents, as well as the investigations of impartial observers, have shown that pulmonary consumption occurs, much more frequently than elsewhere, in countries and localities where a humid state of the atmosphere predominates, and also that it prevails chiefly among those classes of the population who are most exposed to this and other influences which tend to depress the vital powers—particularly the activity of the capillary circulation—and, consequently, to vitiate the blood by suppressing the cutaneous transpiration (sedentary mode of life, prolonged anxiety, grief, and other depressing emotions, &c.).

4. On the other hand, tuberculous diseases are of comparatively rare occurrence in cold and dry climates, where the energy of the circulation and of the cutaneous functions, is maintained by the substantial food, and by the active mode of life of the inhabitants, which suffices to preserve them, in great measure (as respects pulmonary consumption), from the pernicious effects of the inclemency and variations of the weather to which they are continually exposed. Consumption is likewise rare in warm and dry countries, where the inhabitants live a good deal in the open air, and where the insensible perspiration is kept up, without muscular effort, by the influence of the climate. On the other hand, it is frequent among the natives of several countries where the climate is hot and moist (the West Indies, &c.), on account of the relaxation of the system, and of the repression of the insensible perspiration, produced by the combined agency of heat and moisture.

5. In some localities favoured in point of climate, though tubercular phthisis is seldom met with among the inhabitants in general, it may nevertheless be tolerably frequent among those of the lower orders who are exposed to the influence of the anti-hygienic causes which mostly tend to induce tubercular cachexy in all countries. Many individuals, in such localities, as well as soldiers serving at stations where the mortality from consumption is small among the general population, nevertheless die from diseases of the organ of respiration simulating phthisis, which are often erroneously considered as such.

6. The chief indications in the treatment of pulmonary tuberculization by means of climate, are, first, to remedy as far as possible the morbid condition of the blood, which constitutes the cachectic state, and, by this means, to prevent or arrest the formation of the morbid product; and secondly, to allay the general and local excitation occasioned by the organic lesion. These indications are not unfrequently opposed the one to the other, and in many cases the practitioner is obliged to restrict himself to endeavouring to fulfil the second, and to palliate the symptoms by pharmaceutical remedies.

7. Change of air, and a residence, more or less prolonged, in warm countries during the winter—the selection being determined by the particular circumstances of individual cases—ought to be considered as the means best calculated to fulfil the first indication, and should be recommended in all chronic cases as early as possible.

8. The beneficial influence of climate in arresting the progress of pulmonary tuber-

culization, would be so much the more marked in proportion as the disease is recent, and as the patient could be the more speedily removed from the influence of the causes which may have contributed to produce it.

9. The localities which would generally be best suited for the winter residence of patients labouring under pulmonary phthisis, in the early stage, are those which, together with a suitable climate, possess resources for mental occupation and diversion, which would induce them to pass a great portion of their time in the open air, avoiding, however, occasions of fatigue.

10. A prolonged residence in any place where the temperature is very equable and the atmosphere calm, is not advantageous to patients when it is a question to procure the restoration of the blood to its normal state. On the contrary, a moderate agitation of the atmosphere is favourable to them by exciting the insensible perspiration, and by making them, so to speak, breathe by the skin as well as by the lungs.

11. The choice of a climate should be determined by the patient's temperament, the condition of the system, and the more or less advanced state of the disease. In general, warm and dry localities best suit persons of a lymphatic or scrofulous constitution, where the circulation is languid; these are, on the other hand, often too exciting for individuals of a sanguinary or nervous temperament, in whom there is irritability of the air-passages, a disposition to inflammation or to hæmoptysis, with acceleration of the circulation. Such patients would more frequently find themselves better where the atmosphere is somewhat moist, not liable to great transitions, and of which the action is consequently sedation. A similar climate is likewise better adapted to patients in the more advanced stages of the disease, when it is deemed advisable to recommend to these a change of climate.

12. Most persons with pulmonary consumption, who are natives of northerly countries, would be benefited by a residence, during a part or the whole of winter, in a warm climate, even though it were humid — provided the disease were not too much advanced — and from the mere passage from a cold to a milder temperature. Many patients, in whom there exists a state of general or local excitation which requires the employment of sedative remedies, would derive permanent advantage from the action of a warm and moist atmosphere, which would tend to allay irritation and diminish the amount of bronchial exhalations. But the too prolonged influence of such an atmosphere, by relaxing the system, would render most patients liable to an aggravation of the disease, if, on changing the place of their abode, they exposed themselves to the action of a climate having a different character, or to any of the accidental causes of the disease.

13. In many cases of incipient tuberculization, in order to derive a curative action from the influence of climate, we should, so to speak, regulate the doses either of stimulation or of sedation. As the sedative action of an equable temperature and a moist atmosphere, would, in many cases, be advantageous up to a certain point, and afterwards prejudicial; so, in like manner, that of a dry and exciting climate, which may at first have been favourable, when too much prolonged, not unfrequently causes an aggravation of the symptoms, and sometimes a state of general irritability, which, notwithstanding the use of remedies, persists or increases, unless the patient be transferred to a climate more suited to his actual state; which, however, is frequently not practicable.

14. Among the climates most in repute for their efficacy in retarding the progress of pulmonary consumption, there exists a considerable variety with respect to equability of temperature, the state of dryness or moisture of the atmosphere, the degree of warmth, etc. The climates of Upper Egypt, the southeastern coast of Spain, are the most remarkable for their warmth and equability in winter, as well as for the dryness of their atmosphere. To these climates, Hyères, Nice, Menton, Malta, and Naples, approximate nearest as respects dryness, though differing materially in other respects. The West India Islands and Cuba may be mentioned as a type of hot and moist climates. Among the intermediate climates characterized by variable degrees of warmth, equability, and humidity, are Madeira, Algiers, Pau, Pisa, Rome. The three latter have a sedative action, often depressing the vital powers of persons in health, as well as of many invalids.

15. The atmosphere of marshy localities, where endemic intermittent fevers prevail,

is neither preservative nor curative of pulmonary consumption, as has been supposed by some physicians; the disease being tolerably frequent in many of these localities.

16. A residence in the places whose climates are best suited to the particular cases, exerts not only a directly advantageous influence in arresting or retarding the progress of the disease, but likewise, inasmuch as patients are thereby placed under the most favourable hygienic conditions, for promoting the efficiency of remedies which would otherwise be inefficacious.

17. Sea-voyages are often beneficial in the early stage of pulmonary tuberculation, when patients do not labour under urgent symptoms; when the strength is not much diminished; when they have a taste for voyages, and are not likely to be prejudicially affected by the monotonous mode of life usually led at sea, and provided there be no grounds for apprehending the exhaustion frequently produced by sea-sickness in long voyages.

18. The advantage which patients sometimes derive from sea-voyages mainly depends upon the continued renovation of a pure air, which acts as a tonic, promotes the insensible perspiration, and the activity of the other functions of organic life. The saline impregnation of the sea air may possibly somewhat conduce to its strengthening effects, though it has not been demonstrated that a residence near salt-works, and the inhalation of an air strongly impregnated with saline vapour, has been followed by special beneficial effects in cases of consumption.

19. Land-travelling through an agreeable country is better suited to consumptive patients in general than are sea-voyages, because, in addition to the effects produced by renovation of the air, it acts in a favourable manner on the *morale* of those invalids in whom it does not occasion too much fatigue. It can, also, be undertaken in the society of parents or friends. Those who travel by land can stop where they please, and they have within reach the medical assistance which their cases may require. It is only, however, when land-travelling is undertaken in suitable weather, and by easy stages, that it can be expected to be attended with benefit, and with comparatively little risk.

20. The climate of several places possessing mineral springs is very favourable to many patients affected with tubercular disease, in the summer season. The operation of appropriate mineral waters—when these agents are not contraindicated—powerfully tends to improve the quality of the blood when vitiated, and to increase the activity of the various secretions, especially those of the skin. It imparts a salutary impulsion to the movements of the economy, and prepares patients for deriving the greatest possible advantage from the influence of a suitable winter climate.

Dr. Coolidge, in his Statistical Report on the Sickness and Mortality in the Army of the United States, remarks that “the climate of those broad and elevated tablelands which skirt the base of the Rocky Mountains on the east, is especially beneficial to persons suffering from pulmonary disease, or with a scrofulous diathesis. This has been known to the French inhabitants of the Upper Mississippi and Missouri for many years. The reports from the line of posts stretching from the Upper Platte through New Mexico to the Rio Grande, give a smaller proportion of cases of pulmonary disease than those from any other portion of the United States. The air in this region is almost devoid of moisture, there are no sudden changes of temperature; the depressing heats of the eastern summers are never felt; and although in the north the winters are extremely cold, a stimulant and tonic effect is the only result of exposure in the open air. It is of great importance that the climate of this region should be generally known, that the present injudicious course of sending consumptives to the hot, low, and moist coast and islands of the Gulf of Mexico should be abandoned. The towns of New Mexico should be selected as a refuge for those showing a tendency to disease of the lungs, or scrofula; anywhere east of the Rocky Mountains, and west of the region where ‘northerners’ prevail.”

In a recapitulation of the deductions in relation to the influence of climate on patients predisposed to or affected with phthisis, it is shown by an examination of the consolidated table given, that, with the exception of West Point, the lowest ratio of cases of consumption occurs in New Mexico, being only 1.3 per 1000; and the highest in the South Atlantic region, where it is 9.2 per thousand. This agrees with the previous statements of Dr. Forry, based upon similar data, with those drawn from statistics of the British army, and with those of Alexander Keith Johnson (*on the*

Geographical Distribution of Health and Disease), inferred from a still wider examination of medical geography.

The general conclusions of Dr. Coolidge are as follows:—

“*First.* That temperature, considered by itself, does not exert that marked controlling influence upon the development and progress of phthisis which has been attributed to it.

“*Second.* That the most important atmospherical condition for a consumptive is *dryness*.

“*Third.* That next to *dryness* in importance is an *equable* temperature—a temperature uniform for long periods, and not disturbed by sudden or frequent changes. A uniformly *low* temperature is much to be preferred to a uniformly *high* temperature. The worst possible climate for a consumptive is one with long-continued high temperature and a high dew-point.—C.]

You will find a great discrepancy of opinion among authors, and among practitioners with whom you may converse, with respect to the *regimen* which consumptive persons should follow. One man gives all his phthisical patients beef-steaks and porter; another restricts all his to vegetables and asses' milk: and each will boast, and bring forward most triumphant examples, of the *success* of his system. Now it is quite obvious that for a sick person who receives benefit from the one of these plans of diet, the contrary plan could scarcely be otherwise than injurious; and reason at once suggests that there must be some distinction between the cases that get better under the one system, and those that improve under the other. Doubtless, we must have regard to the constitution and habits of the patient; and sometimes trial alone will show which plan is the most appropriate; but I believe the best clue to lead us out of the difficulty will be found in Dr. Latham's division of phthisis into mixed and unmixed. The object is, to sustain the patient's strength without exciting inflammation in his lungs. If, with the specific disease, there be conjoined an inflammatory condition of the pulmonary substance around the tubercles, or of the bronchial membrane; in such cases an antiphlogistic diet is, for the time at least, the proper diet. On the other hand, when the disease manifests no tendency to transgress its specific limits, then the diet should be generous and full: and it may be so without being over-stimulant. Debility, however induced, adds to the disposition to the deposit of tubercular matter; and therefore the debility arising from insufficient nutrition is to be avoided as carefully as is compatible with the other indication, which is, to obviate inflammation of the lung. With these hints you will be able, I trust, to strike the balance between the risk of augmenting the local mischief directly, on the one hand, and that of depressing the general strength, and so increasing the local mischief indirectly, on the other. Milk is a sort of animal diet, and it is both nutritious and unstimulating: therefore milk may, in many cases, form the staple of the food, if the patient like it, and if it agree with him: but there is much variety in this respect in different persons. I repeat, that you will too often find prejudices entertained, on the one side or the other, in regard to the diet proper for consumptive persons: but the commonest error of the two is, I believe, that of reducing the patient's strength by a needless restriction of his nutriment, lest inflammation should ensue.

Louis, should you refer to him, would be likely to lead you into the opposite mistake: for he affirms, that neither bronchitis, nor pneumonia, nor pleurisy, have any effect in exciting tubercular phthisis. But this opinion is entirely opposed to the general sense of most men of experience. Many a case of consumption can be traced back to a severe catarrh, and no further. Many, which ran a short course, were dated, within my own knowledge, from the last visitation of influenza. If M. Louis had meant that thoracic inflammation will not produce tubercles in the lungs of a person who has not the scrofulous diathesis, and that tubercles may and do arise without any previous inflammation, I should quite agree with him. But he draws his conclusions from cases of phthisis. I have no doubt whatever that the dormant predisposition is often awakened into actual disease, and that latent tubercles are often accelerated in their progress, by inflammation of the pulmonary tissues. Whether this happens directly from the local inflammation, or indirectly from its effects in lowering the vital powers, is a question which no one can solve, and of which the solution is

not of much consequence. What we are sure of is, that every one who bears a real or suspected taint of scrofula in his frame, should scrupulously guard against every known and avoidable cause of catarrh, pneumonia, or pleurisy. I hold M. Louis' doctrine on this head to be unsound and unsafe: and I mention it only to admonish you against it.

In offering you a few final observations on the *remedies* of phthisis, I shall take leave to abstain from weighing the pretensions of a number of *specifics*, which have from time to time been highly recommended; but which never have come into general use, as they would have done, no doubt, if they had been entitled to such a denomination.

In the first place we must satisfy ourselves as to the kind of case we have to deal with; whether it be mixed or unmixed. We must watch our patient: and keep him on reduced diet, and take blood in small quantities by leeches or cupping from the chest, whenever marked inflammatory symptoms arise; whether they are discovered by observation of the general or of the physical signs. The bleedings must of course be small—and palliative only of the symptoms.

Emetics, frequently repeated, have been recommended in the early stage of phthisis: partly on account of their reputed efficacy; partly on theoretical grounds; it being supposed that the tubercular matter may be thus removed from the mucous surfaces as fast as it is deposited. Of the value of this emetic plan, I am unable to speak from any experience of my own.

Counter-irritation is often of undoubted service: mustard-poultices to the chest when it is painful; or a blister, or a succession of blisters, or friction with a liniment containing croton oil, to encounter local symptoms. The effect of counter-irritation upon the progress of the tubercular disorder is apparent sometimes by accident. Dr. Abercrombie has related an example in which cerebral disease operated in this way; the previous symptoms of phthisis disappearing. In some cases mania seems to have a similar consequence, obscuring the manifestation, and probably retarding the course, of consumption. It has been often remarked—you will find this stated by Sir B. Brodie—that after amputation of a scrofulous leg, phthisical symptoms, very little noticed before, have rapidly increased. And there is another fact, in relation to phthisis, analogous to these, which it is fit you should know and attend to, viz., that the progress of consumption is often suspended by pregnancy:—and while a mother is suckling her child, if the suckling be not too long continued, so as to exhaust the mother. I suppose there is no doubt that women disposed to phthisis have been kept alive by successive pregnancies and sucklings. It is a very rare thing for a pregnant woman to die of phthisis. I have known only one instance of it. One of my patients in the hospital, a French woman, died of that disease; and we found suppurating tubercles in her lungs; and a fœtus of about five months in her womb.

Riding on horseback has been strongly advised in the earlier periods of the disease. Its main advantage seems to arise from its allowing the enjoyment of fresh air, and of exercise, without putting the patient out of breath: and these advantages are great. It is affirmed that many phthisical patients remain free from cough, and those affected with hæmoptysis cease to spit blood, so long as they continue to take exercise on horseback. Gestation in a carriage, or in a boat, has the same good effects, but in a less degree. We are not able, however, to look upon equitation as so certain a cure in consumption as Sydenham did; who says that riding on horseback is as much a specific for phthisis, as the Peruvian bark is for an ague.

Iodine and its compounds, and especially the iodide of potassium, have been much praised for their reputed efficacy in phthisis. Given in small doses, I believe that they often have a beneficial influence upon the general health. I wish I could tell you that I had ever known them work a cure of the manifested disease.

The oil obtained from the liver of the cod-fish—a substance formerly thought curative of chronic rheumatism—has of late been extensively administered, with unquestionable benefit, in strumous disorders, and especially in pulmonary phthisis. It is not directed against any particular symptom: but appears, in a much greater degree than any other drug that I know of, to be antagonistic of the consuming power of the disease. Unlike many of the oils, it does not generally purge. One of its most obvious and frequent effects is that of hindering the waste of the fatty tissues of the body: and even of promoting, by the excessive supply of that nutriment, a new de-

posit of adipous matter. The patient recovers flesh and weight, resumes a healthier aspect, and acknowledges sensations of returning strength and comfort. Meanwhile his cough is mitigated, he expectorates less, his pulse is reduced in frequency, hectic symptoms disappear, and the auscultatory signs declare a corresponding change for the better in the diseased lung. As bearing upon these ascertained facts, I would direct your attention to some interesting observations by Dr. John Hughes Bennett (to whom we are indebted for the introduction of the oil into this country as a remedy for phthisis), upon the *structural relation of oil and albumen in the animal economy*. They are contained in a paper read before the Royal Society of Edinburgh; and published in the *Monthly Journal of Medical Science* for September, 1847. Dr. Bennett shows that the proper nutrition and healthy organization of the body depend upon the maintenance of a certain relation between the oily and the albuminous principles which enter into its composition: and that in numerous forms of disease, the excess or defect of one or the other of these principles may be distinctly traced. "Tubercle," he remarks, "is a product varying much in constitution, but most frequently composed of an amorphous molecular matter, little altered by the addition of æther, and rendered more transparent by acetic acid." The whole range of morbid changes denominated tubercular, belongs to the class in which there is an excess of the albuminous, and a deficiency of the oleaginous principle. In these speculations do we not obtain some glimpse of the way in which the cod's-liver oil (and other oils, perhaps), may tend to correct, or to keep in check, the strumous disposition? Whatever may be its *modus operandi*, I have often been surprised as well as gratified by the improvement that has followed the use of this remedy in consumptive patients. I believe that the earlier it is resorted to, the better; but in every stage of the disease its healing power has been fully ascertained. In many advanced cases it does no good at all: in some its restorative effect is really wonderful. The proper dose is from two drachms to half an ounce, three times daily. To avoid the risk of blunting the natural appetite, I am in the habit of directing that the oil be taken at bed-time, and soon after each of the two principal meals of the day; in other words, as long as possible before the meal next ensuing. Of the various kinds of oil in use, that which is the purest, that which is obtained by the simplest methods from the fresh liver of the healthy cod-fish, is not only the least nauseous, but in my opinion the most remediate also. It is not so nauseous, patients tell me, as might be supposed.

Upon children, or others, affected with scrofulous swellings of the cervical glands, the beneficial influence of the cod's-liver oil is perhaps still more certain and conspicuous.

Often — too often — all that we can attempt to do is to relieve the most urgent or distressing symptoms: and to make easier the patient's decline. One symptom which is both distressing and weakening is the nocturnal perspiration. The common remedy for this is the dilute sulphuric acid: and a very good remedy it is, but it is not equally adapted to all cases. If the bowels are costive — or if the bowels have not, as they often have, a tendency to be relaxed — then the sulphuric acid may be freely given: and it will often have very good results. It may be exhibited three or four times a day, in doses of from twelve to twenty minims. But when this fails, or when the bowels are irritable and will not bear it, we must have recourse to other means. One of these is sponging the surface of the body, at bed-time, or before the patient settles himself for the night, with tepid vinegar and water: using twice as much water as vinegar. And if the bowels are at the same time purged, I find the compound kino powder of the *Pharmacopœia* an admirable medicine. It certainly has much power over the perspiration; and it has these further advantages, that (containing opium) it tends to control the diarrhœa, and to calm the cough.

Steel is another substance which exercises a marked influence sometimes over the hectic fever. It was its efficacy in this way that gave celebrity to the famous antihetic mixture of Dr. Griffith, the *Mistura Ferri Composita* of the *Pharmacopœia*. Certain it is, that when steel is borne in the advanced stage of consumption, it often does a world of temporary good; — but in many cases it is *not* borne well. It increases the cough, occasions headache, and heat of skin, and distresses instead of relieving the patient. Nor is it always easy to say beforehand, whether it is *likely* to suit the case or not. I apprehend it will at length be found most applicable to the unmixed forms — the uninflamatory forms, that is — of phthisis. I have frequently,

however, succeeded in checking the wasting sweats by the *Tinctura Ferri Murialis*, given in doses of twenty minims thrice a day, after other expedients had failed me.

When the cough is very troublesome, and especially when it breaks the patient's rest at night, we must endeavour to quiet it; and there is no drug, I fear, that we can depend upon for that purpose, but opium. The old paregoric has been, and is, a favourite form for giving opium to appease cough; and old-fashioned apothecaries will tell you that the alteration which was made by leaving the aniseed out of this compound tincture of camphor, in the last Pharmacopœia but one, impaired its efficacy. Whether it was so or not I cannot tell; but Dr. Prout is of opinion that *aniseed* has considerable power in allaying the irritation on which the cough depends. He infuses three drachms, or half an ounce, of the bruised seeds in half a pint of distilled water at a temperature not exceeding 120°; and lets it stand till it is cold. On his strong recommendation I have tried this, as a vehicle for paregoric, when the same dose in other vehicles had failed; and I must say, that it has frequently been followed by a marked abatement of the frequency and violence of the cough. The aniseed is restored in the paregoric of the last Pharmacopœia, that of 1836. Hydrocyanic acid has sometimes a very soothing effect upon this harassing symptom. However, at last, opium will be found our sheet-anchor, not merely for the cough, but for the diarrhœa which is so seldom absent in the latter periods of phthisis. The diarrhœa depends, as I have told you, upon an ulcerated state of the bowels. In those cases in which it could scarcely be kept in check at all, I have always found very extensive ulceration in the large intestines: but the diseased condition is often seated higher up, in the ileum or jejunum. The Physicians to the Brompton Hospital commend bismuth as a remedy for this symptom: the tinctures of catechu and of rhatany are also of much service, combined with laudanum and with the officinal chalk mixture:—or a few grains of the *confectio opii* may be given in peppermint water, after every loose evacuation:—or in obstinate cases, a pill composed of a quarter of a grain of the sulphate of copper, and the same quantity of opium, will often answer well, though it sometimes gripes. I mention these several expedients, for you will often require them all. The injection of a small quantity of starch, as much as the rectum will receive and retain, with ten or twenty drops of laudanum, generally affords the patient most sensible comfort; and suspends the further action of the bowels for a considerable time.

These, I think, are the principal means by which we may endeavour to smooth the pillow of the patient dying of consumption. Sometimes very little pain or distress is felt at all, from first to last; the intellect remains free, and the patients are proverbially sanguine about the issue of their disorder. In other cases, do what we will, the patient suffers greatly. One harassing incidental combination of symptoms is nausea and vomiting. I should have stated before, that when, in phthisis, these symptoms last long, and are accompanied by pain and tenderness of the epigastrium, they denote, almost always, a thinned and softened condition of the mucous membrane of the stomach. They may be alleviated by a leech or two—by a blister—by the effervescing draught: or the prussic acid may be used; that is a medicine which certainly tranquillizes an irritable stomach, as it now and then allays an urgent cough. Sometimes, again, the bones of the miserable patient are laid bare, in consequence of pressure upon parts in which the circulation is already very feeble. We cover these with soap-plaster; take off the pressure by arranging cushions; or, what is much the best of all, we put the patient upon the water-bed, invented by Dr. Arnott.

LECTURE LVIII.

Melanosis of the Lung; true, and spurious. Accidental intrusion of solid substances into the air-passages.

I YESTERDAY adverted to certain callings which are unhealthy for various reasons, and among the rest for this;—that the work-people engaged in them breathe habitually an atmosphere loaded with particles of matter which clog or irritate the pulmonary tissues. There is one morbid condition, so produced, which hitherto, or till lately, has scarcely been mentioned by writers on disease, but which deserves a moment's attention; for although it is very uncommon in many parts of this country, it is by no means rare in some others. The texture of the lungs is spoiled by matters carried in with the air, in the acts of breathing. This morbid state has been called *spurious melanosis*. The lungs are found after death to be throughout of a black colour, more or less uniform. Sometimes the pulmonary substance is dry and friable, as well as black; sometimes moist, œdematous, infiltrated with an inky fluid; not unfrequently broken down into irregular cavities of various sizes; and these cavities are often full of the same black liquor.

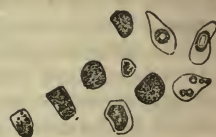
You are not to confound these appearances, when you happen to meet with them —(and as you will probably scatter yourselves, some here and some there, over various parts of the kingdom, some of you are very likely to meet with them)—you must avoid, I say, mistaking these black appearances and products, for *true melanosis*. The disease so denominated is a singular one. It was first fully described and named by Laennec in 1806. It consists in a morbid product, presenting a black or deep brown colour of various degrees of intensity, moist generally, unorganized, and differing in the form it assumes, and in its consistence, according to circumstances. I shall take this opportunity, for I am not likely to have a better, to tell you the little that has been ascertained in regard to this kind of disease; and having done so, I shall revert to a short account of *spurious melanosis*.

These black deposits take place most frequently of all in the areolar tissue, and in the adipous tissue: and they occur in greater abundance, and in larger masses, according as these reticular tissues are more plentiful, and more lax. They are met with also in the compound organs of the body; especially in the liver. Less frequently in the lungs. Sometimes in the eye. Occasionally in the brain. The serous membranes are obnoxious to the same kind of disease; the mucous very little so. The black or dark-coloured matter may also exist, in a liquid condition, in the natural cavities of the body. And lastly, the melanotic material is sometimes mixed up with scirrhous and brain-like malignant tumours.

With respect to the shapes in which it appears—it is sometimes dotted, the surfaces affected by it looking as if they had been thickly sprinkled over with coal-dust or soot. But more commonly melanosis assumes the form of solid tumours, of variable magnitude. These tumours are largest, where reticular tissue is most loose and abundant. They may be no bigger than a pin's head, or they may be as large as a man's head. Masses of this kind have been found in the horse, weighing as much as six-and-thirty pounds. In the human subject they may attain the size of an orange. These large tumours (like large pulmonary tubercles) are usually formed by the union and agglomeration of several smaller ones, and hence they have generally a lobulated surface: while the shape of the separate smaller tumours is mostly spherical. Sometimes the areolar tissue lying around the melanotic masses is condensed into a kind of cyst: more generally the black matter is in naked contact with the tissue, whatever that may be, in which it is lodged.

From the serous surfaces, especially from the pleura and peritoneum, knobs of a dark colour are seen in some instances to project; in others, round tumours, as big

FIG. 74.



Melanotic deposit in cells of an engorged lung. Some of the cells contain oil-drops.

as peas, or cherries, hang from these surfaces by a sort of peduncle. The omentum is a common *habitat* of melanotic tumours.

Occasionally, I say, the black matter is found spread in a continuous layer upon the serous membranes; or is collected in a liquid state in their cavities. But this, compared with the occurrence of solid tumours, is rare.

When this remarkable disease is met with in one tissue or organ of the body, it is met with in others. It is never confined to one part, but pervades several: resembling in this respect both the scrofulous matter which constitutes tubercle; and the matter of cancer. Müller indeed considers melanosis to be a species or variety of carcinoma.

Scattered notices of these singular and striking changes in the animal frame occur in the works of Morgagni and of Haller; but since the period when Laennec first drew the special attention of the profession to the subject, the black matter has been carefully analysed by several expert chemists. Without going into any tiresome detail, which you would scarcely remember, as to its exact composition, it is interesting to know that it is very like that of the blood: and no doubt the material is somehow deposited from the blood. Very little, however, has been ascertained about its primary origin and cause. Some have supposed that the melanotic matter is analogous to the natural pigments which are found in the animal economy; all of which are known to be rich in carbon. It is a curious fact that the disease has been more often observed in white or grey horses than in others. (I should tell you that the complaint is not at all uncommon in various quadrupeds: examples of it have been noted in the horse, ox, dog, cat, rabbit, rat, and mouse.) It has been conjectured that, in white animals, the colouring matter of the surface, and of the hair, has been diverted, by some morbid process, from its proper locality. But the very same disorder occurs also, though not so often, in dark, or bay, horses and cows: and certain pathologists imagine that in these cases there has been an undue accumulation, in the blood, of the carbon which is destined to colour different parts. In the one case, you see, they hold that the pigment is misplaced; in the other that it is excessive. What value these speculations as to the nature and origin of the disease may possess, time alone can determine.

When the tumours are divided, and moist, or when they are rendered moist by admixture with water, they freely impart the colouring matter; staining white paper, and blackening one's fingers, just as Indian ink might do. The disease most frequently happens, when it happens at all, in the decline of life.

The changes to which the melanotic tumours are liable, are very much like the changes which tubercular matter is apt to undergo. In certain situations, where the secreted black material is subject to pressure, and is poured out in a soft consistence, the watery parts are sometimes absorbed, and the mass becomes hard and firm. On the other hand, the pressure occasioned by the tumour sometimes provokes inflammation in the tissues surrounding it; and then it is liable to be broken down, exactly in the same way as that in which tubercles soften prior to their expulsion from the lungs.

The injurious effects of these collections of black matter arise from the pressure they occasion; and they may evidently thus cause pain, irritation, ulceration; and, according to their situation, number, and extent, they may materially interfere with important functions. And in this manner they do, in fact, at length destroy life.

There are no symptoms, that I know of, distinctive of this disease, except the appearance of the black masses upon the surface of the body. Nor can I pretend to point out to you any cure for it, when it has been ascertained to exist.

Yet it is right that you should be aware of what pathologists have learned respecting this curious morbid state; although that be little, and not very satisfactory. And I have introduced this brief consideration of melanosis here, somewhat irregularly perhaps, to enable you to distinguish from it that pulmonary disease to which I referred in the outset of the lecture, and to which I shall now return. This, I say, has been called *spurious melanosis*: and it has doubtless been mistaken for the specific disease of which I have just given you a sketch; for *real* melanosis.

It is, however, a very different affection.

Laennec had conjectured that certain kinds of black discoloration of the lungs were of extraneous origin; were owing to the introduction of black matters from without in the process of respiration: and Mr. Pearson, in this country, had thrown out the same idea. But that this was actually the case was first ascertained in Edinburgh.

Dr. J. C. Gregory had a patient who died in the infirmary of that city, and whose lungs exhibited the following appearances:—They both presented one uniform black carbonaceous colour, which pervaded every part of their substance. The right lung was broken down, in its upper and middle lobes, into irregular cavities; and the walls of these cavities were black; and they contained a considerable quantity of black liquid like ink. Portions of the pulmonary substance were dense, hepatized, and friable. The rest of this lung was œdematous; and when the serum which rendered it thus œdematous was pressed out, it also, the serum I mean, was quite black. The left lung was infiltrated, in the same manner, with black serum. No tubercles could be detected. The bronchial glands were not enlarged, but they were stained of the same sable hue as the substance of the lungs. No other organ of the body presented any trace of this black discoloration.

Dr. Christison, who is known to be a very exact and able chemist, undertook an analysis of the black matter contained in the serum expressed from these lungs. I shall not follow out the details of his researches (you may read them at length in the 109th number of the *Edinburgh Medical and Surgical Journal*), but content myself with stating their result. And I may state it in Dr. Christison's own words. "In the product of this experiment (he says) it is scarcely possible not to recognise the ordinary products of the distillation of coal. A gas of the same quality was procured, and likewise a naphthous fluid holding in solution a crystalline principle, analogous to, if not identified with, naphthaline."

Now the man, whose lungs presented the appearance I have described, had, for the last ten or twelve years of his life, been employed in the coal-mines at Dalkeith. He had been exposed therefore to the habitual inhalation of coal-dust into his lungs in breathing: and taking this circumstance in conjunction with the result of the analysis of the black matter contained in the lungs, and nowhere else throughout the body, we cannot doubt that the carbonaceous substance so abundant in these organs was introduced from without. In truth we have now a large amount of evidence in proof that it must have been so. In the twenty-first volume of the *Medico-Chirurgical Transactions*, Dr. William Thomson has recorded the results of extensive inquiry into the subject. Thus he gives ten examples of black sputa going along with pulmonary symptoms during life, and of black infiltration of the lungs discovered after death; and of the ten persons who were the subjects of these observations, nine had been engaged in working coal-mines, and the tenth was a moulder at the Carron iron-works. He gives also six cases of black infiltration of the lungs, all occurring in persons exposed to the inhalation of carbonaceous matters (one of them was an engineer, and the others were all colliers); but in these six cases there had been no black expectoration noticed during life.

That such carbonaceous particles, floating in the atmosphere, may be, and must be, and actually are, drawn into the lungs during inspiration, no one who has been long in this smoky town can doubt. Many persons remark that they expectorate during winter, and while in London, a little mass or two of dark grey, dirty mucus, every morning; but when in the country, and in the summer, the mucus so spat up is transparent and clean. So I have noticed, and pointed out to some of you, that the *crachoirs* of the patients in the hospital often bear witness that there has been during the previous twelve hours, one of our dense and dirty fogs, which come with an easterly wind, and bring with them a vast quantity of blacks, and soot, and smoke. Immediately after the prevalence of one of those filthy blankets of vapour, we find the contents of each of the little vessels given to the patients whose expectoration is kept for inspection, to be deeply tinged with black. And I had, in the year 1832, a patient whose sputa were remarkably loaded with dark matter. He came into the hospital complaining of cough, and of shortness of breath, and of a sensation at the lower part of the right side of the chest, as if it were pierced by needles; and he was spitting a considerable quantity of thick mucus, which was almost black. This colour never entirely left the expectoration while he remained in the hospital; but it very greatly diminished, in proportion as his ailments were relieved. Now this man was a stoker at one of the gas-works. And he attributed his illness, which had come on gradually, to the great alternations of heat and cold to which his occupation exposed him: and the blackness of the sputa he ascribed to the continual inhalation of coal-dust. And no doubt he was right.

It may seem strange, if the inhalation of atmospheric air loaded with minute particles of coal or other carbonaceous matter, be sufficient to produce this remarkable condition of the lungs, and the characteristic *black spit*, that the change, and its nature and cause, should not have been earlier made out: when we consider the vast number of men who are employed in our mines and collieries. It appears, however, that a great repugnance has existed, and probably still exists, among the labourers in the coal mines, to allowing their dead to be opened and examined. And it appears further that the peculiar state of the lungs which I have been speaking of is produced in a comparatively small number of those who are so employed. The precise cause why some are thus affected, and some are not, has yet to be discovered. Dr. Thomson has circulated among the medical men residing in the coal districts, a list of queries (which he gives in his paper), respecting various points of interest in relation to this pulmonary condition: and we may expect to obtain, by degrees, more exact information about it. I recommend it as an interesting subject of inquiry to such among you as may have opportunities of prosecuting it. It has been conjectured that the specific change takes place, in a marked degree, only in lungs that were previously unsound. It is a question whether the cavities met with in the pulmonary substance, in the fatal cases, were the result of the spurious melanosis; or of the expulsion of tubercular matter which had coexisted with, or preceded, the melanotic state. It is a curious circumstance that the black spit, as it is called in those districts, sometimes does not make its appearance until a considerable period has elapsed after the time when the labour in the coal mines was given up. Dr. Makellar, who has more recently described the disease as he met with it among the colliers in the Lothians, is of opinion that the blood becomes unnaturally loaded with carbon; and that when once a certain quantity of the black matter from without has been lodged in the pulmonary tissues, a disposing affinity is thereby created for the carbon in the blood, so that more and more of that substance continues to be deposited, even long after the patient has abandoned his occupation, and ceased to inhale the noxious atmosphere. Certainly this is a complaint that offers several interesting points of research, and requires further investigation.

You will remark that the spurious melanosis is distinguished from the true, by its occurrence in those persons only who are somehow exposed for a certain length of time to breathe an atmosphere which is largely encumbered with carbonaceous particles; whereas true melanosis may occur in any locality. The spurious discoloration never affects any other organs than the lungs and bronchial glands; the true black deposit of melanosis is never confined to a single organ or tissue. Moreover, the one disorder is absolutely beyond remedy; the other, as soon as its presence is rendered probable, by the black expectoration, and the pulmonary distress, may be mitigated, checked, perhaps gradually cured, by removing the patient from the operation of the exciting cause, and pursuing such other measures as the symptoms may seem to require. The distinction is not a matter therefore of mere curiosity: it bears upon the treatment to be followed, which is our proper business. It is connected also with medical police or hygiene, which we should all of us cultivate as extensively as we may; as a science intimately related to our strictly professional pursuits, and to the welfare of the community.

Other forms of disease, unquestionably cancerous, occasionally infest the lungs. Their chief symptoms, when the malady does not reach the surface of the body, result from the pressure which the carcinomatous masses exercise on the parts in their neighbourhood. I shall postpone a more particular consideration of these effects of intra-thoracic pressure, until I come to aneurismal tumours, which have a similar mechanical influence.

There is yet another affection of the breath-machine, to which I must briefly direct your attention: a casualty that is apt to befall the air-tubes. I said nothing of this, indeed, last year; but having since witnessed an example of the accident to which I allude—the entrance, namely, of some solid substance into the windpipe—I have thus been reminded of my former omission, and taught at the same time the necessity that every medical man should have well considered such cases. I was taken to Kentish Town, in the autumn (1837), by a professional friend, to see a child, into

whose trachea a small nail, what is commonly called a *tack*, was thought to have passed. When I saw the boy, he seemed to have nothing the matter with him : but he had been subject, ever since the accident, to paroxysms of most violent choking cough ; alarming the parents and his attendants for his life. There was good reason for concluding that the nail, which was missing, and which he said he had *swallowed*, had really got into the windpipe, and was still there, or in the lungs ; and the question was much discussed, what ought to be done in such a case ? The result was, that nothing was done : but that after the lapse of several weeks, the nail was at length coughed up.

Now there are some interesting points arising out of this sort of mischance. The instance I have just referred to will probably be published ; and therefore I dwell upon it the less. Dr. Stokes has devoted a short chapter to the consideration of foreign bodies in the air-passages : and examples of that accident are more common than you might suppose.

It is, at first sight, a surprising circumstance, that a solid body of any considerable magnitude (a molar tooth for instance), should be able to pass at all through the narrow chink of the glottis. But, supposing the chink to be plugged by the sudden entrance of a passing substance, just at the commencement of a forcible endeavour to inspire, when, of course, the opening is at the widest, that substance must necessarily sustain, as the chest expands, a strong degree of pressure from the external atmosphere : strong enough, often, to force it through. If you cork a bottle that contains air only, and sink it sufficiently deep in the sea, the pressure of the water will push the cork into the bottle. The condition of the lungs, in the case supposed, and the condition of the bottle, are analogous. A vacuum beyond the plug is attempted by the act of inspiring, and obviated by the displacement of the plug inwards. There are no such powerful forces called into action to drive the intruding substance out again.

The matters which have been actually thus caught in the rima glottidis, and forced through, are, as you might almost expect, oddly various in kind. Morsels of food : the stones of fruit ; of these there are many instances : teeth ; three such cases are referred to by Dr. Stokes : portions of bone : pebbles : a piece of money : a nut : a nut-shell : a button : a musket-ball : a large shot : a fragment of nutmeg : iron nails : kidney-beans : ears of grass or corn ; of these, four examples at least have been noticed ; one is mentioned by Dr. Stokes, two are recorded in the *Gazette Médicale*, and I show you a monument of the fourth, in this interesting preparation, for the history of which I am indebted to Mr. Mayo. The young son of an English nobleman was riding in a carriage, in or near Paris, and had an ear of rye in his mouth. The carriage made a sudden jolt, and the ear of corn disappeared. Little was thought about this at the time : but soon afterwards symptoms of pulmonary irritation set in, attended with hectic fever, and with the most foetid expectoration. The boy gradually sank. The ear of rye lay, as you may perceive, in an abscess which was common to the right lung and to the liver, through the diaphragm.

If any of you have tried the boyish trick of slipping beneath your wristband an ear of bearded corn, you will have no difficulty in understanding how and why, with every movement of the parts in contact with it, the ear will travel onwards ; and how improbable it is that such a substance should ever be expelled from the lungs by coughing. Yet, in one of the cases recorded in the *Gazette Médicale*, by a physician whose sister was the subject of the accident, an ear of barley was so rejected, seven years after its entrance. During that long period she had suffered repeated attacks of copious hæmoptysis. Her recovery was perfect.

The very enumeration which I have just made may convince you that the accident is not a very unfrequent one ; and it is more than probable that fatal cases happen, the nature of which escapes detection.

The results of the accident are various also.

In the first place, it sometimes causes speedy death by apnœa.

2dly. It may be followed by inflammation of the lung, and perhaps abscess ; and destroy life.

3dly. Death may ensue, after symptoms resembling those of chronic phthisis.

4thly. The "foreign body," as we oddly enough call it, may be expelled through the glottis, after a variable period of time. Sometimes, yet not always, its expulsion

is the condition and the harbinger of the patient's recovery; but he is never safe while it remains.

Death may take place in a few seconds when the substance sticks in the glottis. Death *has* occurred within three days, when the substance had passed the glottis; and in eleven days when it had reached the lung. The intruding piece of matter has escaped, through the natural passages, after remaining imprisoned for seventeen years. In that case, the patient died, hectic and emaciated, a year and half afterwards.

It may be worth our while to consider these particulars somewhat more closely; and to inquire what, in different cases, becomes of the foreign body which thus, to use a common phrase, "goes the wrong way;" and what the symptoms are to which it may give rise.

First, then, I say, it may get wedged in the slit of the glottis, and produce immediate suffocation. I mentioned, before, the frequency of this kind of death by misadventure. If you are summoned to any one whom you find comatose, or apparently just dead, and you learn that he had been suddenly attacked with choking during a meal, lose no time in examining his pharynx and gullet. You may chance to save a life so. The accident often happens to persons who are drunk. No doubt it happens oftener than we are aware of. The attack is very likely to be mistaken for an apoplectic seizure.

In these cases of sudden choking, the morsel of food is not always caught in the rima glottidis. If it be large enough to stick fast in the *pharynx*, it may provoke, through a reflex action, an abiding spasm of the little laryngeal muscles, and so produce death by apnoea. The remedy for such an emergency, as Dr. Marshall Hall truly observes, must be *immediate*: and this is what he tells us should be done.

"Pressure being made on the abdomen, to prevent the descent of the diaphragm, a forcible blow should be made by the flat hand on the thorax. The effect of this is to induce an effort similar to that of expiration; the larynx being closed, œsophageal vomiting takes place, and the morsel is dislodged.

"If this plan fail, not an instant being lost, the pressure should be kept up on the abdomen, the finger should be introduced into the throat, and the same smart and forcible blow made on the thorax as before. By the irritation of the fauces the cardia is opened, and by the blow on the thorax (firm pressure being made on the abdomen) an effort similar to that of expiration, with a closed larynx, is made, and a direct vomiting ensues, and the morsel of food is carried away."

2ndly. The substance, if small, may, after it has passed the chink, remain in the larynx; entangled in its ventricles, or between the chordæ vocales. In that case it usually occasions very severe laryngeal symptoms—spasmodic gasping cough, choking sensations, croupy respiration, and pain in the larynx—symptoms which harass the patients without intermission, until death ensues, or until the substance is driven upwards into the pharynx, or drops downwards into the windpipe. There is, however, one instance on record, in which a piece of gold was lodged for years in the ventricles of the larynx, without these distressing consequences.

3rdly. Having passed the upper part of the larynx, it may stop, and become fixed beneath the cricoid cartilage, or in the trachea. In these situations, unless it quite blocks up the passage, its presence may be productive of but little distress. A wheezing or croupy sound during one or both of the movements of respiration, and some degree of pain and tenderness of the part where the substance was lodged, have constituted all the evidence of its position in the air-passages, in more than one instance. A very singular and whimsical case of this kind, related by Professor Macnamara, is referred to by Dr. Stokes. A boy had made a whistle, by perforating a plumstone, and extracting the kernel. This, during a strong inspiration, passed from between his lips, through the glottis, and became fixed transversely in the larynx. So little inconvenience did it create, that the boy, finding that he still whistled as he breathed, went about for some hours, pleased to display this new accomplishment. For three days he continued to occupy himself in his childish amusements, suffering now and then a seizure of suffocating cough. He was then taken to the Meath hospital. He had no pain in deglutition; but he said that when the cough was severe, it caused pain in his throat. He had also uneasiness in the epigastrium, a bloated countenance, and a frequent pulse. The chest sounded well on percussion,

and the vesicular murmur was natural. The fits of coughing were followed by white frothy expectoration. Laryngotomy was performed; but during the struggle and the convulsive cough which took place when the opening was made, the stone (so the patient declared) was coughed up, and swallowed. The symptoms were relieved; and the whistling ceased. But it was found that, as the wound healed, the distress and the whistling sound returned; which showed that the stone lay above the opening; and that the disappearance of the symptoms had been owing, not to its dislodgment, but to the admission of air below the point where it was fixed. Soon after this, however, it changed its place, passed down into the right bronchus, and then up again towards the larynx. By a second operation it was extracted; and the patient recovered without any bad symptom.

4thly. The substance may get beyond the trachea, into one of the bronchi, and stay there. And it is a very curious fact, and one which has evident importance in respect of diagnosis, that it is almost always the *right* bronchus which the substance enters. Dr. Stokes has explained why it is so. The septum that divides the extremity of the trachea into two branches is not placed in the middle of the channel, but decidedly towards the left; so that any solid body falling down through the windpipe, is naturally directed into the right bronchus. Perhaps this tendency is aided by the more vertical direction, and by the somewhat greater capacity of that tube, compared with its fellow. Now you will readily apprehend what sort of symptoms would be likely to result from the impaction of a solid body in either of the primary bronchi. It would be very apt to excite inflammation of the corresponding lung, which inflammation would reveal itself by its proper signs; but it would produce peculiar auscultatory phenomena, prior to and independent of such inflammation. It would prevent, partially or altogether, the entrance of air into the lung of that side. Hence, when we have other reasons for thinking that a solid body has passed the glottis, if we find the vesicular murmur suspended or enfeebled in one lung, while percussion gives out its usual clear sound, we may conclude that the intruder is lodged in the bronchus belonging to that lung. It seems not improbable that collapse of a portion of the lung may sometimes result from its presence there. This would modify the symptoms; but still the same conclusion would be warrantable from them.

Dr. Stokes believes, and his opinion is fortified by his own experience on the subject, that smooth bodies (beans or shots, for example), are more calculated than such as are rugged and uneven to cause urgent distress when impacted in one of the bronchi; inasmuch as they more completely plug and obstruct the tube, thereby depriving the patient at once of the use of half his lungs. An irregular substance, which can neither seal the passage up, nor be closely grasped by its spasmodic contraction, will probably occasion less dyspnoea, and at the same time will be less likely to be dislodged by the effort of expiration. Under these circumstances we look for more chronic symptoms.

5thly, and lastly. The intruding substance may not be fixed anywhere, but may shift its place from time to time; and this, in fact, is what most frequently happens: and when it does happen, it gives rise to a very striking and distinctive series of symptoms. Paroxysms of suffocating cough and extreme distress, when the substance is driven up into or near the larynx; with intervals of comparative quiet, and sometimes indeed of apparent health, when it subsides into the trachea or bronchi. But during these intervals, the signs that sometimes mark its situation in those tubes may perhaps be discoverable.

There are, then, a set of general symptoms, which lead us to believe, or to suspect, that some solid body has entered the air-passages: and there are other sets of particular symptoms, which inform us, with more or less certainty, whereabouts it is fixed, or that it is not fixed at all. A person, previously in good health, is seized with violent cough and choking dyspnoea, suddenly, during a meal, or while he had in his mouth some loose substance, which he fancies he has swallowed. This is a sufficient clue to the probable nature of the case: and we next inquire for a sense of soreness in the windpipe, and wheezing respiration; for signs of bronchitis or of pneumonia, especially in the right lung; for signs of obstruction of the bronchus on one side, and especially on the right side; or for alternations of suffocating cough, with intervals of

outward calm. In the last case, we may expect to find the bronchus unstoppered during the periods of laryngeal irritation; and *vice versâ*.

When we know that a solid body has been entrapped in the air-tubes, our business is plain; there is no room, in my opinion, for hesitation: we must let the substance out through an artificial wicket. There is danger, so long as it remains in these vital passages, of speedy suffocation; of fatal damage to the larynx, or to the lungs; of cerebral mischief during the violent paroxysms of coughing. Convulsions and apoplexy have, under such circumstances, actually occurred. Against these perils there is no security, except in the early performance of tracheotomy. If the included substance be loose and smooth it will presently be shot forth at the new orifice; if it be fixed, or angular, it may generally be extricated by a skilful and delicate hand.

Even while this sheet has been passing under the press (May, 1843), another instance has occurred of the same accident, and excited a degree of anxious interest in the public mind, scarcely less than is accorded to a royal illness. It befell a gentleman whose name was previously famous. Mr. Brunel, in amusing the children of a friend with some tricks of legerdemain, put a half-sovereign into his mouth; and the coin slipped, as from its size and shape it might easily do, through the chink of the glottis. It seems to have occasioned no very urgent distress. The patient was made aware, by some internal sensation, that it lay towards the right side. After more than three weeks had passed, the trachea was opened: but the piece of money did not come forth. Probably its weight prevented its being driven up and down the windpipe; and when it laid edgeways, its form did not oppose much impediment to the breath. The same weight, however, brought it back to the larynx whenever Mr. Brunel placed himself with his head downwards. In some of these experiments, coming crossways I suppose, it produced most violent cough, and feelings of impending suffocation: but in a final and happier trial, at the end of six weeks, it dropped out again, through the natural passage—just as a coin may sometimes, by good luck, be shaken out of a box through a slit in the lid.

A still more recent example of a similar mixture of bad and good fortune has been recorded by Dr. James Duncan in the *Northern Journal of Medicine*. A man was amusing himself with tossing up a shilling, and catching it in his mouth. Suddenly it fell into his larynx, and produced violent cough and severe dyspnoea, which gradually subsided. The difficulty of breathing returned in paroxysms, upon his making a deep inspiration, or after certain movements of the body. When the larynx was compressed externally, the man felt that the coin was lying opposite to the cricoid cartilage. He was now held with his head downwards by three strong men, was shaken once or twice in that position, and his larynx was moved from side to side, when lo! the shilling re-entered his mouth, and dropped out upon the floor. During this process he suffered neither cough nor dyspnoea.

An almost exact counterpart of this case has since been communicated to me by Dr. G. B. Halford. In the year 1852, when that gentleman was house-surgeon to the Westminster hospital, a man was admitted, about noon, who stated that on the previous evening, while entertaining his children by throwing a shilling into the air and attempting to catch it in his mouth, the shilling slipped into his windpipe. He had been to several medical men, and had taken emetic after emetic in vain. The shilling could be distinctly felt opposite the crico-thyroid membrane. The man's voice was reduced to a whisper. Dr. Halford "directed the porters of the hospital to turn him upside down in the corner of the surgery, when, after several expiratory efforts, the shilling rolled out of his mouth."

Notwithstanding the speedy and easy dislodgement of the coins in these instances, it may be prudent and requisite, in less fortunate cases, when the piece of money does not fall, at first, in the right direction, to lessen the risk of suffocating cough, by making an artificial opening in the trachea. If the substance be within reach, it may be then plucked forth with forceps: if not, the safety valve thus established will render

the inversion of the patient's body less distressing and less hazardous; and allow it to be repeated until it shall prove successful.

For Mr. Brunel's convenience a sort of platform was constructed, moveable upon a central hinge. To this platform he was bound in the prone position, by a broad strap passing across his shoulders; and then his head was lowered until the platform was brought to an angle of about 80 degrees with the horizon. This did not succeed until after the opening was made in his windpipe.

[For a full account of everything connected with the subject of foreign bodies in the air-passages, and the treatment best adapted for the relief of the patient, the reader is referred to the very able treatise of Dr. Samuel D. Gross, *Philada.*, 1855.—C.]

LECTURE LIX.

Diseases of the Heart: usually partial. Changes in its Muscular Texture. Mechanism of those Changes. Natural dimensions of the Heart. Natural Sounds. Modifications of these by Disease. Review of the Physical and General Signs that accompany Cardiac Disease.

You will perhaps accuse me, gentlemen, of a disposition to magnify the importance of every new class of diseases at which we arrive, in our survey of the morbid conditions of the various parts of the body in succession. There are few complaints, in truth, which are not important; either from the discomfort to which they give rise, or from their tendency to abbreviate the span of human existence. Yet of the strictly vital organs the derangements are necessarily the most perilous; and therefore, to us, the most interesting. Two props of the tripod of life we have passed in review, and seen how they may be weakened, and how they may fail altogether. The office of the *heart* is not less essential to life and health, than that of the brain, or of the lungs. The well-being of every portion of the frame depends upon its being duly supplied with healthy arterial blood, and duly relieved of that which has become venous: and this supply and relief require that the central organ of the circulation should be sound in its structure, and perfect in its working. But it is frequently otherwise. I can remember, indeed, the time when disease of the heart was thought to be a very rare thing; but it is now well-known to be one of the commonest of disorders, and it connects itself with a variety of other affections, with which it was formerly supposed to have no relation.

Like other organs that are complex of structure and formed of different tissues, the heart is subject to *partial* disease. Its lining membrane alone may, in the first instance, become the seat of inflammation, with its various effects; or its investing membrane only may undergo morbid alterations; or the muscular substance that constitutes the organ itself may be gradually changed in its qualities, in its bulk, or in its proportions.

But the morbid conditions of the investing and lining membranes do not always, or immediately, compromise the life of the patient. They are fatal at length, in ninety-nine cases out of a hundred, through the alterations to which they lead in the muscle wherewith they are connected. It may be practically useful therefore to consider, first, these ultimate morbid states which are incompatible with the continuance of life; and then to trace them back to the next link in the chain of their causes, which will be found, in a very great number of instances, to consist in some antecedent morbid state of the exterior or of the interior membrane.

The heart, you know, is a living forcing-pump; a hollow muscular engine, with its chambers and their valved outlets, its contractile walls and their strength and thickness, so admirably adjusted, that the healthy balance of the circulation is continually

maintained, under many varying outward influences and inward emotions which tend to disturb it. In treating of diseases of the heart we have to consider, therefore, the modes in which its mechanism may be spoiled or deranged; and the effects of such derangements.

Not only the component tissues, but different portions also of the organ may be separately diseased. It seldom happens, indeed, that the whole heart is affected; although that is probably the vulgar belief. The left side is much more obnoxious to morbid changes than the right: and when both sides are implicated, the alteration is almost always more decided and conspicuous in the left than in the right chambers.

In the rapid sketch which I attempted of general pathology, in the outset of the course, I pointed out the various kinds of alterations to which the tissues and organs of the body, and therefore the heart among the rest, are subject. One or more of the chambers of the heart, you will remember may become larger or smaller than is natural; or have their walls increased or diminished in thickness, and consequently in power; or one or more of its outlets and orifices of communication may be widened or contracted: and the purposes and function of the organ will be more or less impaired by such changes.

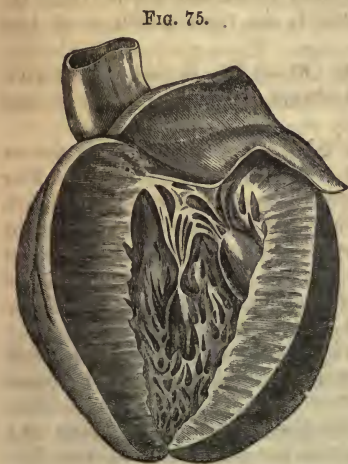
In order, then, to have a clear conception of cardiac disease, it is necessary to analyse it, and to investigate the derangements of the several parts of the heart. And I begin with *hypertrophy*; augmentation of bulk in its muscular substance. And I must first of all define one or two phrases which are current among pathologists in respect to this condition.

The muscular tissue of one, or more, of the chambers of the heart may become thicker and stronger than natural, while the capacity of that chamber, or of those chambers, remains unaltered. The hypertrophy in that case is said to be *simple*.

But, while the muscular parietes are thickened, the corresponding chamber may become unnaturally large. This constitutes the *active aneurism of the heart* of Corvisart, the *eccentric hypertrophy* of more modern writers.

On the other hand, it has been supposed that the capacity of a cavity of the heart may diminish in size as its walls increase in thickness: that the hypertrophy may take place *at the expense* (as it were) of the chamber. This has been called *concentric hypertrophy*.

Now, of these three reputed forms of hypertrophy, considered in their relation to *disease*, two only, the simple and the eccentric, have any real existence. The third, or concentric form, never occurs, I believe, except as a congenital malformation. And of the two genuine species of hypertrophy, the eccentric, which is plainly a compound affection, consisting of *hypertrophy with dilatation*, is much the most common. The reason of this is to be found in the physical cause of the morbid condition, in most instances. The physical cause, in nineteen cases out of twenty, is some obstacle, mechanical or virtual, to the



Hypertrophy of the left ventricle. From a specimen in Dr. Gross' collection.

perfect accomplishment of the function of the chamber; some obstruction opposed to the free and thorough exit of the blood from it; or something which hinders the easy play of the organ. Hence, in the first place, a gradual yielding, or tendency to yield, in the sides of the affected chamber, from the continual and unwonted pressure of the accumulated blood against them: and in the second place, a *striving* action of the muscle to overcome the hindrance, or to counterbalance the obstacle; and consequently, according to the law formerly announced, an augmentation in the bulk of the muscle whereof the function is thus increased. If the hypertrophy, which is the result of a truly conservative process, keep pace exactly with the amount of the obstacle and *exactly balance* it, no dilatation happens, or next to none. But this is comparatively seldom the case. According to the principles of mechanics, a little distension of the spheroidal cavity must require an increase of force to propel from

it a given quantity of blood, in the same time, through a given discharging orifice. So that incipient dilatation becomes (in addition to the supposed obstacle) an efficient cause of hypertrophy: and the two, the dilatation and the hypertrophy, commonly make progress together.

Even when there is no mechanical impediment, incipient dilatation (and consequent hypertrophy) may spring from morbid conditions of the nutrient blood. In this way, as we shall see hereafter, eccentric hypertrophy does often arise, without any valvular fault or physical obstacle to account for it, from renal disease, whereby the due purification of the blood is prevented.

Cruveilhier appears to have been the first to reject *concentric hypertrophy* from the catalogue of cardiac diseases. The smallness of its cavity, with a proportional increased thickness of its walls, was regarded by him as a transient condition of the ventricle, depending upon the mode of death. He found these phenomena very strongly marked in the hearts of all those whose bodies he had examined after decapitation by the guillotine; "Les parois ventriculaires se touchaient dans tous leurs points." It is therefore his opinion that the hearts which had been thought, by others, to present examples of concentric hypertrophy, were in reality "hearts more or less hypertrophied, which death had surprised in all their energy of contractility."

This question has since been considered by Dr. Budd, in a communication to the Medical and Chirurgical Society, which you may read in the twenty-first volume of its *Transactions*. He has lately favoured me with a statement of his matured views upon the subject.

The semblance of concentric hypertrophy is most common in the left ventricle; and depends upon the ventricle being nearly empty at the time of death, and upon the corpse being examined while the heart is contracted by the *rigor mortis*. The fallacious appearance is accordingly noticed in cases where, from the manner of dying, the left ventricle, or the entire heart, contains but little blood, and where, from the muscular power not having been previously exhausted, the rigor mortis is of long duration.

"In all these concentrically hypertrophied hearts (writes Dr. Budd) the ventricle may be readily dilated by means of the fingers, and always dilates of itself when the rigor mortis goes off.

"In the published cases of concentric hypertrophy, in which there was no disease of the valves (I have given eight such cases in my paper, and could now add a long list to them), there were no signs, or only very slight signs, of disease of the heart, during the life-time of the patient. This circumstance is sufficient proof that the cavities of the heart in these cases could not have been during life permanently in the contracted state in which they were found after death. A left ventricle that could scarcely contain an almond (a common form of expression in the description of these cases) would surely have caused a great impediment to the circulation."

Moreover, concentric hypertrophy could answer no mechanical purpose; nor could its formation be accounted for on mechanical principles. But "concentric hypertrophy of a ventricle, in a high degree, with obstruction at its discharging orifice, and an extraordinary channel for the passage of the blood, occasionally exists as a congenital malformation; and in most cases, the right is the ventricle so affected."

To resume. Recollect that there may be two distinct kinds of physical cause of excessive action of the heart, and therefore of hypertrophy. In the one kind, there is some mechanical obstruction to the exit of the blood from one or more of the cavities; a constricted state of the *orifices*, is the most common condition. In the other kind, without any such mechanical bar or dam to the fluid, there is something to hinder the free and sufficient play of the organ; unhealthy blood perhaps it may be, or malposition of the heart. If the heart be pushed, for instance, out of its proper place and posture by effusion into the pleura, or by distortion of the chest, it will not work with the same ease as when all is perfect and symmetrical; and the unusual labour imposed upon it fully to execute its office, will lead to hypertrophy. The causes of hypertrophy may therefore be situated *within* the heart itself, or *without* and beyond it: but in all those cases in which the effect of the hindrance or obstacle is to *detain* the blood in one or more chambers, the hypertrophy will be likely to be accompanied by *dilatation*: and, generally speaking, the hypertrophy and dilatation result from disease in some part which lies *beyond* the affected chamber, in the order

of the circulation. Thus either a narrowing, or a dilatation of the aorta at its commencement, will tend to cause hypertrophy and dilatation of the left ventricle.

That *contraction* of the aorta, or of the aortic orifice, may have this consequence, you will have no difficulty in perceiving. The blood cannot so readily pass through the narrowed channel; hence it will tend to accumulate in undue quantity in the ventricle, and therefore to stretch and dilate it; and the increased muscular efforts necessary to drive the delayed blood onwards, tend also to thicken the muscle itself. But it may not be so obvious that *dilatation* of the mouth of the aorta—a wider channel of egress—would also virtually prove an obstacle to the emptying of the ventricle. Yet it certainly would, in two ways. In the first place, dilatation of the entrance of the aorta implies a diminution in the elasticity of that vessel; and the blood after it has left the heart is urged onwards by the healthy elasticity. But again, dilatation of the mouth of the aorta commonly implies an imperfect closure of that vessel by the sigmoid valves; so that during the diastole, a part of the blood is apt to regurgitate from the aorta, and to keep the ventricle morbidly full. You see, therefore, that a deviation from the healthy state of the aorta and of the valvular apparatus which lies at its mouth, may obstruct the course of the blood, and lead to hypertrophy and dilatation, whether the deviation be in the one way or in the other; whether, I mean, the natural size of the vessel be increased or diminished. Again, disease of the mitral valve, obstructing the flow of the blood at *that* point, will lead to an accumulation in the left auricle, in the pulmonary veins, and in the lungs themselves. The auricular action is always less regular and energetic than the ventricular, so that we less frequently meet with *hypertrophy* of the auricles, but very often with dilatation. And if we go to the other side of the heart, we find hypertrophy with dilatation, and more especially dilatation of the right ventricle, when, from some reason or other, the blood passes with difficulty towards or through the *lungs*: either from disease of the pulmonary artery, or from disease in the substance of the lungs—emphysema, for instance; and if the difficulty be great, the accumulation and distension will affect successively the right auricle, and the *venæ cavæ*; and then we have, in most cases, general dropsy. So that, I repeat, disease in the heart tends to propagate itself in a direction contrary to that of the circulation. Furthermore, if the muscular tissue of the heart be pale, flabby, soft and weak, as it frequently is in feeble, ill-nourished, cachectic persons, or if it have undergone that kind of fatty degeneration which I formerly described, it will the more readily yield to the centrifugal pressure of the blood it embraces. In this way we may have dilatation without any hypertrophy. I am anxious that you should in the outset comprehend the mechanism by which the natural dimensions, and relative proportions of different parts of the heart may be altered in disease.

One reason why disease of the heart used formerly to be overlooked, was that these natural dimensions and relative proportions were not ascertained or much attended to. It is not easy to form any very precise estimate of the size of a healthy heart. It is commonly held that if the heart be about the same size with the closed fist of the subject, its general dimensions may be considered to be natural. Bouillaud, who has taken much pains with this matter, weighing and measuring a great number of different hearts, states that the *mean* weight of that organ, with the origin of its large vessels, and empty of blood, in adults from twenty-five to sixty years old, is from eight to nine ounces; that in subjects from sixteen to twenty-five years old it may be one or two ounces less; and that, in very large and robust persons, it may rise to ten or eleven ounces. Also, what we should expect, that the weight is less in women than in men.

So much for the general bulk of the heart. And we must have some standard whereby to estimate its relative proportions. Every one knows that the walls of the left ventricle are thicker than those of the right. Bouillaud found that the mean thickness of the walls of the left ventricle *at its base* was seven lines, while that of the right ventricle was two-and-a-half lines. And taking the thickness, generally, he says, that the thickness of the parietes of the right ventricle has not a greater ratio to that of the parietes of the left, than two to five, or even than one to three.

So again of the auricles: he lays it down that the mean thickness of the walls of the left auricle, is to the mean thickness of those of the right, as three to two.

He holds also that the mean capacity of the right ventricle exceeds, by a little, that

of the left: and that the right auricle is larger than the left. Dr. Kirkes, however, thinks that probably "the capacity of the two ventricles is exactly the same. It is difficult to determine with certainty how much this may be; but taking the mean of various estimates, it may be inferred that each ventricle is able to contain, on the average, about three ounces of blood, the whole of which is impelled into their respective arteries, at each contraction." You must always make allowance in actual cases, for the possible distension of these cavities with blood, beyond the size to which they would have contracted if they had contained no blood.

I may add, that Bouillaud declares the rule I just now mentioned, which had been proposed before his researches were instituted—the rule, viz., which makes the bulk of the healthy heart equal to the fist of the subject—to be tolerably correct. By keeping in mind these general facts, you will be better able to appreciate the appearances presented by the heart when it is taken from the body to be examined: but you will recollect that they relate to *averages* only.

Now having pointed out the modes in which the natural proportions of the heart, and of its several parts, may be morbidly altered; and given you a rough standard which may enable you to estimate these proportions in the state of health, and the deviations from them in the state of disease: I will go on to consider the symptoms, by which the altered conditions are accustomed to declare themselves. And it is with respect to the heart, as with respect to the lungs; there are *general* symptoms or signs, and there are *physical* symptoms or signs: and the information derived from these sources respectively is of variable utility. Neither of them can be safely neglected; and it is often found that the indications derived from one of these sets of symptoms are confirmed or corrected by those collected from the other. I believe it will be best to pursue the same course in both cases, and to speak, in the first place, of the signs that are brought within our notice by the sense of hearing.

But, in order that we may comprehend the *morbid sounds* of the heart, we must first make ourselves acquainted with those that belong to its healthy condition.

The heart may be heard by the ear laid flat against the præcordial region, or through a stethoscope, to beat over a certain space. That space, in ordinary circumstances, corresponds to the inferior half of the sternum, and to the cartilages of the ribs, from the fourth to the seventh on the left side. The apex of the organ may often be *seen* to pulsate between the cartilages of the fifth and sixth left ribs; about two inches below the nipple, and one inch from it towards the sternum.

This is the space over which, in the sound state of the heart and lungs, the pulsations of the former are plainly audible. But there are several diseased conditions both of the heart itself, and of the parts around it, which interfere with this rule.

In the first place, if the heart be larger than natural, it will be heard to beat over a proportionally large space. In this way it may come to be heard all over the chest in front; and behind on the left side of the spine; and even, in extreme cases, on the right side of the spine.

Again, the extent of space over which the heart may be heard to beat will be increased in proportion to the thinness of its walls; and diminished, *cæteris paribus*, according to the thickness of its walls. So that when the heart has nearly its proper size, if its walls be thin, it will be heard beyond its natural limits; and if its walls be morbidly thick, *i.e.*, if it be affected with considerable hypertrophy, it will not be heard beyond, nor even to the extent of its natural limits. I will endeavour, presently, to explain the reason of these differences.

Again, and this it is of great importance to remember, the heart may be heard far beyond its natural limits, even when it is perfectly healthy, in consequence of the lung between the ear and the heart having become solid, and therefore a better conductor of sound: and the solidification may have resulted from hepatization, or from the presence of a number of crude tubercles, or from cancerous deposits. The sound of the heart's action will also be conveyed to a distance by the liquid effusion in pleurisy, and by aneurismal tumours. If we are not aware of these circumstances, we are continually liable to fall into mistakes.

The heart is likewise heard more distinctly, and over a space which is comparatively larger, in children than in adult persons; in those who are lean and spare, than in those who are fleshy and fat; and I need scarcely say that it may be heard over a

wider extent of the chest whenever its action is augmented by exercise, by emotion of mind, or by febrile excitement.

The *impulse* of the heart is another point which you must attend to. In healthy persons who are thin, you may generally feel the stroke which the heart gives to the ribs, by placing your hand on the præcordial region. In persons who are fat, you often cannot feel the heart at all in this manner. For obvious reasons, it is felt more distinctly, over a larger space, and higher up, while the person is stooping forwards, or makes a forced expiration; less distinctly, over a smaller space, and lower down, when he makes a deep inspiration, or is lying on his back. In proportion as the heart is enlarged by disease, it can be felt more extensively: and when there is hypertrophy, the force with which it strikes the parietes of the chest is sometimes extraordinary, and very instructive. You will see the ear and head of the listener distinctly lifted at every pulsation. Sometimes the whole of the patient's body, nay his very bed, is shaken by the strong shock of the heart during its systole. There is no sign of hypertrophy so sure as that afforded by the heart's impulse. You feel, not a smart, quick, and sudden knock, but a steady, heaving, irrepressible swell, which is perfectly characteristic. You may always infer increased thickness of the walls of the organ, when you meet with this regular heaving motion; and the extent to which the whole heart is enlarged in such cases may be conjectured by the extent of space over which the heaving impulse is perceptible.

The *sounds* which we hear are two. One of them coincides, in point of time, with the impulse: and barely precedes the beat of the radial artery. It happens, therefore, when the ventricles contract; during the systole. It is called, accordingly, the *systolic* sound, or the *first* sound of the heart. The other of the two sounds coincides with the diastole, and is spoken of as the *second* or the *diastolic* sound. It takes place at the instant when the heart reverts to that place and condition in which it had been prior to the systolic movement. These two sounds occur in quick and regular succession, and then follows an interval of silence, after which the two sounds are repeated; and so on.

The two sounds are not, however, exactly alike. They differ somewhat, both in quality and in duration. The first is a dull, prolonged noise; the second a shorter and smarter sound, having more of a clacking or flapping character. Attempts have been made to assign the respective duration of each sound, and of the period of repose. I confess that I have never succeeded in measuring them satisfactorily in my mind. Probably Dr. C. J. B. Williams' estimate is as near the mark as any. He divides the whole period, from the beginning of one pulsation to the beginning of the next, into five equal parts: and allots two of these to the first sound, one to the second, and the remaining two to the interval of silence. This order of succession is called the *rhythm* of the heart, and it may be perverted.

Respecting the physical *causes* of these natural sounds there have been much recent discussion and research. Our time, however, will permit me to do little more than tell you what I believe to be the facts of the matter. And I take, first, the diastolic sound, as being the simpler of the two. It used to be ascribed to the contraction of the auricles: but that was quite a mistake. The contraction of the auricles, such as it is, happens *immediately before* each systole of the ventricles: whereas the sound in question occurs *immediately after* it, and is succeeded by the period of silence. This we know from the visible movements of the organ when exposed in a living animal. In truth, the auricular contractions are very feeble, and are not attended with any appreciable noise. I have no doubt that the second sound is produced mainly, if not altogether, by the sudden shutting of the floodgates placed at the mouths of the two great outlets of the heart. The recoiling blood forces back the semilunar valves of the aorta and of the pulmonary artery, as one unfurls an umbrella; and with an audible check as they tighten. There is no other tenable mode of accounting for the sound. Experimenters have contrived, by hooks and wires, to prevent these valves from unfolding; and then the flapping sound has been converted into a hiss. Disease of the same valves demonstrates the same things; as we shall presently see. Nevertheless, it is both possible and probable that the relapse of the whole organ to its former place may contribute an ingredient towards this second sound.

The first, or systolic sound, is more complex. Physiologists are not yet agreed as

to its cause. Upon this disputed question I cannot pretend to speak authoritatively. In all probability it is a compound sound: but it must be *chiefly* produced by the collision of the blood with the inner surfaces of the ventricles, and with the surfaces of the tricuspid and mitral valves. In part it has been attributed to the impulse of the heart against the ribs. It has been thought to consist also, in part, of the sound that results from the muscular contraction of the ventricles: that the systolic sound commences with the tightening of the walls of the ventricles, including the valves; and is prolonged by the muscular noise. You are aware, I dare say, that the vigorous contraction of a large muscle is accompanied by audible sound. If, during the stillness of night, when lying in bed, with your cheek and ear upon the pillow, you set your teeth firmly, you will hear a continuous dull rumbling, like the noise of carriage wheels in the street, and evidently caused by the action of the masseter and the temporal muscles. Dr. Williams states that, with the help of a flexible stethoscope, one may hear the voluntary jerking contraction of his own abdominal muscles: the sound being as loud as that of the heart's systole, and very like it in character. But Dr. Halford has satisfied himself, and many others who have witnessed his repeated experiments, that when the entrance of blood into the heart is prevented by forcibly compressing the venæ cavæ and the pulmonary veins, the movements of the heart go on, but the characteristic sound ceases, to recommence at once upon the readmission of the blood. The presence of the blood in the heart during the systole seems therefore essential to the production of the sound. My own opinion is that the impulse against the ribs, and the muscular rumbling, have some share (a small share probably) in causing the sound, but that it is mainly due to the collision between the blood and the walls of the cavity which contains and moves it. Dr. Halford however declares that *all* sound ceases upon the exclusion of the blood; and he contends that both the sounds "depend upon the same cause, which is simply the backward current of the blood producing forcible closure and *tension*, first of the auriculo-ventricular (first sound), and secondly of the ventriculo-arterial valves (second sound)."

The natural sounds which I have been describing are liable to be changed, or modified, by disease. I just now told you that, *cæteris paribus*, the heart is heard more clearly and extensively when its walls are thin, less widely and loudly when they are thick. Of this Dr. Williams offers the following explanation:—"The transition of a thick muscle from slack to tight can never be so complete and sudden as that of a thin one; where there are many fibres they choke and muffle each other's vibrations; hence the sound is dull and prolonged, rather than loud and clear. If we observe the different sounds produced on tightening thin silk, and thick baize or cloth, we find that the thinness of the silk gives a unity and briefness to the impulse which it receives, and the sound is short and loud; whilst in the baize the impulse is divided and prolonged in the complexity of the fibres, and the sound is dull and less brief: so, under similar circumstances, a thin ventricle will give a louder, sharper sound than a thick one."

But other modifications of the natural sounds, of a more striking and extraordinary character, are yet to be explained. Either sound, or both, may be accompanied by a noise, which, in its commonest type, very closely resembles that produced by the blowing of a pair of bellows. Four persons out of five, I should think, if they were asked what this sound resembled, when they heard it accompanying each systolic movement of the heart, would say that it was exactly like the repeated blowing of bellows in an adjoining room. It is called, accordingly, by the French, the "*bruit de soufflet*;" and in homely English, a *bellows sound*. This is the generic sound. It may be divided into species; but it is scarcely worth while so to divide it. We are only likely to confuse our notions by over-refinement. So I will only add, that, when this bellows sound is very harsh or rough, persons will tell you that it is more like the noise of a rasp, or a file, or a saw; but all the while it is some kind of bellows sound. These sounds are often denominated *murmurs* also.

Now what is the cause of this singular deviation from the natural noises made by the successive contractions and relaxations of this hollow muscle, the heart? The whole matter may, I believe, be briefly thus expressed. The blowing sound may be occasioned by any change which alters the due proportion between the chambers of the heart, and their orifices of communication with each other, and with the blood-

vessels that respectively enter or leave them; it may also be occasioned by a preternatural velocity in the passage of the blood through a healthy and well-adjusted heart. Dr. Elliotson, I think it is, who has offered this apposite illustration of the phenomenon. If the arches of a bridge have a certain relation to the quantity of water in the river, and to the force of the current, the water passes through them quietly, and without any noise. Diminish the size of the arches, and the water begins to go through them with an audible rushing or roaring sound. The very same thing will happen if the arches remain unchanged in size, but the quantity of water in the river, and therefore its velocity and force, be augmented by heavy rains. So it is in the heart. If one of its orifices—say the aortic orifice—be narrowed, by disease of the valves, or in any other way, the blood will not, as before, glide through it smoothly and without noise, but will yield that sound which we call a bellows sound. So also, if the orifice retain its natural dimensions, but the capacity of the cavity from which the blood is driven be augmented. Nay, the same blowing sound may be produced though the cavities and orifices are all healthy, and duly proportioned to each other, if the velocity of the circulating blood be increased beyond a certain limit. If you bear this explanation in mind, it will be found applicable, I think, to almost every case in which there is a blowing sound accompanying the *systole* of the organ. If, at the same time, the valves over which the blood must pass be rigid, or rough, or even loose and vibrating, those circumstances may modify the blowing sound, and render it louder, or hoarser, than it would otherwise be, and justify the appellations of *bruit de scie*, and *bruit de râpe*, with which you will find the French books full, and many of our English books also.

But this explanation applies to a *systolic* blowing sound only. What are we to say when there is a similar sound attending the diastolic movement of the heart? Why a diastolic bellows sound will mostly, if not always, be found to result from and to denote some organic disease affecting the valves of the heart. Thus, if the mitral valve be converted, as it often is, from a pliable folding valve into a bony and rigid unvarying chink, the blood which passes through it from the auricle to the ventricle, during the diastole, may (though it seldom does) cause a rushing or blowing sound. On the other hand, the reflux of blood through the unshut mitral orifice, during the ventricular contraction, may also be attended with an audible noise; and thus we have another and not unfrequent source of a *systolic* murmur. Again, if the aortic valves are imperfect, as they often are, and do not effectually close that vessel, blood will regurgitate through them during the diastole, and produce a bellows sound. That this is the true explanation of the diastolic murmurs, I am convinced, both by the observation of disease, and by the results of experiments upon living animals. In some which were made by Dr. Hope, and which he was good enough to allow me to witness, the short clack of the diastole was at first distinctly audible; then hooks were introduced, so as to prevent the perfect closure of the sigmoid valves during the diastole, and then the short smart clack was converted into a prolonged bellows murmur; and upon letting them go again, the short smart clack recurred. The presence of a diastolic bellows sound has repeatedly enabled me to foretell some disease of the sigmoid valves, interfering with their proper function—that of forbidding the re-entry of the blood into the ventricle from the aorta; and what I have thus predicted during life, has been verified by observation after death.

Bellows sounds are occasionally caused by accidental circumstances, extraneous to the heart: by mere posture sometimes, or by distension of the abdomen, either of which may *tilt* the heart, and alter its position with respect to its great vessels. Instances are recorded of bellows sounds removed by tapping the belly in dropsy, and reproduced by the reaccumulation of the dropsical fluid. You may even *make* a temporary bellows sound by forcibly pressing your stethoscope upon the præcordia, especially in children, in whom the ribs are feeble and yielding. And you may fall into errors of diagnosis if you are not aware of this.

Such, then, are the principal sounds, natural and morbid, which are audible by the naked ear applied to the region of the heart, or which may be heard through the stethoscope. But we derive assistance, in respect to cardiac disease, from percussion also. It enables us to measure, in some cases, the bulk of the heart; in others, to ascertain that the pericardium is distended by fluid. In the perfectly healthy state of the viscera of the thorax, the heart is somewhat overlapped by the thin edge of the

lungs; and the sound elicited by percussion over a part of the præcordial region is intermediate between the hollow sound rendered by lung, and the flat sound yielded by the solid heart. In the centre of the præcordial region, where the heart is not covered by lung, the sound is decidedly dull. When, however, the heart is enlarged by disease, a larger part of its surface is exposed, and a larger portion of the præcordial region gives a dull sound on percussion. And when the pericardium is full of liquid, which distends and expands it, you will sometimes find that not less than a third part of the anterior and lateral portion of the left side is quite dull: and it is interesting often to measure, by percussion, the diminution or extension of the limits of the dullness, as the amount of effused fluid decreases or augments.

What I stated before, concerning the effect of different positions of the body upon the space over which the healthy beating of the heart may be heard, felt, and sometimes seen, applies, *mutatis mutandis*, to the natural dullness which the heart causes when the præcordial region is percussed. The space comprehended by this dullness is thus defined by Dr. Latham. "Take the fifth costal cartilage on the left side, and let a point, midway between its junction with the sternum, and its junction with the rib, be the centre of a circle two inches in diameter. This circle will as nearly as possible define the space of the præcordial region, which is naturally less resonant to percussion than the rest." The dullness should diminish or disappear in the supine position, and when a full breath is drawn; and increase in degree and extent upon a forced expiration, and when the posture is prone.

There is another physical sign which is much dwelt upon by Laennec, and which is sometimes very striking. In certain conditions of disease, the hand placed over the situation of the heart perceives a peculiar thrill or vibration accompanying its movements. The sensation conveyed to the hand is really very much like what Laennec compares it to, viz., that tremor which you feel, when coaxing the back of a cat while it is purring with pleasure. Accordingly he calls this sensation "*fremissement cataire*," the purring thrill. You feel this vibration often when there is present also a loud and strong bellows sound; and Dr. Thomas Davis was of opinion, that the bruit de soufflet, and the fremissement cataire, constitute, in fact, but one phenomenon, which is rendered evident to the touch by the vibrations communicated to the hand, and to the hearing by the vibrations communicated to the ear, through the solid walls of the chest. I know, however, that the fremissement cataire does accompany other sounds, as well as the bellows sound: sounds of which I have not yet had any occasion to speak, but which I shall make you acquainted with when we come to the subject of pericarditis. And I pass from this general account of the sounds belonging to the action of the heart, in health and in disease, to consider the other symptoms by which we judge that such disease is present.

Among the general symptoms, then, of cardiac disease, some are direct—as pain; palpitation or excessive action of the heart perceptible by the patient; irregular or intermittent action, which the patient may or may not be conscious of: and some are indirect, declaring themselves through the medium of other parts and organs—such are dyspnoea; cough; dropsical accumulations; hæmorrhages; various affections of the nervous system, especially an increased and morbid sensibility, what is usually called *nervousness*: and some others, which I will cursorily notice as we proceed.

I shall take this opportunity of considering, once for all, some of these symptoms; whether they really proceed from organic disease of the heart or not: for the determination of the question, whether they do or do not indicate such disease, is often of great moment, and is not always easy.

We are not, in general, sensible of the beating of our hearts; but when the pulsations become inordinately forcible, they make themselves felt, and the sensation is, in many cases, a most troublesome and distressing one. Palpitation implies increased force, or increased frequency—or an increase both in force and in frequency—of the contractions of the heart. Every one has experienced palpitation in his own person who has run himself out of breath. The pulsations are sometimes tumultuous also, and irregular, as well as unduly frequent and forcible; but this is by no means always or necessarily the case. There may be great palpitation with perfect regularity of the heart's action. The increased beating not only can be felt internally by the patient, but it may often be heard both by himself and by others. However, we do meet with persons whose hearts throb with excessive violence, without their being at

all aware of it. Such cases, are always, I believe, cases of disease; whereas the palpitations that annoy and harass the patient are very often connected with functional disorder only.

Irregular action of the heart consists in some derangement or discord of its rhythmical movements, and is discovered by the condition of the arterial pulse — by unnatural fluctuations in the strength, or in the number, of its beatings, or in both. Sometimes a few rapid and feeble pulsations occur at uncertain intervals, and are followed by others that are fuller and slower. Sometimes one or more beats are left out, and the next beat, as if to make up for this pause, is unusually strong. The pulse is then said to *intermit*. The intermissions may be unperceived by the patient himself; but in general they are attended with a singularly disagreeable fluttering, or trembling sensation in the breast. The pulse may intermit though the heart does not: the ventricle may now and then contract so faintly as not to propel a wave of blood so far along the artery. Intermission implies irregularity; but the action may be irregular and disorderly without intermitting.

Now, any of these deviations from the natural rhythm and action of the heart alarm people very much, and impress them with a belief that they have some fixed disease of that organ; and you will continually be appealed to for your opinion on this point. I suppose there are few medical students who have not, at some time or another, admitted into their minds the apprehension that they had disease of the heart; an apprehension engendered by its occasional palpitation or irregularity. For though there may be palpitation without irregularity, yet it is practically convenient to consider the two together.

These deviations certainly belong both to organic disease and to mere functional disorder of the heart; but I repeat, that in a great number, nay, in a great majority, of the cases in which they so distress and alarm the patient as to lead him or her to complain of them, they are unconnected with any change of structure; and this it is of much importance that you should be aware of.

Palpitation of the heart, and intermission or irregularity of the pulse, are often dependent upon some disordered condition of the stomach, and will cease at once when that disorder is rectified. It is curious that this may happen although the gastric affection does not manifest itself by any other symptom: and it is curious, too, how slight a cause may suffice to produce the irregular action. A friend of mine, a barrister, used to be very anxious about himself, because a fluttering sensation frequently occurred at his heart; an intermission of one or two beats, and then a violent throb when the organ again resumed its play. This is a sensation very familiar to my own consciousness, and probably most persons have occasionally experienced it. However, it happened so often to the gentleman I speak of, that it made him very unhappy. He persuaded himself that he had disease of the heart, and that he should some day suddenly drop down dead. But there was no other symptom of cardiac disease, direct or indirect, general or physical. He was accordingly told that the intermission depended upon some fault in his digestive organs; and he was advised to leave off different articles of food and drink in succession, in order to discover whether any one particular thing offended the stomach, and gave rise to the symptom. He began by abstaining from tea, which he had been in the habit of drinking in considerable quantity; and thereupon the fluttering of the heart ceased. After a while he took to tea again, and then the fluttering returned. He repeated the experiment many times, and always with the same result, till at length his mind was satisfied; and by renouncing tea altogether he got rid of his palpitation and of his apprehensions. I mention this instance, because it came within my own cognizance; but it is only a sample of many such, and *tea* is frequently found to be the disturbing substance.

I must caution you, however, against the mistake which is often made, of inferring that the heart is free from organic change because its irregular movements are accompanied by dyspeptic symptoms. Structural disease of that organ is very apt to derange the digestive functions. You will commonly find that patients who labour under such disease are exceedingly liable to flatulence of the stomach; and free eructation of the gas which plagued them mitigates wonderfully the cardiac distress. It does so, no doubt, by relieving the diaphragm from that upward pressure which had embarrassed the motions of the heart.

We judge that palpitations and irregularities are merely symptomatic consequences of gastric disorder when they occur occasionally only; when the rhythm of the heart is perfect during the intervals; and when we fail to discover any other physical or general signs that its texture has undergone alteration.

Besides these overstrong or irregular movements, which are symptomatic of disorder of the stomach, and are remedied by correcting that disorder, there are palpitations of a purely nervous kind. I mean that they depend upon a peculiar and highly sensitive condition of the nervous system; which condition is itself dependent, in general, upon a particular state of the vascular system. Persons of a "moveable" constitution, whether male or female, are subject to these palpitations: but especially young women: and, of these, such as are pale, exsanguine, hysterical, in whom the menstrual functions are deficient, or excessive, or somehow unnatural. Anæmia, if not a constant, is certainly a frequent and most remarkable feature of this nervous state. The blood is aqueous; poor in fibrine, and in red particles. The age, and frequently the sex, of the patient form leading points in the diagnosis. Nervous palpitations are apt to come on when the patient is quite at rest: palpitations that result from organic disease are, on the contrary, mitigated, usually, by repose. The occurrence of palpitations *in the night*, however, is but an equivocal circumstance, for nervous persons who dream, awake often with palpitation; and the recumbent posture is apt to excite or to aggravate the palpitations that are organic. Neither, in forming our diagnosis, can we trust implicitly to the presence or absence of physical signs. The heaving impulse of hypertrophy is indeed wanting; but, as I told you formerly, the short abrupt knock of chlorotic palpitation is often attended with a systolic bellows murmur: and this murmur is not confined to the præcordial region, but may be traced distinctly in the subclavian and carotid arteries. These unnatural sounds are common in persons whose blood has been drained of its red particles by frequent hæmorrhages, or by copious or repeated venæsection. We may suppose, in seeking to explain them, that the weak and flabby heart dilates a little, so that the healthy proportion between its ventricles and their outlets is for a time disturbed. But the sounds must in some other way also be dependent upon the thin and impoverished condition of the blood in such patients; and this reminds me of another diagnostic clue with which you should be acquainted. In nervous susceptible persons, especially if they exhibit the pallor of spontaneous anæmia, or are blanched by loss of blood, very curious noises are often audible, by means of the stethoscope, in the *neck*. Continuous rushing or roaring sounds, very like those which are to be heard in shells, and which poets feign, and the vulgar believe, to be the noise of the distant sea.

Shake one, and it awakens; then apply
Its polished lips to your attentive ear,
And it remembers its august abodes,
And murmurs as the ocean murmured there.

Sometimes the sound is more like the hum of a gnat, or the sighing of the wind through a crevice. Dr. Hope very truly states that it may be imitated, by a prolonged whispering pronunciation of the syllable *who*. Bouillaud, from its resemblance to the whizzing of a well-known toy, calls it the "*bruit de diable*." He fancied this singular sound to proceed from the arteries of the neck: but it is quite distinct from the true arterial bellows murmur, and it has been clearly shown (first by Dr. Ogier Ward), that it is produced by the descent of the attenuated blood through the great cervical veins. The sound, though continuous, has often a marked and regular increase, or swell, which keeps time with the heart's systole, and is believed to depend upon the pulsating pressure of the contiguous artery. It is best heard on the right side of the neck, just above the clavicle, and just behind the posterior edge of the sterno-mastoid muscle. You must take care not to *produce* these sounds, as you easily may, by pressure with the stethoscope. So, also, you may suspend them at your pleasure, by pressing, above the stethoscope, upon the track of the veins, so as to stop the current of blood through them, without arresting the pulsation of the arteries. This proves that the murmurs are *venous*. I have no leisure to go more into particulars concerning these sounds; but when you meet with them, concurring with cardiac palpitations, in a young, nervous, anæmic subject, the palpitations, ninety-

nine times in a hundred, will turn out to be simply functional — independent of any organic disease. No doubt there may be co-existing change of structure; but that is a rare exception, and when it does occur other signs proper to structural disease will be present, and will betray it.

Now these palpitations, and these musical or rushing sounds in the jugular veins are to be cured by remedying the state of the blood. And the remedies are preparations of steel, aloetic purgatives, animal food, the cold shower-bath, and exercise, short of producing great fatigue, in pure air.

I have further to remark, with respect to intermissions of the heart's action, and therefore of the pulse at the wrist, that they are frequently connected, both in health and in disease, with feebleness, and also with unusual slowness, of pulsation. So that a slow pulse which is likewise feeble is often converted into an intermitting pulse by depletion; by blood-letting, for example, or by an active purgative; and the intermit-tence may be removed again by a stimulant. I mention this now, because there is another and very different state of disease, in which the pulse is apt to intermit. I mean when there is *plethora capitis*, and cerebral mischief is present or impending. But then the pulse will be full and strong, and labouring. In these cases a stimulant treatment would of course be injurious; while blood-letting, which would *cause* the other form of-intermission, is the *remedy* of this.

Some assistance in determining between organic disease and mere functional disorder of the heart may perhaps be derived from observing the position of the patient. It is stated that when there is mere nervous palpitation, the patient lies as well, and perhaps better, on the left side than otherwise: whereas, when the heart is actually diseased, the decubitus on the right side is more comfortable than that on the left. If there be any tenderness of the heart, or of its enveloping membrane, the posture on the right side is supposed to be the easiest, because the heart is further removed from the ribs, and impinges upon them during the systole with less force. However, no great stress can be laid upon this symptom.

Of the remaining general symptoms of heart disease there is not much to be said. Dyspnœa and cough are indirect symptoms declared through the lungs, between which and the heart there is a close and obvious reciprocal influence. But dyspnœa and cough are direct symptoms of pulmonary disease; and even of pulmonary disease they scarcely help the precise diagnosis. That disease of the heart may materially alter the quantity of blood that is sent to, or transmitted from the lungs, is too plain to require any formal proof; and where the quantity of blood in the lungs is affected, the quantity of air necessary to ventilate that blood must vary: in other words, dyspnœa must ensue. Hæmoptysis is also an equivocal symptom.

One very common effect of cardiac disease is an impeded and sluggish transmission of venous blood from the abdominal viscera. Hence congestions of various parts, and especially of the liver, which enlarges and grows tender; and the biliary secretion and functions are deranged. These symptoms are a fruitful source of mistake, leading the unwary practitioner into the belief that the whole of his patient's malady is hepatic; whom he comforts accordingly with the assurance, that "it is all liver."

The circulation through the *brain* is also apt to be much disturbed in heart diseases; and to this circumstance we must attribute the headaches and giddiness that often accompany them; the dread and causeless apprehension which such patients frequently exhibit; the cowardice and irritability which disease of the heart engenders in men who previously were intrepid, and of strong and firm nerves; also that propensity to dreaming, and especially to distressful and frightening dreams, so commonly observable in them; and the sudden startings from sleep in agitation and alarm. The relations that subsist between apoplexy and organic disease of the heart were fully discussed in a former lecture.

One of the most common indirect symptoms of cardiac disease is dropsy; yet, sometimes the disease of the heart may continue long, and even prove fatal, without giving rise to any dropsy. It will produce that symptom, or not, according as it leads to venous congestion or not. Hence dropsy is more particularly connected with dilatation and attenuation of the right cavities of the heart. But these are points to which I must revert.

Having thus run over, gentlemen, the morbid changes to which the heart, as a muscular organ, is liable; the alterations of thickness in its walls, and of capacity in

its chambers, and the derangements of the natural relations between the several chambers and their orifices; having considered, also, in a brief and cursory manner, the sounds which the heart gives out in its different movements during health, and the modifications to which these sounds are subject in disease; and having, moreover, passed in review the general symptoms which frequently display themselves in connexion with cardiac disorder, we shall be the better prepared, I hope, to investigate, when we next meet, some of the specific diseases of that important organ.

LECTURE LX.

Diseases affecting the muscular texture of the heart; and their treatment. Fatty degeneration. Rupture. Changes to which the valves of the heart are subject. Effects, and diagnosis, of those changes. Angina pectoris.

I KNOW not how I can so well put you in possession of what I know, or think, concerning particular structural diseases of the heart, as by taking them in succession, and offering a sort of running commentary upon them. The *mechanism* of those structural changes, and the altered sounds, and the other physical signs, arising out of them, I endeavoured to explain in the last lecture. Bear in mind that in this place I can do no more than draw broad outlines.

Simple hypertrophy of the left ventricle. This sometimes occurs when we can discover no mechanical obstacle to the passage of the blood out of the ventricle, which might account for it: none, I mean, by the closest scrutiny made even after death. Is it then possible that this change may be brought about by physical causes which are not permanent, and have no place within the body: such as undue action of the organ for a length of time, in consequence of habitual bodily exertion? A runner, for example, we may conceive to keep his heart beating with a degree of force and frequency beyond what is natural, for the greater part of the day; and that for many days, or weeks together. Again, can simple hypertrophy grow out of that excessive action of the heart which may be kept up, day after day, for a long period, by protracted mental emotion? It is difficult to answer these questions. But I presume that causes of this kind—that any cause, in short, which implied long-continued increase in the function of the organ,—*would* suffice to generate hypertrophy. What is certain, however, is that such causes seldom do act with sufficient intensity and constancy to produce these effects: and simple hypertrophy of the left ventricle, with no physical obstruction to the flow of blood through the heart, and no impediment to the free play of the organ, is rare.

We ascertain its existence when it does exist, first, by the account which the patient gives of himself. He has a *sensation* of beating of his heart, which he ought not to have; he feels it and hears it beating as he lies awake in bed; or even at other times when he is at rest. The pulsations are regular. Hypertrophy has no tendency in itself to cause the pulse to intermit or to become irregular. The breath may be short, but there is no marked dyspnoea: the circulation of the blood through the lungs is not much affected by this alteration of the left ventricle; they are in fact protected by the mitral valve: there is seldom any dropsy: but the arterial circulation being forced, there is a tendency to active congestion in the capillary vessels. As there is no mechanical obstacle to bridle the excessive power of the muscle, the pulse is full and strong; the face is florid; the patient is liable to headache, to bleeding from the nose, to active hæmorrhage, and to local inflammation. If you listen to the heart in such a case, you find that the systolic sound is less loud and clear than is natural. It is not heard beyond the præcordial region, nor even perhaps over its whole extent: but there is no bellows sound. And if you place your hand upon the left breast, you feel that steady, swelling, incontrollable impulsion, which I spoke of

in the last lecture, as the surest sign that I am acquainted with, of hypertrophy. Sometimes the præcordial region is manifestly bulging and prominent.

If I were to preach for an hour concerning the treatment of such cases, I could say no more than this: that they require perfect quiet of mind and body; undeviating abstinence; in short, the strict observance of the antiphlogistic regimen as formerly described; and some of the antiphlogistic remedies: particularly moderate topical bleedings, often repeated; with a close attention to the functions of the digestive organs. These are among the cases in which, if in any, we may expect to cure hypertrophy.

Hypertrophy provoked and sustained by inflammation of the membrane which lines the ventricle is not mere hypertrophy. That complex change is a most interesting one, and will demand our attention hereafter.

If simple hypertrophy of the left ventricle be rare, hypertrophy of the same chamber from a mechanical obstacle, or from some fixed hindrance to the easy working of the hydraulic machine, is exceedingly common. What difference, then, let us inquire, is made in the symptoms, in the treatment, and in the prospect of recovery, by the presence of a permanent physical impediment, out of which the hypertrophy has grown?

The mechanical impediment will frequently signify its existence, by causing some unnatural sound: a systolic bellows sound most commonly, which is audible over the sternum, along the course of the aorta. And the mechanical impediment will *tend* to cause faltering of the pulse; but generally the hypertrophy corrects that tendency. So, on the other hand, the mechanical obstacle corrects the tendency of the hypertrophy to cause active capillary congestion: and when the obstacle is considerable, it will prevent the pulse from being so full and strong as in the former case. If to the physical signs of hypertrophy of the left ventricle there be added a systolic bellows sound, and a disproportionate smallness and feebleness of the pulse at the wrist, we may safely conclude that there is some impediment to the escape of the blood from the left ventricle into the aorta; and that this impediment has given occasion to the hypertrophy.

Now, in this case, the hypertrophy is really an endeavour towards health. The increased power of the ventricle compensates for the bar which is opposed to the current of the blood. The blood would not be able to go on without the hypertrophy. There would ensue a tendency to stagnation in the circulation, a faltering pulse, imperfect arterialization of the blood, blue cheeks and lips, dyspnoea, dropsy; but the augmentation of bulk and force in the impelling muscle obviates this: obviates it at least for a while: puts off the evil day to a distance. Since this is the case, and since we have no means of removing the mechanical impediment, we should be mad to desire the cure of the hypertrophy, which is to a certain degree a remedy for the impediment; nor indeed could we cure it if we would. But we have to endeavour to keep it within due bounds. If the beating be troublesome to the patient, we may alleviate that symptom, and check what there may be of superfluous energy in the contractions of the morbid chamber, by abstracting blood from the præcordia by leeches; and by soliciting the action of the kidneys, by means of cooling diuretics, among which small doses of digitalis may find an appropriate place. The labouring action of the heart is sometimes calmed by the application of a belladonna plaster. In this variety, also, of the disease, it is of primary importance that no undue efforts of the body be made, and that the patient be protected, as much as possible, against all causes of mental emotion; that scrupulous temperance be enforced; and that all the functions of the body be carefully watched and regulated.

These are not cases in which we can look for recovery: but they are cases which bad management and imprudent habits may hurry on to a fatal termination: and which judicious treatment and a disciplined course of living may render tolerable, and carry forwards for a considerable period.

Under the same condition of mechanical impediment, we oftener have eccentric hypertrophy of the left ventricle: hypertrophy, *i. e.*, with dilatation. Of course the bulk of the whole heart is augmented by both of those conditions; and sometimes it becomes enormous, as big as that of a bullock. The symptoms will differ somewhat, according as the hypertrophy, or the dilatation preponderates, and therefore it will be as well to state here what are the symptoms of simply dilated ventricles. They are a

diminished *impulse* of the heart's action; and therewith a clearer sound than is natural. The first sound approximates to that of the heart's diastole; to the clacking second sound, and it is heard extensively. There is more or less tendency to fluttering palpitations and irregularities of the pulse, which is usually weak and small; to faintness and debility, and to coldness of the extremities: and when the *right* ventricle is dilated, there are some other symptoms which I shall notice presently.

Now, I say, there will be a mixture or modification of the symptoms, when the left ventricle is both dilated and hypertrophic. The dilatation will aid the mechanical impediment in giving a tendency to irregularity and intermission of the pulse; and the hypertrophy will tend to rectify that disposition. And we must *trim* our management of such cases accordingly. If the pulse flutter, we cautiously administer tonics, or stimulants: if it be steady, and the signs that belong to simple hypertrophy predominate, and are excessive and troublesome, we must starve the patient, take blood from his side, purge him and give him diuretics; but at all times keep him as *tranquil* as we can.

Simple hypertrophy of the right ventricle is not a common disease. When it occurs, it results from some actual or virtual impediment to the passage of the blood from the ventricle into the lungs. The most extreme instance of it that I ever saw, was in the heart of a medical friend's son, who died at the age of seventeen; having been for many years affected with the *morbus cæruleus* as it has been called, *i. e.*, an habitual blue state of the cheeks, lips, and tongue, finger-nails, and the skin generally; attended with shortness of breath, and augmented by every kind of exertion. It is seldom that persons thus affected live so long as this poor boy did. The heart, as is usual under such circumstances, was malformed. The septum between the ventricles was imperfect at its upper part; and the aorta belonged as much to the one ventricle as to the other. The pulmonary artery would not admit a goose-quill; the walls of the right ventricle were as thick as those of the left.

Authors tell us that hypertrophy of the right ventricle of the heart is a cause of pulmonary apoplexy. I explained to you in a former lecture why I cannot believe in this doctrine. In the first place I say that the increased thickness and strength of the walls of that chamber supply a measure of the *difficulty*, and not of the *freedom* and *force*, with which the blood is conveyed to the lungs. In the second place pulmonary apoplexy does not result from rupture of vessels by the *vis à tergo*: and is quite a different lesion from *cerebral* apoplexy. It is simply an *accident* of pulmonary *hæmorrhage*. And lastly, I never met with pulmonary apoplexy coincident with mere hypertrophy of the right ventricle. The right ventricle lies *on this side* the lungs, in the order of the circulation; and accordingly, following the rule I mentioned in the last lecture, its morbid states are for the most part *effects*, and not *causes*, of *pulmonary* disease.

The commonest affection of the right ventricle is dilatation, with or without some increase of thickness, and even sometimes with attenuation, of its muscular parietes. This is in general the consequence of *long standing* pulmonary disease; which has prevented the easy passage of the blood out of the right ventricle. And the passage of the blood may be hindered, as I showed you when we were upon the subject of pulmonary emphysema, partly, and directly, by obliteration of the blood-vessels of the lung, but mainly and indirectly by its contraction. Portions of the lung suffer primary disease, which permanently diminishes their bulk; other portions are then stretched and at length permanently dilated as the thorax expands in inspiration; and so by degrees pulmonary emphysema is produced, as a secondary disease. The same shrunken state of the lungs tends also, as Dr. William Gairdner has well explained, to constantly overload and dilate the heart; and the resistance to this tendency leads frequently to some thickening of its walls. That eminent pathologist has established the great probability "that dilatation and hypertrophy of the heart are never otherwise than secondary affections, and that they are dependent in a very great majority of cases, 1st, on valvular deformity and other obstacles to the circulation in the heart or great vessels (dilatation *from within*), and 2ndly, on the expansion of the thorax under abnormal conditions (dilatation *from without*). The consequence of either of these forms of dilatation, or even of the tendency to either of them, may be hypertrophy of the muscular substance, due to the effort of the organ to act effectively under an increased resistance to its contraction."

This condition then of pulmonary disease is often, or ultimately, attended with dilatation of the right auricle, and of the jugular veins, which stand out in relief from the sides of the neck, and exhibit an undulating sort of pulsation, produced by the regurgitation of a part of the blood, whenever the ventricle contracts. I have taken from the neck of a person dead of such disease, veins into which I could slip my forefinger. There is no surer sign of dilatation of the right cavities of the heart, and of an inadequate tricuspid valve, than abiding regurgitant fulness and pulsation of the jugular veins. With all this there is a fluttering action of the heart, an irregular pulse, great distress and shortness of breathing, a dusky skin, and blueness of the countenance, which is bloated and anxious, and a tendency to delirium and drowsiness; while, sooner or later, the whole areolar tissue of the body becomes charged with accumulated serum. Some degree of this may now and then be noticed towards the fatal close of phthisis. Much oftener it accompanies the later periods of extensive pulmonary emphysema. The same condition of the right chambers, producing the same afflictive consequences, is the very frequent sequel of organic changes which originated in the left side of the heart.

Disease, such as I am now describing, in its advanced stages especially, is difficult to treat. If you stimulate, you run the risk of increasing the patient's distress; if you deplete, you incur the hazard of producing fatal syncope, of bringing the heart to a pause from which it is never able to recover. Here, again, you must try to keep the kidneys active; you must enjoin that, as far as may be possible, all causes of agitation or hurry, everything which has previously been found prejudicial to the patient, may be sedulously warded off. I have found more benefit in these cases from steel, cautiously employed, than from any other drug. Without forcing the heart's action, it appears to have the effect of increasing the tone of its muscle; which it thus enables, *for a time*, to compete more successfully with the load it has to carry, and the impediment which it cannot overcome. We can do no more in such cases than palliate.

Hypertrophy—or dilatation—or dilatation with hypertrophy—may affect, in their various degrees and combinations, one chamber of the heart; or several at the same time: or all of them together. It would be vain to attempt to represent, in verbal description, these complicated changes. Enough, I trust, has been said, to enable you to unravel them when they come before you; and to ascertain with sufficient exactness, the general indications which they severally furnish, and the plan of treatment which they require.

You will often find the muscular substance of the heart pale, soft, and flabby; easily broken down, or penetrated, by pressure. This may occur with general debility and looseness of tissues; it sometimes accompanies a plentiful deposit of fat about the organ; and it is supposed to be sometimes also a consequence of inflammation affecting the muscle. Walls thus soft are likely to yield under pressure; but I know of no particular symptom by which we can detect with certainty the soft condition.

One mode in which such softness may originate deserves your especial attention. In an early lecture of this course when speaking on the subject of general pathology, I described two distinct kinds of *atrophy*; the first consisting in mere diminution of bulk, without change of texture; the second involving alteration of texture, without any necessary reduction of bulk. These distinctions are well exemplified in the heart. There are two forms of cardiac atrophy: one well known, in which the organ simply wastes and dwindles in all its parts and dimensions, during the course of some wasting disorder; the other newer to pathology, as yet but imperfectly understood, in which the texture of the muscle degenerates also, and suffers a sort of conversion into fat. It is not that the heart is encumbered by an excessive accumulation of adipous matter upon its exterior, concealing, dipping down among, displacing and thinning probably its muscular fibres. That morbid state of the organ is common enough. The change to which I now advert, and which has been described by Rokitanski, and illustrated by Mr. Paget, depends not upon any deposit among the fasciuli, but upon some alteration of their proper tissue. There may be no increase in the quantity of fat natural to the furrows and depressions on the outside of the heart; “the whole of the organ” (I adopt Mr. Paget's description) “may preserve its customary size, shape, and general external appearance, but it feels soft, doughy, inelastic, unresisting, and may be moulded and doubled up like a heart beginning to decompose after death. It

seems never to have been in the state of *rigor mortis*." "In colour it has not on its surface, much less on its section, the full ruddy brown of healthy heart, a colour approaching that of the strong voluntary muscle,—but it is, for the most part, of a duller, dirtier, lighter brown, in some parts gradually blending with irregular marks or blotches of a paler fawn colour."

Microscopic observation reveals the nature of these changes, and exhibits a number of minute oil-particles, scattered more or less thickly along the course of the muscular fibre. The muscle thus spoiled retains no longer its proper power, and the functions of the organ are defeated.

Of death occurring, apparently, through this fatty degeneration of the heart—a state which might easily escape the notice of any but a careful and practised examiner—Mr. Paget details three very interesting examples. They have recalled to my mind one or two instances that I have witnessed of sudden and fatal failure of the circulation, under very similar circumstances. They were perplexingly obscure at the time—but Mr. Paget's remarks elucidate what I now believe to have been their real pathology.

This insidious disorder has been studied more recently by Dr. Ormerod, the late Mr. Barlow, and others, and especially by Dr. Richard Quain, who has embodied the fruits of his own researches, and the principal facts collected on the subject by previous observers, in the 33d volume of the *Medico-Chirurgical Transactions*.

In accordance with the views which I formerly set before you, Dr. Quain distinguishes fatty growths upon the heart and between its fibres, from fatty degeneration of those fibres themselves. These unnatural conditions spring from different causes: the one being the result of an accumulation in the blood of the elements of fat; the other the result of decay and disintegration. The fatty growth may occur alone; the fatty degeneration may occur alone; but they often meet in the same heart.

In examining a heart thus diseased, the eye first notices the fainter tracing, or the utter absence, of those transverse marks which cross the fibres of all the voluntary muscles, and less distinctly those of the involuntary muscle, the heart. In an early stage of the disease, these cross lines are dimly seen, and the fibre is studded, here and there, with small dark points. When the disease is more decidedly expressed, the dots are more numerous, and the striæ disappear. These dots are little globules of oil. Lying within the sheath of the fibre, they make it soft and friable.

The parts of the heart which have undergone this change are altered in colour as well as in consistence. They are pale, like a faded leaf, or of a yellowish brown, or a muddy pink colour, and they commonly have a spotty or mottled appearance. The change of texture varies in degree, and in extent. It may render the muscle merely soft and flabby, or it may reduce it to a state in which it feels like a wet kid glove, and can be torn as readily as wet brown paper. Every chamber of the heart is liable to this kind of disease, but most of all the left ventricle, then the right ventricle, then the right auricle, and least of all the left auricle. Generally it is more evident in the columnæ carneæ, and near the endocardium, than elsewhere.

Fatty degeneration of the heart may proceed from a defect of healthy nutrition throughout the body, in consequence of some general disorder, or of natural decay in the decline of life. In such cases

FIG. 76.



Specimens of fatty degeneration of the heart.

A. Heart-fibres taken from the columnæ carneæ of the mitral valves of a young woman, æt. 30; the fatty degeneration was scarcely observable in the ventricle, where the fibres still retained their striæ.

B. An extreme case of fatty degeneration, showing an entire conversion of the muscular fibre into oil-molecules, still retaining a linear arrangement. It is taken from the right ventricle of an old gentleman, who had Bright's disease of the kidney and pulmonary phthisis, and was affected with fits during the last two years of his life.

the same morbid change is commonly manifest in other parts also; in the arteries, in the liver, in the kidneys, in the cornea.

But fatty degeneration may be limited to the heart, and even to a small portion of the heart, and then it is owing to some local failure of nutrition; of which perhaps the most common cause is a diseased condition of the coronary arteries. You are probably aware that these two vessels have no large or free communication with each other: and it is a very instructive fact that when one of them alone is diseased, that part only of the heart frequently is found to be affected which receives its supply of blood from the unsound artery. Fatty degeneration of the heart is also met with after bygone inflammation, whether of the muscular tissue itself, or of its lining or its investing membrane. It is no uncommon accompaniment of hypertrophy. In every instance the change seems ultimately traceable to deficient nutrition. To the same principle may be referred that diminution of its firmness, and deterioration of its texture, which the heart is apt to sustain in the graver cases of typhus fever.

Under this dilapidating process the walls of the heart may become so soft and yielding as to bulge out into a pouch, or even so fragile as to crack; in which latter case the patient almost always dies suddenly, the motion of the organ being stopped and strangled by the effusion of blood into the pericardium. So that to die of a broken heart, is not a mere metaphor. A clergyman from the country, whom I previously knew, called at my house in the autumn, and waited some time in my absence; but went away at last without seeing me: and after consulting Dr. James Johnson, set out for his home, ten miles on the other side of Colchester. He had been unwell for some time; had suffered occasional attacks of dyspnœa; and was unusually nervous and irritable. He must have been conscious of some severe distress, for he was extremely anxious to get home, and bribed the post-boys to drive fast. As soon as he reached his own house, he took some supper, and went to bed, apparently comfortable. Half an hour afterwards one of his servants went to him, and found him asleep. At the expiration of another half-hour, he was again visited, and was then a corpse. Among other changes, the pericardium was full of blood, which had escaped from the heart through a rent in the left ventricle, large enough to admit one's finger. That part of the ventricle which surrounded the laceration, was unnaturally thin, to the extent of a crown-piece. There are several specimens of rupture of the left ventricle in the Museum at St. Bartholomew's Hospital. George II. died of rupture of the heart. It is curious enough that a Duchess of Brunswick, of the same family with George II., died of the same disease. In her case an *ulcer* penetrated the parietes of the right ventricle, which in other respects was healthy. The death of a great and good man of our profession, and of our time, Dr. Aber-

FIG. 77.



FIG. 78.

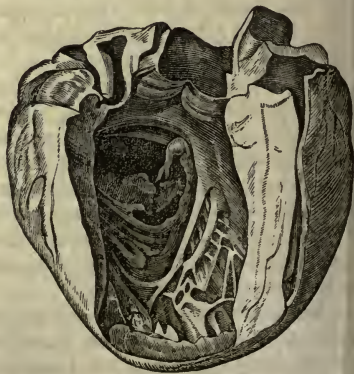


Fig. 77.—Rupture of the heart.

Fig. 78.—Aneurism of the left ventricle, formed by dilatation of a circular portion of the anterior wall, in a girl aged 19. The pouch was filled with a laminated coagulum; its mouth was narrow, round, and smooth, and its parietes consisting apparently of endo- and pericardium, with small deposits of a soft yellowish substance between them. The disease had probably commenced 18 months before death.—St. Bartholomew's Museum. Series xii. No. 53.

crombie, of Edinburgh, was caused by rupture of the heart. In most instances, the rupture has taken place in the left ventricle.

Although this fatal accident may occasionally, as in the example just referred to, be produced by the progress of a perforating ulcer, its commonest cause is that degeneration of structure which we are now considering. There is reason, indeed, to believe that it is very rarely owing to any other cause. Among 83 fatal cases of fatty degeneration of the heart, collected by Dr. Quain, there were 28, or about one in every three, in which laceration of its muscular tissue was discovered after death. In eighteen of these, the outer wall of the left ventricle was ruptured; in three, that of the right ventricle; in one, that of the right auricle; in one, the septum between the ventricles. The rent in a few other of these cases had not gone through the muscle.

The left ventricle is also liable, almost exclusively I believe, to those partial distensions of its walls into lateral cells or pouches, which are spoken of as aneurisms of the heart.

Is there any sign, or any group or succession of symptoms, by the notice of which we can assure ourselves that a living patient's heart is affected with fatty degeneration? I know of no such signs. When that change depends upon a local cause, and is limited to the heart, it is always associated with other changes, of an earlier date; and its proper symptoms, if it have any, are then mixed up and confounded with those of the earlier structural disease. When it proceeds from some general cause, we may sometimes infer its presence with more or less of probability.

There are no auscultatory signs peculiar to the movements of a fatty heart. The pulse has been observed to be weak, sometimes irregular, sometimes remarkably slow; but these qualities are frequent accompaniments of other morbid conditions. The same is to be said of shortness of breath; of attacks of syncope, which are common; of coma, and of præcordial pain, which have been less often noticed in these cases. In seeking to form a diagnosis, the time of life must be taken into account. The fatty degeneration, when it is the result of general decay, is prone to begin about what has been called the climacteric period—the sixty-third year. In more than one half of Dr. Quain's cases, the age of the patients was above sixty. In twenty-one instances out of sixty-eight, death took place in the way of *syncope*. Of the whole number (eighty-three), sixty-eight died *suddenly*. Death is apt to occur upon some shock, or unusual effort:—a hasty ascent, straining at the water-closet, the act of vomiting. “The principal character (writes Mr. Paget) which all these cases seem to present is, that they who labour under this disease are fit enough for all the ordinary events of calm and quiet life, but are wholly unable to resist the storm of a sickness, an accident, or an operation.” I believe this to be generally true. Dr. Begbie has, however, recorded two interesting instances of this malady, by which the lives of two eminent men were cut short (Dr. Chalmers and Dr. Abercrombie), both of whom “were actively engaged up to the hour of death in the labours of two most arduous and onerous professions.”

I say that the evidence of fatty degeneration of the heart can never amount to more than presumptive evidence. But if there be tokens of feebleness of the heart, yet no tokens of any valvular or mechanical flaw—if the patient have attained his “grand climacteric”—if he have shown of late a tendency to grow somewhat fatter, and somewhat paler also, and sicklier in complexion—and if there be withal a marked *arcus senilis*—then you may reasonably conjecture that (to use Dr. Begbie's words) “the great organ of life is yielding, through the progress of time, to those organic changes which mark the decay of its structure, and foretell the not distant cessation of its long-continued functions.”

The last circumstance that I have mentioned, the presence of the *arcus senilis*, furnishes a strong presumption that the fatty change may be in progress elsewhere also in the body. But you must not give this symptom more weight than it deserves. The cornea is sometimes alone in suffering the change. I am acquainted with a gentleman under forty years of age, who, enjoying excellent health, presents a well-pronounced *arcus* in both his eyes, especially at the summit and at the base of the circle, and in whom that appearance has remained unaltered, certainly since he was twenty-four years old, and perhaps from an earlier date.

When rupture ends the scene, usually there is sharp pain at the time of its occur-

rence, and the final struggle is soon over. It would appear, however, from two cases related by Dr. Latham, that laceration of the *septum*, though marked by severe and abiding pain, is not necessarily fatal within so brief a period. One of these patients was kept alive by stimulants for three days after the presumed instant of the rupture. The other endured eighteen hours of mortal agony; and, judging from the symptoms, it seems not improbable that the rupture took place nearly three days before he died.

For that fatty ruin of which I have been speaking there can be no repair. Yet much may be done, even by drugs, for our patient's comfort; and more, by counsel and warning, for his safety. That portion of the muscle which still preserves its primitive structure and qualities may be sustained in its imperfect functions by medicines calculated to renovate the blood, and so to strengthen the muscular tissues. Great improvement does often become manifest under the cautious employment of preparations of iron. When syncope is threatened, diffusible stimuli may be freely used. Above all, you must inculcate temperate habits, and a life of constant quiet. The patient must never be tempted nor surprised into any act which implies unusual effort. He must, for instance, lose a journey rather than hurry on foot to a railway train for which he is late. He must be content to get wet through, rather than run for shelter in a sudden shower. He must never lift a burden, nor climb stairs hastily, nor strain to relieve costive bowels. Neither will it be safe for him, even on what seem legitimate occasions, to yield to feelings of anger, or any kind of excitement. These cautions are indeed more or less applicable to all cardiac disorders; but they are especially requisite whenever there is reason to suspect that the texture of the heart is infirm, and incapable of bearing the stretching pressure of a hurried, or of an impeded, stream of blood.

Many of the morbid conditions of the muscular substance of the heart spring from pre-existing morbid conditions of the membrane which lines, or of the membrane which invests, the heart. It is necessary therefore, in the next place, to inquire into the nature and history of *these* morbid changes: and I will first request your attention to the diseases of the lining membrane. The investing membrane is familiar to you as the *pericardium*. Of late years, since the diseased states of the internal membrane have been more studied and understood than they formerly were, it has been called the *endocardium*: a convenient enough name, which may occasionally spare us circumlocution. Now this endocardium is liable, among other changes, to inflammation, under which it becomes whitish, opaque, and covered sometimes with a thin layer of coagulable lymph. In a well written and well reasoned essay, which you may see in the *Lancet* for 1846, Dr. Munk has shown how hypertrophy of a chamber of the heart may be engendered by chronic or subacute inflammation of its lining membrane; and, what is most interesting, how such hypertrophy may be cured, when the disease is detected and treated in time, by means which arrest and remove the endocarditis. General diffused inflammation, or other change, of the membrane is, however, comparatively rare. Certain parts of it are much more obnoxious to disease than others: those parts which enter into the fabric of the valves and orifices of the organ. The membrane is here in close contact with a dense fibrous tissue; and participates in the changes to which that tissue is subject. And it is an important fact, that the membrane, valves, and orifices of the left side of the heart, as well as its muscular substance, are much more frequently affected with disease than those of the right side. I have adverted to this fact before. What is the prevailing cause of it I cannot tell; but it seems to be a portion of a more general fact; namely, that the arteries are more liable to chronic morbid changes than the veins. Some explain the difference by alleging that the left side of the heart has the heavier task to accomplish. But nature seldom executes her purposes so clumsily, as not to adjust the strength of her machinery to the labour it is destined to perform. Others remark that fibrous tissue is more abundant, and therefore the changes proper to that tissue are more numerous and extensive, on the left side. And this may be the true explanation. Others, again, have conjectured that the arterial blood is more irritating than the venous. But there is no evidence of this: and it is better to content ourselves with noticing the fact, without attempting to account for it by mere gratuitous hypothesis.

You are not, however, to suppose that the right heart is exempt from valvular disease. When there is much change in the left, we often find a less degree of the same

kind of change in the right. The valves of the pulmonary artery are, perhaps, the least frequently of all the valves found otherwise than healthy.

Many of the alterations that take place in the internal lining of the heart result, apparently, from inflammation, which causes a deposit of lymph upon or beneath the serous membrane. The valves are apt to lose their thinness, their transparency, and their pliancy. They become thick, stiff, puckered, curled up, or glued to each other, or to the opposite walls of the channel. On the other hand, quite independently of inflammation, they may become morbidly thin, riddled with holes, and even rent asunder. What are called vegetations or excrescences may also project from them, very much resembling warts. Or they may be converted wholly or partly into bone.

FIG. 79.



FIG. 80.

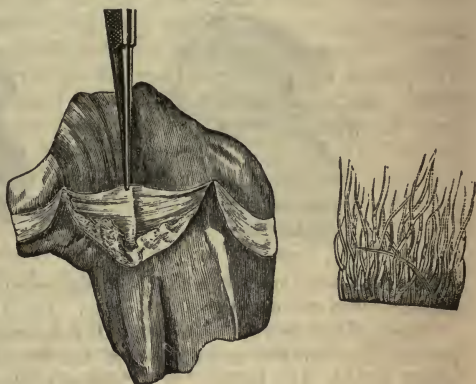
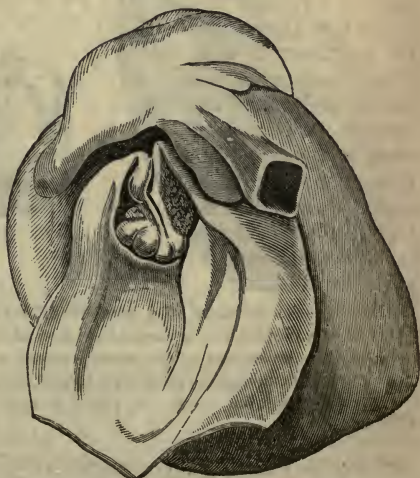


Fig. 79.—Fibroid thickening of the mitral valve.

Fig. 80.—Fibroid thickening of a pulmonary valve, extending symmetrically on both sides of the curtain, and consisting of a soft fibrillating deposit. It was found in a man who had a broken spine.

Alterations of some kind or another are very frequent in the *semilunar valves* of the *aorta*. When they are of such a nature as to diminish the orifice during the systole, they commonly occasion a systolic bellows sound. When the diseased valves offer no obstruction to the exit of blood from the ventricle, but do not close again immediately afterwards, so as effectually to prevent the reflux of that fluid from the aorta, they commonly give rise to a diastolic bellows sound. When both these defects of function occur, there is often a *double* bellows sound; a sawing alternate noise; one murmur during the systole, another, distinguishable in tone and quality, as well as in time, during the diastole. These sounds are conveyed along the tube in which they are formed, and are therefore most audible in the track of the aorta, as it leaves the heart. If the sound be diastolic, it will *take the place* of the smart clack of the second sound of the heart, or perhaps prolong it. Sometimes the new sound is very loud and curious. I had a patient in the hospital last year, in whom this diastolic sound was, in character and intensity, like the cooing of a pigeon. The patient could plainly hear it: nay, it could be heard by a person standing near him, but not touching his body, even with a stethoscope. In that instance we found one of the aortic valves irregularly thickened, with its free edge loose and flapping, and

FIG. 81.



Ossification of the mitral valves. From a preparation in Dr. Gross' cabinet.

unable to fulfil its function of closing the aperture. During the diastole it was retroverted, and vibrated in the regurgitating stream of blood; and thus, no doubt, the musical note, heard alternately with the first sound, was produced. In March, 1837, I heard in a man (Henry Milton) who was under Dr. Latham's care in St. Bartholomew's Hospital, and who had acute rheumatism, a very shrill diastolic sound, like the repeated whining of an imprisoned puppy-dog wishing to be released. This remarkable sound was audible, by means of the stethoscope, even in the radial artery. The patient died at last in St. George's Hospital, and his case is mentioned in Dr. Hope's book on the Heart. One of the aortic valves was torn downwards to some distance from its edge, and formed a flap which was perforated by a round hole.

FIG. 82.



Fig. 82.—Aortic valves of a child aged four years; they are opaque and thickened, and their free margin curled backward towards the artery. Two of the valves are closely united by their adjacent margins.—St. Bartholomew's Museum. 11th Series, 52.

FIG. 83.

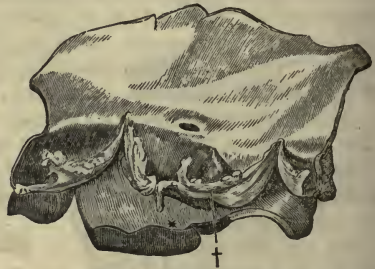


Fig. 83.—Atheromatous deposit in the valves of the aorta of a man aged 26, with rupture at the point marked by *; there was also congenital union at the point (marked by †) of two of the valves. The case is described in the Reports of the Pathological Society, vol. iv. p. 100.

I need not again point out to you the manner in which such disease of these semi-lunar valves tends to produce hypertrophy and dilatation of the left ventricle.

FIG. 84.

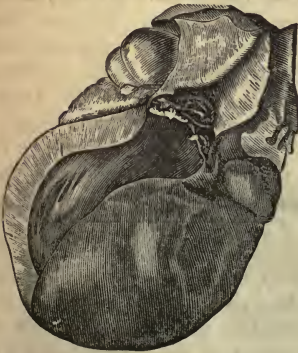


FIG. 85.



Fig. 84.—Aortic valves of a man æt. 47, rendered perfectly rigid by calcareous deposit. The patient was affected with granular kidneys and cirrhosis of the liver.

Fig. 85.—Ossification of the aortic valves; a thick calcareous deposit has taken place between the valvular membranes, interposing a rigid and almost imperforate diaphragm between the cavity of the heart and the vessel. A. Upper surface. B. Under surface.—From St. George's Hospital Museum, E 18.

The *mitral* valve is often thickened; and it is particularly subject, more so even than the aortic valves, to ossification. And the effects of the ossification are to prevent its closing the auricular orifice during the systole; and to prevent its lying flat against the walls of the ventricle, and allowing a free passage of the blood out of the left auricle, during the diastole. The orifice is often converted into an unvarying oval slit, having puckered edges, and resembling a button-hole; or the valve projects, like a thimble of bone, into the left ventricle. And it is remarkable how small the chink,

which is thus permanent, may be, and yet life go on. The heart having been taken out of the body, and the auricle filled with water, I have seen the water pass into the ventricle, by its gravity, *stillatim*; drop by drop.

Let me just remind you, that the direct and necessary consequence of constriction of the mitral orifice, is an accumulation of blood *behind* it; *i. e.* in the left auricle, in the pulmonary veins, in the lungs. Hence so much mechanical congestion, that the blood bursts at length through the bronchial membrane; hæmorrhage, slow or copious, ensues from the air-passages; and *pulmonary apoplexy* is formed.

In extreme cases, where the mischief is chiefly confined to the mitral valve, the blood necessarily reaches the ventricle in a penurious manner; that chamber contracts unsteadily and irregularly; and its cavity sometimes diminishes. This I think I have seen. But far more commonly there is disease of the aortic valves also; and the condition of the left ventricle is that of hypertrophy with dilatation.

When there is a permanent chink in place of the limber valve, there may be a double bruit. The first heard during the systole, and produced by the regurgitation of blood from the ventricle into the auricle, through the rigid slit. This is common. The second accompanying the diastole, and resulting from the mechanical impediment to the free passage of the blood from the auricle into the ventricle. This is uncommon. The constriction must be great for the diastolic murmur to occur at all: and when it does occur, it is faint; from the comparative feebleness (I presume) of the auricular contractions.

The form and the consistence of the altered valves being the same, no difference whatever in the sounds, or in the general symptoms, will arise from the particular nature of the changes. It will, I mean, make no difference whether the obstacle to the flowing blood, or the imperfect closure of the orifice, depends upon mere thickening of the valve by cartilaginous deposits, or upon ossification, or upon wart-like vegetations. These last may be found upon any of the valves, but like other morbid states they are less frequent on the right than on the left side of the heart; and they are most common of all on the aortic valves. Ossification — the accumulation of the phosphate of lime — is almost confined, I believe, to the left side. I never saw the tricuspid valve, or the semilunar valves of the pulmonary artery, converted into bone.

The warts, or wart-like excrescences, which are so often found upon the valves of the heart, are very curious things. Sometimes they are separate, and in rows like beads. Sometimes several appear to spring from a common base, which spreads out so as to exhibit a cauliflower appearance. And occasionally they hang in long strings from the valve into the adjoining chamber of the heart. In a patient of Dr. Hawkins', I saw a cylindrical excrescence of this kind which measured an inch in length. The valves presented slit-like perforations; and from the edge of one of these slits in the mitral valve, this long vegetation dangled into the ventricle. The whole of the valves of the aorta were covered, on their ventricular surface, with similar but shorter excrescences.

They vary much, these vegetations, in consistence. Sometimes they are soft, easily crushed, and capable of being readily detached from the smooth surface of the valve. Others are more firm, and yet separable from the valve without injury to it. Others, again, are so adherent, so rooted into the valves, that they can be removed only by tearing or cutting them off. They are found sometimes on the free edge of the valves; sometimes on their surface, or even on the inner membrane of one of the chambers, especially of the left auricle.

Much difference of opinion has prevailed respecting the nature and origin of these singular appearances. It was a common notion among the French, at one time, that they were really, what they so much resemble, venereal warts. What seems to be certain is, that they are somehow connected with *inflammation* of the internal lining of the heart; and of that which covers the valves in particular. But, then, are they lymph poured out from the inflamed membrane? or are they fibrin deposited from the blood upon an inflamed membrane? It is probable that the last is, sometimes at least, the true explanation of their origin. You know, that when the membrane lining a vein becomes inflamed, the blood in contact with it has a strong tendency to coagulate upon it, and to adhere to it. The fleshy excrescences found on the valves are often attached to the edges of slits in the valve: the broken surface having pro-

bably been the especial seat of inflammation. When the formation of vegetation is recent, they are very soft and frangible. But the most interesting fact that I am acquainted with, in evidence of the mode in which these little projections may arise, is one that accidentally came to light in one of Dr. Hope's experiments upon an ass, at which I was present. The aortic valves had been held back by a wire passed into the vessel, with the view of ascertaining the physical cause of the second sound. The animal had previously been rendered insensible by a narcotic poison; and the circulation was kept up—languidly, however, towards the last, by artificial respiration. Upon the final cessation of the heart's motions, the organ was removed from the body and examined: and the valve that had been mechanically irritated by the wire was found studded with these little wart-like appearances, which were so soft as to admit of being readily brushed off from the subjacent membrane. Here the deposit took place after the death of the animal, and while some of the functions of organic life alone were kept up by the artificial breathing.

Some curious circumstances still remain to be mentioned, which, in some instances, are connected with the formation of these warty vegetations. I shall not, however, enter upon them in the present lecture: but when I speak, at our next meeting, of rheumatic inflammation of the heart and its membranes.

Any or all of the lesions that I have been describing may and must lead, at length, according to their places and magnitude, to some of those changes in the actual and relative dimensions of the heart that were considered in the last lecture. They obstruct the stream of blood when moving in its natural course, and when its passage out to be free; or they allow of its *refluent* course, when that ought to be effectually opposed: and the necessary results, in either case, are dilatation of one or more of the chambers of the heart, with thickening, or with attenuation, as the case may be, of its walls. I have already spoken of the symptoms, physical and general, to which these secondary changes give rise; and of the treatment which they admit and require.

There being valvular disease, and that valvular disease giving rise to a bellows-sound, can we distinguish the particular valve affected? Generally, we can. Our skill in diagnosis outruns here, as indeed it too often does, our skill to cure. A few simple rules and considerations enable us, in most cases, to satisfy our natural curiosity to penetrate the exact condition even of changes that are incapable of repair. These rules relate chiefly to the time when the murmur is heard; to the direction in which it is most audible; and to the state of the arterial pulse.

When a bellows-sound accompanies the systole, it must be caused by a current passing *out* of a ventricle. But serious disease of the valves, sufficient to occasion a murmur, on the right side of the heart, is very rare. In nineteen cases out of twenty, valvular murmurs belong to the left side; so that practically the distinction lies, almost always, between two orifices, the mitral and the aortic, the inlet and outlet of the left ventricle. The natural inlet has become an outlet also: or the natural outlet is obstructed. Now if the sound be heard at the base of the heart, and along the track of the thoracic aorta, up towards the right clavicle, and even in the carotids; and if it be less audible towards the apex, and if the pulse be steady and regular, the mischief is seated in the semilunar *valves of the aorta*: there is some obstacle which produces a ripple in the onward stream of the blood.

On the other hand, if the pulse be irregular, and if the sound be better heard down towards the apex of the organ, on the left, it is owing to regurgitation through a diseased *mitral* valve. Such regurgitation is often attended with a purring thrill.

When, what scarcely ever happens, the sound does result from injury of the semilunar valves of the pulmonary artery, it is heard plainest in the track of that vessel, up towards the *left* clavicle. So, also, a murmur produced by change in the tricuspid valve would be loudest towards the apex, *on* the right. The arterial pulse for obvious reasons is but little influenced by disease affecting the orifices of the right heart.

Again, if the morbid sound be diastolic, it accompanies the entrance of blood *into* a ventricle; and for similar reasons to those assigned before, the fault is most probably in the *left* ventricle. It may be owing to the direct but impeded passage of the blood from the left auricle through a narrowed mitral orifice: yet this *very* seldom occasions any audible noise. Or the diastolic murmur may proceed from regurgita-

tion through the defective aortic valves; the natural outlet having become an inlet also; and this is exceedingly common. We attend, as before, to the situation and the track in which the sound is the loudest. We listen also for the smart clack of the natural second sound; and if it be not audible or be very indistinct, we have, in that circumstance, corroborative evidence of an imperfect aortic flood-gate. Moreover, we are again assisted by the pulse. The pulse of aortic regurgitation is, sometimes at least, very striking and peculiar: sudden, like the blow of a hammer, without any prolonged swell of the artery. This pulse always reminds me of the well-known chemical toy, formed by including a small quantity of liquid in a glass tube, exhausted of air, and hermetically sealed. On reversing the tube, the liquid falls from one end of it to the other with a hard short knock, as if it were a mass of lead. The sensation given to the finger by the pulse, when there is much regurgitation through the aortic valves, is very similar to this. It is as if successive balls of blood were suddenly shot along under the finger. Dr. Hope calls this pulse a *jerk*ing pulse; the pulse of unfilled arteries. And this abrupt pulse makes itself *visible* in the arteries; the wave of blood lifts, and moves, and sometimes contorts the vessel. When this kind of pulse occurs with a diastolic bellows-sound heard along the track of the aorta, and the short clack of the second sound is absent or muffled, you may be quite sure that the aortic orifice is patulous during the diastole. The reflux of the blood, when the patency is great, is strong enough sometimes to produce a palpable shock or jog, called the diastolic impulse. And this refilling of the ventricle from the artery may even provoke it to a supernumerary contraction.

In a patient by whom I was lately consulted, the hard, sudden, hammering pulse led me to conclude that the blood regurgitated from his aorta: and accordingly, upon applying my ear to his chest, I discovered a loud murmur, coincident with the diastole, and most distinct in the direction of the right clavicle. The shock of this man's artery was plainly to be felt through his clothes, by one's hand laid lightly upon the bend of his arm. His wife told me that, for five years past, this jarring blow had made it uncomfortable for her to take his arm when they were walking together. The same kind of jerking impulse was strikingly perceptible in the femoral arteries, and in the carotids.

Of regurgitant sounds belonging to the right side of the heart I can tell you nothing. I never heard one, that I know of, from the pulmonic valves. Through the tricuspid orifice the blood is believed to be often reflux; causing, as I stated before, turgescence and pulsation of the jugular veins. The structure of the valve permits this ebbing movement of the blood under circumstances which might otherwise be perilous. The tricuspid has accordingly been called the *safety* valve of the heart. But the reflux seldom, if ever, announces itself by a bellows-sound.

We cannot always thus rigidly connect morbid changes with definite signs. Disorders arise of which the symptoms are more cognizable and constant than the nature or exact seat. We assign a name to the peculiar assemblage of symptoms, and make it thenceforth a distinct object of our study; tracing the symptoms as well as we can up to their organic causes and conditions. Now the complaint called *angina pectoris* is one of this kind. It is, moreover, a very curious and a fearfully interesting disorder; and I shall devote the remainder of the present hour to its consideration.

This disease was first accurately described, in this country at least, by the celebrated Dr. Heberden, the author of the Commentaries. It had been adverted to by many writers before, but obscurely; and Dr. Heberden's observations were quite original. The description that he has given of the complaint, in the second volume of the Transactions of the College of Physicians, is very accurate and striking. He calls it a *disorder of the breast*; and observes that "the seat of it, and the sense of strangling and anxiety with which it is attended, may make it not improperly be called *angina pectoris*."

"Those who are afflicted with it are seized whilst they are walking, and more particularly when they walk soon after eating, with a painful and most disagreeable sensation in the breast, which seems as if it would take their life away if it were to increase or to continue. The moment they stand still all this uneasiness vanishes. In all other respects the patients are, at the beginning of this disorder, perfectly well; and in particular have no shortness of breath, from which it is totally different."

Such is the brief description of the malady given by Dr. Heberden. You will observe, that the distress occurs in paroxysms; and the patient, at first, has intervals of apparent health: and even when the disease is more advanced, he has periods of comparative ease between fits of suffering. The paroxysms are especially liable to come on when the patient is walking, and, above all, when he is ascending, — going up a hill. He is then seized, all at once, with a very painful sensation, which seems to be, in many cases, indescribable, but which is always referred to the heart, or its neighbourhood. Sometimes the sensation is spoken of as being a spasm, as giving the sufferer a notion of constriction. I have been told by one who laboured under this disorder, that he felt, during the paroxysm, as if the sides of his chest were held together by a transverse bar of iron. The impression is constant that to continue the exertion which has produced the attack — to stir another step — would be fatal. Yet the patient is not out of breath. It is not dyspnoea that oppresses him; for he can, and generally does, breathe freely and easily. He lays hold of any neighbouring object for support. His face is pale and haggard; and you would suppose, from his appearance, that he was actually at the point of death. But in the early stages of the disease, the pang soon subsides, the distress is over, and the patient is entirely himself again. It is a singular fact, which I cannot at all explain, but of which I have been assured by several persons affected with this disorder, that when the pain and inability to stir a step further have come on after a short walk, and have subsided upon the patient's stopping, he has often been able to resume his walk, and to pursue it for a long while, without any repetition of the distress.

After the lapse of some time, perhaps of some months, the anguish does not so instantaneously cease upon standing still; nor does it always require some bodily exertion to bring it on. It will occur when the patient is quiet, even in bed. He feels as if the action of the heart were arrested; and he is obliged to rise up, every night it may be, for many weeks together. In exquisite cases it will be brought on by causes of any kind that slightly accelerate the circulation: coughing, straining at stool, mental emotion.

The pain, which is at first referred to the left mammary region, shoots backwards often, towards the spine, or across the chest below the clavicles. Frequently it extends, accompanied by a sort of numbness, to the left shoulder, and down the left arm; stopping short, in a curious manner, and from some inexplicable cause, either just about the insertion of the deltoid muscle, or at the elbow, or at the wrist. Sometimes, however, it runs down to the very extremities of the fingers; particularly of the last two fingers, following mainly the course of the ulnar nerve. Occasionally similar pains affect the right side and arm; and now and then, all the four extremities at once. There is (I say) no dyspnoea in the genuine form of the disease; although you will find it stated by some modern writers, of good repute, that the paroxysm is accompanied with difficulty of breathing. In the instances that I have seen, and they have not been very many, the patient was able slowly and fully to inspire and expire, even when the fit was on him. The truth I believe to be, that other affections, more akin to asthma, have been confounded with *angina pectoris*; and this confusion has led to the belief, that it is not altogether so dangerous a complaint as used to be thought: but in its genuine shape it is undoubtedly a very fatal disorder. Sir John Forbes, by a diligent search among authors, has collected some statistical facts respecting it, which are worth remembering. Thus, out of eighty-eight cases, eight cases, or one in eleven, occurred in females. The ages in eighty-four of these eighty-eight cases are recorded; and of the eighty-four, seventy-two were above fifty years; and twelve, or one-seventh of the whole, under fifty years. It is a disease, therefore, for the most part, of advanced life: and this alone would afford a strong presumption of its dependence upon some organic change. Again, the event of the disease was recorded in respect to sixty-four of the patients. Of these forty-nine died, almost all of them suddenly; while fifteen recovered or were relieved. And among the forty-nine fatal cases, there were only two of women.

That the seat of the disorder is the heart, or the aorta, and that it consists in some structural change, can scarcely be doubted. Yet some pathologists are disposed to consider it a merely neuralgic affection, "commencing, for the most part, in the pneumogastric nerve, and spreading in different directions, as other nerves becomes involved." But this doctrine is scarcely consistent, in my judgment, with the facts —

First, that the paroxysm is excited by such causes as are "especially calculated to disturb the natural action of the heart, bodily exertion, and mental emotion;" and, secondly, that the disease is so very frequently and so suddenly *fatal*. This is not all the character of mere neuralgic diseases in general. And when we add to these facts the further fact, viz. that, in a vast majority of instances, organic disease of the heart, or of the great blood-vessels, has been discovered after death, I think we shall be obliged to admit, that the symptoms are often (I believe I might say always) dependent upon cardiac disease. One theory explains the "breast pang," by supposing that the blood, whenever its movement is accelerated by exercise or otherwise, arrives in the heart faster than it can be transmitted onwards; and accumulating in its cavities, painfully distends them. I confess that this commends itself to my mind as being a very reasonable theory. The great Dr. Jenner took a most ingenious view of the matter, which was made public and further enforced by Dr. Parry. He had found, in examining the bodies of some who had died of well marked angina pectoris, that the coronary arteries of the heart were ossified; converted into bony canals, and constricted in their calibre. He thence concluded that the paroxysms result from the circumstance, that when some increase of the muscular contraction of the heart happens to be called for, the increased supply of blood, rendered necessary by the additional exertion, is not capable of being furnished by the diseased nutrient arteries of the organ; that the heart comes to a stand, because its muscular tissue is not duly injected with arterial blood. The patient is on the very brink of fainting: nay, does at length faint irrecoverably. He accordingly called the disease "*syncope anginosa*." And this simple and beautiful theory was for some time admitted as the true one. However, later investigations have abundantly shown that angina pectoris may occur in a decided form, without there being any ossification or other disease of the arteries; and, on the other hand, that the coronary arteries may be ossified, and yet no angina pectoris be the result. Moreover, Dr. Jenner's theory does not account satisfactorily for the *pain*.

I may here again avail myself of the researches of Sir John Forbes, and give you a numerical account of the organic changes in the heart that have been found associated with this disease. The total number of instances collected by him, in which the body was examined after death, was forty-five. Of this number there was disease found in the liver only, in two instances: organic disease of the heart, or great vessels, in forty-three. Sir John Forbes, indeed, makes the last number thirty-nine, instead of forty-three, excluding four cases in which nothing morbid was found in or about the heart, except an excessive coating of fat. This Dr. Fothergill considered the essence of the disease: and certainly a heart cannot be said to be in a healthy condition which is thus loaded with adipous matter. The fat is generally deposited at the expense of the muscular substance, which is apt in such cases to be thin, pale, and soft; atrophied, in short. Taking, however, the table as it is given by Sir John Forbes, the thirty-nine cases, in which there was no disease except in the heart and great vessels, were thus distributed:—In ten of the cases there was organic disease in the heart alone; in three, organic disease of the aorta alone. In one instance only was the disease confined to the coronary arteries; but there was ossification, or cartilaginous thickening, of the coronary arteries, combined with other disease, in sixteen instances. Again, there was ossification, or other disease of the *valves* of the heart, in sixteen cases also. There was disease of the aorta (ossification, or dilatation, or both), in twenty-four cases; and in twelve cases there was preternatural *softness* of the heart.

Now I strongly suspect that this last condition, preternatural softness—in other words, fatty degeneration—will ultimately prove to be the main physical condition of angina pectoris. To express what I mean somewhat differently:—that group of symptoms to which we give the name of angina pectoris, is (as I conjecture) an authentic exponent of that physical state of the heart to which we give the name of fatty degeneration. When the examples collected by sir John Forbes were recorded, this morbid change was unrecognised by medical science, and the diminished consistence which it implies would easily escape notice. Observe that the two things do often assuredly go together. Several of Dr. Quain's instances of fatty disease, were also instances of true angina. Disease of the coronary arteries is perhaps the most frequent source of partial fatty degeneration. Disease of the coronary arteries is

perhaps the most frequent accompaniment of angina pectoris. The substantial change, and the nominal disorder, both belong to the same advanced period of life; both are prone to end in sudden death, and in sudden death of the same kind, death by syncope. As exacter observations multiply, I expect that angina pectoris will be acknowledged as the surest, as the only sure, indication of a fatty heart.

Pain of a peculiar character; a feeling of immediate dissolution, terminated often by actual syncope: these are the prominent and principal elements of angina pectoris. If these depend on fatty degeneration, it is supposable that one or more of them may be wanting. Dr. Quain teaches us that the inexpressible sense of dying is sometimes the only symptom of the disease which he is portraying. Sometimes the pain is added; frequently the syncope. And this is intelligible if (what I think probable) the anguish results from over-distension of the unsound heart. Syncope may occur without such distension. The softened state of the muscular tissue would admit of its being stretched a little whenever the centrifugal reaction of the contained blood was augmented; augmented by posture, by bodily effort, or by mental agitation. And this facility of yielding under a less and less degree of disturbing pressure would accord with the ascertained fact that fatty degeneration is a progressive change. The cardiac nerves may be variously implicated in the textural alteration, and may variously resent the distending force. That slight over-expansion of a heart so diseased should excite the breast-pang is conceivable enough. We are familiar with pain of a similar sharp kind when the intestine is stretched by pent-up gas in colics. The *juvantia* of the disorder favour the same view. Stimulants, helping the languid and labouring muscle to contract effectually upon its contents, are effectually also, often, in relieving the paroxysm. This appears to me a more probable theory of the phenomena of angina pectoris than the theory which ascribes them to mere neuralgia, or the theory which ascribes them to spasm of the heart. Yet both these theories have been speciously advocated. To that which I offer you, you may give as much weight as it may seem to deserve.

I have spoken of this disorder as continuing to recur for months; it may even be for years: but it is frequently fatal within a much shorter period. Nay, the time between its first manifestation and its mortal close is, not very seldom, appallingly brief. My friend, Dr. Latham, lately gave me the following sketch of a case of this kind, which had fallen under his own observation. A gentleman, about fifty years old, was recovering from the influenza, of which nothing remained but a slight cough, that troubled him at night. It was to relieve this that Dr. Latham was consulted. The gentleman looked perfectly well. After Dr. Latham had prescribed for this little ailment, the patient begged to see him the next day to talk over with him (he said) a very strange affection he had. Accordingly he then described a paroxysm of angina pectoris in terms that could not be mistaken; dwelling especially upon the præcordial pain, the sensation down the left arm, the feeling of approaching dissolution, and then the perfect recovery. This gentleman had, during the previous summer, performed a walking tour through Switzerland, and returned home in excellent health. The first notice of his angina was less than a month ago, when he was walking up Hampstead Hill. It was then that he had his first paroxysm. In the short period which had elapsed, the attacks had rapidly increased in severity and frequency: occurring now every two or three days, or every day, or several times a day, with or without an obvious exciting cause. Dr. Latham made a careful examination of the chest, and found the respiration perfect, the heart free from all unnatural murmurs, and its beats rhythmical. The only thing that particularly attracted his notice was the exceeding feebleness of its impulse. In the afternoon of the next day Dr. Latham visited him again, when he described a paroxysm he had suffered in the course of the morning, of much greater severity than any that he had hitherto experienced. Dr. Latham saw enough to convince him that his patient's existence was very precarious: and as he had previously been a stranger to him, he inquired about his friends, and took down the address of a brother, intending to call and apprise him of what he feared. On reaching his own home, two hours afterwards, a messenger met him, announcing that his patient had fallen into another paroxysm, soon after he left the house, and was dead. The body was carefully examined by a thorough anatomist, Mr. Stanley. There was no disease of the aorta,

or of the heart generally; but the coronary arteries resembled tubes of coral, being completely ossified as far as they could be traced.¹

The patient may even expire in the first or second paroxysm. This happened in the case of the late lamented Master of Rugby school.

You will perceive, from what has been said, that the prognosis of this singular and formidable affection is extremely unfavourable. The cases are very rare in which no disease of the heart has been detected: and the organic changes that have been found are remediless, and for the most part, progressive: and, in point of fact, the great majority of those who have laboured under the disease have died suddenly, and prematurely.

It follows also, as another corollary from the facts now brought before you, that there are very few cases in which we can dare to contemplate a cure. Our measures must be preventive when the paroxysms are absent: and our object will be to shorten the fit when it is present and protracted.

Now the preventive measures are simple and obvious. The patient must be cautioned to avoid the exciting causes of the paroxysm; walking up hill; or against the wind, which has also often been known to produce it. Whatever is likely to hurry the circulation, and therefore, among the rest, all mental emotion and anxiety, should be guarded against as much as possible. John Hunter died of angina pectoris: and the fatal seizure was brought on by a fit of anger. The very same precautions must be observed as those which I dwelt upon half an hour ago; and for the very same reasons. Care should be taken also to obtain and preserve a healthy state of the digestive organs. It is observable of this, as I told you before it is observable of other cardiac diseases, that they are often attended and aggravated by flatulence of the stomach and bowels. Persons labouring under a paroxysm of angina often experience great and sudden relief upon getting rid of a quantity of gas, by which the stomach had been distended. The flatulence acts, no doubt, by pressing the diaphragm upwards, and so diminishing the dimensions of the thorax, and impeding the play of the heart. It is upon the same principle that we must explain the fact, that the paroxysms are particularly apt to come on if the patient walks *soon after a meal*: also that they occur in the night, when he is in a horizontal position, and are relieved by his getting out of bed; that is, by his assuming the vertical posture, and taking off the pressure of the abdominal viscera from the diaphragm.

In the paroxysm itself, bleeding has been fairly tried: but, as I think might have been foreseen, it has seldom been followed by any benefit, and sometimes it appears to have done harm. The affection has a nearer relation to syncope, and often to syncope by asthenia, than to anything else. That is the way in which the patients die: and consequently, cordials, stimulants, and antispasmodics, are found to be of service. For the pain, when it is lasting as well as severe, the appropriate remedy is opium. Dr. Elliotson thinks prussic acid is the best thing you can administer. Dr. Davies has more faith in belladonna plasters than in most other things. Dr. Copland advises stimulant liniments externally; and warm carminative or aperient medicines, as the circumstances may require, internally. Hoffman's anodyne, under my own observation, has proved exceedingly useful. The general condition of the sufferer will suggest, I believe, the proper treatment. Not that it will suggest any particular drug, but it will teach you the main principle on which you are to proceed. If auscultation reveal any of those morbid states of the heart which were noticed in the beginning of the lecture, the means which I pointed out as suitable for remedying them may be put in force.

¹ The history of this case has since been published by Dr. Latham in his admirable *Lectures on the Diseases of the Heart*. He adds the important circumstance that "its (the heart's) muscular substance was more loose of texture than natural, but not softened in an extreme degree." He also gives a most interesting detail of Dr. Arnold's seizure and death: and he describes another instance, seen by himself, in which the period between the patient's first paroxysm and his last did not exceed ten days.

In this case "the muscular substance of both ventricles was so soft, as to be pierced through with the slightest pressure of the finger." In the spring of last year a well known baronet died in London of angina pectoris; the disease having run the whole of its manifested course in nine days.

LECTURE LXI.

Pericarditis: its frequent connexion with Acute Articular Rheumatism. Rheumatic Carditis. Anatomical characters of Acute Inflammation of the Pericardium; of the Endocardium. General symptoms. Auscultatory signs. Relations of Carditis with Rheumatic Fever.

I YESTERDAY considered, cursorily indeed, but as fully as the limits of these lectures will permit, the effects of hypertrophy, and of dilatation with and without hypertrophy, of the several chambers of the heart: and the means we possess of obviating or alleviating those effects. The chronic changes to which the *endocardium* is liable, especially in those parts where it covers the valves, and the tendinous rings that support the valves, were next reviewed; with most of the circumstances which give origin to such changes. Lastly, I spoke of that singular and perilous affection to which the name of *angina pectoris* has been applied. I proceed this afternoon to the diseased conditions of the *pericardium*. This membrane is often the seat of acute inflammation; and the consequence of such inflammation is sometimes, though rarely, the speedy extinction of life. But in nineteen cases out of twenty, the disorder proves fatal at a remote period; destroying the subject of it more slowly indeed, but almost as surely. Pericarditis is, therefore, and has always been regarded as, a very interesting disease; and the more so, that it is in many instances a very *insidious* disease also.

Acute pericarditis is liable to arise, like all other internal inflammations, after exposure to cold; or when no exciting cause is to be discovered. It sometimes follows blows received upon the chest; or other mechanical injuries. It is no uncommon result of a contaminated state of the blood, induced by that peculiar renal disease which I formerly mentioned as being one great source of General Dropsy also. But by far the most frequently of all does it happen, in connection with another complaint that we have not yet had before us—acute rheumatism; a febrile disorder, characterized by inflammation of a specific character, affecting the structures that lie around the joints, or enter into their composition—the *fibrous* tissues. I shall therefore consider acute pericarditis with reference to its occurrence in rheumatic fever; for in so doing I shall embrace all the practical points which belong to it under any form. But I must tell you that when *pericarditis* happens, in the course of an attack of rheumatism, so also, to the best of my belief, in almost every instance, does *endocarditis*. For this reason I shall include, in the account I am desirous to give you of *rheumatic carditis*, both these inflammations: inflammation of the investing membrane, and inflammation of the lining membrane of the heart. I mentioned in the last lecture, that, with respect to the latter, to endocarditis, there were some peculiarities noticeable, which I should reserve for the present occasion.

The pericardium is one of the serous membranes; so also may the endocardium be considered. But the pericardium is also a shut sac; and the primary effects of inflammation upon it are the same, *mutatis mutandis*, as upon the shut sac so near it, the pleura. The second series of effects is, however, much more formidable. Adhesion of the pleura does not necessarily abbreviate the natural term of the patient's life; adhesion of the pericardium almost always does: and effusion into the cavity that contains the lung is far less serious than effusion into the bag that surrounds the heart. In the one set of organs the mischief may be great, but it is *final*; in the other, it leads, in most instances, with unfailing certainty, sooner or later, to worse changes, which at length prove incompatible with the further continuance of life.

You will understand, then, without any necessity for my going again into much detail, that the pericardium, under acute inflammation, may undergo the same changes, which, on a former occasion, we saw that the pleura might undergo. Coagulable lymph may be poured forth from the entire membrane, and abolish the cavity by glueing the whole of the pericardium to the heart: or serous fluid may be effused, distending the bag of the pericardium, and keeping its smooth surfaces more or less

asunder: or both serum and lymph may be effused together: or fibrin, in some shape or another, may be *deposited*, for aught I know, from the homogeneous fluid which is thrown out by the inflamed membrane in the first instance; and the result of this mixed effusion may here also, as in the case of the pleura, be the *partial* adhesion of the membrane to the heart.

But in the majority of instances the inflammation spreads over the whole membrane, as it is apt to do in serous membranes generally; and one of these two things happens: either there is a large quantity of liquid effusion, which is not reabsorbed; and then usually the patient dies in a few days: or there is not much liquid effusion, or the liquid part is absorbed, and the pericardium becomes permanently agglutinated to the heart, and *apparent* recovery takes place.

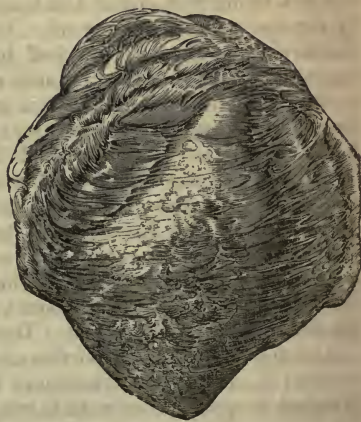
In the cases that have proved fatal at an early period, when the inflamed membrane has been unadherent, it has been found to contain serous fluid; sometimes clear, oftener turbid, frequently tinged with blood; and it has been seen to be covered with a coating of the fibrinous or albuminous part of the blood; what we call plastic or coagulable lymph. The deposited lymph assumes a variety of forms in different cases; but in every case that I have seen, the prevailing character of the unattached surface has been that of *roughness*; and this is a circumstance of some importance, as we shall presently see. The lymph is not arranged in smooth layers; but it is rugged, villous, or cellular. According to the fancy of different observers, it has been thought to resemble lace-work, a sponge, a honeycomb, some kinds of coral, or the interior of a calf's stomach. Sometimes it bristles with a multitude of small, short, pointed papillæ; less frequently it is softer and shaggy; always it is rough and uneven. Dr. Hope, following Laennec, states that the surface looks something like that which would be produced by suddenly separating two flat pieces of wood, between which a thin layer of butter has been compressed. To my own eye, the appearance presented by the membrane, in its recent condition, has been more like the rough side of the pieces of tripe which you see in the butchers' shops, than anything else.

When, on the other hand, the patient dies, as he sometimes will do, soon after the whole of the membrane has become adherent, you will find the medium of adhesion to consist of lymph, in which a number of bloody points or streaks are visible: but still the connecting substance is soft, and the agglutinated membranes can readily be torn asunder.

Such is the state of things on the *outside* of the heart in such cases. But what do we find *within*? Why, here also, in *all* cases *probably*, certainly in by far the majority of cases, we may discover evident traces of inflammatory damage; and we discover them chiefly on the valvular apparatus. There does not appear to be such a tendency in endocarditis to diffuse itself over the whole membrane. Occasionally that naturally transparent portion of it which covers the muscular fibres is rendered whitish and opaque; and occasionally some of the deposits that are common on the valves, encroach also somewhat beyond them, and even stud, here and there, the interior of one or more of the chambers of the heart, and especially of the left auricle. But the valves or the fibrous rings from which they spring, are the parts first and chiefly implicated, especially the aortic valves, and the mitral valve, not uncommonly the tricuspid valve also; and sometimes even the semilunar valves of the pulmonary artery. Inflammation thus affecting both the external and internal membranes of the heart, in acute rheumatism, I would call *rheumatic carditis*.

The inflamed valves undergo two kinds of change, distinct from each other. They become thicker than natural; they lose their transparency and pliancy, and are puckered. These changes depend upon the deposit of lymph *beneath* the membrane;

Fig. 86.



Pericarditis, showing the appearance of recently effused lymph. From a specimen in Dr. Gross' cabinet.

between the membrane and the fibrous substance which it covers. Sometimes they are folded down, and glued, as it were, to the opposite surface. This must be by coagulable lymph deposited on the *outer* side of the membrane. But more frequently than all, they present more or fewer of those wart-like excrescences, or fleshy granulations, which I spoke of in the last lecture, and which are of course *upon* the free surface of the membrane. Sometimes these vegetations are scattered irregularly over the convex surface of the valve, or in its immediate neighborhood; much oftener they have a more definite and curious distribution; an arrangement which I have never seen noticed by any author, but which it has been my lot so many times to observe, that it has led me to remark an anatomical peculiarity with which it is connected; and this piece of minuter anatomy I have looked for in vain in books, and I have in vain sought information about it among all the anatomists of my acquaintance. They none of them have seemed to be aware of it, though they acknowledged that the fact was so when I pointed it out to them. Recently, indeed, I have been told that the peculiarity of structure to which I allude is somewhere adverted to by Morgagni.

It becomes necessary, therefore, that I should describe to you this discovery of mine (if it deserve so grand a name), as I have, for several years past, been in the habit of showing it to the pupils of the hospital in the dead-house. It derives its chief interest from the light which it throws upon the morbid appearances to be spoken of presently. You will find, then, if you examine closely the semilunar valves of the aorta, or of the pulmonary artery, that in each valve there may be distinguished two parts; one thicker, the other thinner. The thicker part lies next the base of the valve; the thinner next its edge. And the valve does not become thin by degrees, but the difference is marked by a manifest line of separation between the thicker and thinner portions; and this is not a straight, nor even one sweeping curved line, but it forms a double curve. It consists of two semicircular lines, running each from the centre of the edge of the valve, from the sesamoid body there situated, to either extremity of the edge, where the edge joins the side of the aorta. So that there are two segments, of a crescentic shape, thinner and less opaque than the remaining part of the valve, and lying near its free margin. This peculiarity of structure is uniformly present. It is less distinctly visible in the valves of the pulmonary artery than in those of the aorta; and it is much less apparent in some individuals than in others; but it is always to be seen when it is looked for.

The anatomical account of this arrangement is not far to seek. The cardiac valves consist of a loose duplicature of the delicate endocardium, between the folds of which is received a thin prolongation of fibrous tissue, from the tendinous rings surrounding or constituting the several orifices that are furnished with a valvular apparatus. In the semilunar valves this fibrous substance does not interpose itself between the entire space of the folded membrane. It reaches the free edge of each valve at three points only; namely, at the centre, where it forms the *corpus Arantii*, and at the two extremities. Between these points it stops short, and has a definite limit and outline; a scalloped edge: and so leaves two crescentic portions of the valve formed merely by the doubled endocardium. The crescentic margins are thin and transparent: the remaining shield-shaped portion of the valve is more or less thick, firm, and opaque.¹

And the physiological reason of this arrangement is also apparent enough; though I failed to perceive it until it was explained to me by Mr. Thurnam. Each valve, when opened out, is convex towards the ventricle. The three valves do not merely meet by their edges. Their common purpose would be but insecurely provided for if such were the case. They meet and bend up, and come broadly into contact with each other. Each valve during the diastole has its right and left crescentic portion applied respectively to the corresponding portion of its right and left fellow valves. The thin segments are pressed mutually together, and lie *dos-à-dos*, as dancers say; while their edges look in the direction of the vessel. All this you may convince yourselves of by injecting the aorta of an ox with wax, and picking out the wax when it is cold.

Now the curious fact which first led me to remark this natural structure is, that

¹ This formation of the arterial valves is described and delineated by Morgagni, in his *Adversaria Anatomica*; as Dr. Todd, since the publication of these Lectures, has been good enough to point out to me.

the minuter vegetations, which form upon the aortic valves, in acute rheumatic carditis, most commonly arrange themselves in a row, like a string of beads, along the line of union between the scalloped edge of the thicker scutiform portion of the valve, and the inner convex margin of the two thinner crescentic portions. Sometimes they follow that double festoon very exactly and completely: sometimes the continuity of the line is broken, and the excrescences straggle from it a little; but still the general tendency to adhere to it is evident. No one that I know of has publicly noticed this fact; yet that it is a fact, a good many persons, who have been for some time about the Middlesex Hospital, are perfectly aware. The truth is, that death seldom happens early in these cases; and perhaps the valves have not always been carefully examined when opportunity did offer. My friend, Dr. Latham, had been watching for such a case in vain for some years. At length, however, two of his hospital patients died in the first attack of rheumatic carditis; and he tells me that, looking with great interest for the morbid appearances within the heart, he found them such as I have been stating. I have chanced to see six or eight such early fatal cases.

The arrangement just described is the most common one, so far as the aortic valves are concerned; but sometimes even there, and generally upon the mitral and the tricuspid valves, the little wart-like excrescences have a different position; jaggling the free edge of the valve with numerous fine serræ, like the teeth of a small saw; or being disposed, just within its border, in one continuous line.

After what has been said, you will readily detect the physical cause of this curious distribution of the wart-like excrescences. The membrane suffers inflammation. Soft lymph exudes from it, or is deposited upon it; and as fast as it is formed or deposited, it is pressed aside, by the repeated concurrence of the opposed surfaces, from the crescentic portions of each valve; and heaped up along the boundary lines of contact; just as a thin layer of butter on a board would be displaced, and heaped up in a little curvilinear ridge, by the pressure of one's thumb. The double festoon, and the little marginal teeth, are obviously both formed in this way.

If my verbal description has been insufficient to make all this clear to your apprehension, the drawings before you speak, I hope, in plainer language.

These, then, are the appearances commonly seen within and without the heart, when the patient does not long survive the first attack of rheumatic carditis. When death takes place at a later period, you find more than this. You find the consequences which flow from these primary lesions, operating as mechanical causes of further change: hypertrophy and dilatation in their various degrees and combinations; or, sometimes, atrophy.

You will please to bear the primary changes in mind; for they satisfactorily account for the *physical signs* of pericarditis and endocarditis which are displayed in these cases, and which I shall describe and explain after I have shortly inquired into the *general symptoms*.

The symptoms, then, of pericarditis, as set down by authors, and such as I have myself frequently noticed, are the following. There is often, very early in the disease, a singularity of manner, and peculiar expression of countenance, difficult to describe, yet strikingly manifest to the observer; a strangeness of deportment mixed somehow with an aspect of distress. To this are frequently added, pulsation within the chest; a sense of oppression in the epigastrium; a catch in the breathing; a dry cough; inability or unwillingness on the part of the patient to lie on his left side; pain in the situation of the heart, increased by a full inspiration, by pressure upon or between the corresponding ribs, and more particularly increased by pressure upwards against the diaphragm by means of the fingers thrust beneath the cartilages of the false ribs; stiffness and pain in and about the left shoulder, and extending thence down the left arm, and stopping short perhaps at the elbow or wrist. This last circumstance, however, the pain shooting down the arm, is more common in *chronic* affections of the heart. Sometimes jactitations occur, like the jactitations of chorea. And I have yet another symptom to mention, and a very important one; and that is delirium, sometimes quiet, but often wild and furious delirium, not dependent upon any disease of the encephalon.

Of course there are also the febrile symptoms which accompany the acute rheuma-

tism; or if the pericarditis occur independently of acute rheumatism, there will usually be fever symptomatic of the local inflammation.

Now each of these symptoms I have repeatedly observed; but they seldom all concur in the same case. If they did, there would not be much difficulty in the diagnosis: nor would the cardiac disease be so often overlooked as it is. The diagnosis of pericarditis has been confessedly uncertain and obscure. Not unfrequently, nearly all the symptoms that I have been enumerating are wanting; or are so indistinctly marked as to attract no attention. It is therefore an important matter to ascertain what help we may derive, in these equivocal cases, from auscultation.

In truth, the help which we sometimes get is peculiarly valuable and satisfactory. There are characteristic morbid sounds to be heard when the heart is beginning to labour under rheumatic carditis.

The morbid sounds which may reach the ear applied in such cases to the surface of the chest are two: very distinct the one from the other, and very distinguishable; depending upon different causes, and denoting diversities of operation and of site in the morbid processes going on within. But they are not both audible in all cases.

One of these sounds I have been accustomed to call a *to and fro* sound. It conveys to the ear the notion of the rubbing of two rough surfaces, backwards and forwards upon each other. It seems near to the ear; and therefore near to the surface of the patient's body. Like all the other morbid sounds heard within the chest, it is capable of much variety in tone and degree. Sometimes it very closely resembles the noise made by a saw in cutting through a board. Sometimes it is more like that occasioned by the action of a file, of a rasp, of a nutmeg-grater. But its essential character is that of *alternate rubbing*; it is a *to and fro* sound. This very peculiar sound I had noticed and described, and explained, before I was aware that it had attracted the attention of any other persons. Others, however, had remarked it, and had correctly interpreted its meaning. I claim no credit therefore for the discovery of what I think a very important symptom: but I claim for the symptom itself that additional weight which accrues to it, from its having been originally perceived by different observers, independently of each other. The physician who, in this country, without my being aware of it, had noted and published some cases in which this phenomenon occurred, is Dr. William Stokes, of Dublin. There is a good deal said about it by Bouillaud also; and he too appears to have discovered the sound, without any previous knowledge of its having been noticed by others. I have heard the *to and fro* sound now in some scores of cases. In a few of these it never ceased except with life. The patients died during the primary attack, and the *to and fro* sound remained as long as the heart continued to beat. In all the other cases, the *to and fro* sound was audible for a few days only, and then ceased entirely, and probably for ever: the patients recovering more or less completely.

The other of the two morbid sounds, is the ordinary bellows-sound, with which you are already familiar. In the case in question it is a single sound; a deep-seated rush, or whiz, accompanying the systole of the heart. It usually continues long; often for life.

These two sounds, the superficial *to and fro* sound and the deep-seated bellows-sound, may sometimes be heard, by a careful listener, to exist together. Sometimes the bellows-sound begins to be distinguished when the rubbing sound ceases; appears to supervene upon it, or to take its place; perhaps it then first becomes audible, simply because it was previously drowned in the louder superficial sound. Sometimes there is no *to and fro* sound, but only the deep blowing noise; or (what in many cases is extremely probable, nay, what I may venture to say is certain) the *to and fro* sound has *come and gone* unnoticed—unlistened to.

Now of these sounds, which I repeat are perfectly distinct, and capable of being easily discriminated the one from the other, the first mentioned, viz., the *to and fro* sound, is always indicative of inflammation of the *external* membrane; the other, the bellows-sound, is always, as I believe, in these cases, indicative of inflammation of the *internal* membrane of the heart. You will bear in remembrance, that I am speaking of these sounds as they somewhat suddenly occur for the first time, and especially as they are apt to occur in *rheumatic* carditis at its *first* accession.

Those of you who have seen the thorax opened in an animal whose heart still continued to palpitate, may have observed, as I have done, that the pericardium lies

closely in contact with the heart, but that a considerable extent of slipping motion between them goes on at every successive act of systole and diastole. They glide over each other evenly and without noise; but this is only while the surfaces are smooth and healthy. When they are already made rough by inflammation and the deposit of lymph, which lymph always, as I have shown you, is rough in such cases, then the attrition will be no longer noiseless: it will give rise, in the alternate movements of the organ, to the harsh and superficial *to and fro* sound. But why does that sound, when once it has occurred, at length cease; and why, having once ceased, does it never by any accident, when the inflammation has been universal, recur? Clearly because the pericardium has become adherent to the heart: after which there can be no motion of the one membrane upon the other, and therefore no sound indicative of such motion.

That this is the true explanation of the occurrence, and of the permanent cessation, of the *to and fro* sound, I am now (March, 1837) convinced. It was a matter of inference with me for some time. A few of the patients died during the primary attack. By much the majority recovered. I do not mean got perfectly well as they had been before; but they regained a great share of their usual health, perhaps seemed, and *thought* themselves *well*, and left the hospital where they had been under treatment. Now, of those who died, the pericardia were non-adherent. The opposite surfaces of the membranes were rough, and like tripe: and the *to and fro* sound never ceased in these persons. Such cases are always soon fatal. But, in the others, did adhesion take place? I make no doubt of it. Within the last twelve months I have had demonstrative proof of it in two instances. One of these occurred in a hospital patient, whose case has been published in the *Medical Gazette*.¹ He was a painter, nineteen years old; and he became my patient last May, with acute rheumatism and carditis. From the 26th of May to the 13th of June, a *to and fro* sound was distinctly audible, as well as a bellows-sound which had preceded it. After that date, the bellows-sound continued, but the rubbing sound was no longer to be heard. The patient improved; and was about to be discharged from the hospital: when, on the 29th of June, sixteen days after the sound of friction had finally ceased, he suddenly dropped down dead in the garden of the hospital.

Here I had concluded that the pericardium was adherent; though I had not expected to have so soon the opportunity of verifying my opinion. And accordingly, except over a small portion of the posterior part of the right ventricle, the union between the heart and its investing bag was complete at all points. The agglutination was evidently the work of recent disease. The medium of adhesion was of considerable thickness; and consisted of coagulable lymph, and coagulated half-organized blood. The pericardium was stripped off, as I have seen a poulterer skin a rabbit, and with about the same ease. This was a very interesting case to me, for it was the first in which I had had the privilege of examining the heart after having witnessed the peculiar succession of phenomena that I have been describing.

But since that time, I have met with another such case in private practice. The particulars of it are sufficiently important to warrant my relating them.²

In the month of October, 1836, I was taken by Dr. Sweatman to see a patient of his; whom I found sitting up in bed, pale, with sharp features, breathing shortly and laboriously. His legs were anasarcaous, and his belly was tense and fluctuating.

I learned that he had been for years given up to intemperance in drinking, and to indolent and low habits. He told me, that the wind troubled him, shooting up through the whole of the left side of his chest. On further inquiry I found his meaning to be that he had much pain there. There was loud wheezing over the upper lobes of both lungs: both sides of the thorax were dull on percussion at their lower part; and on the right side no breathing whatever was audible below. These latter symptoms were indicative of dropsical effusion into the pleura also. The jugular veins were swollen and tortuous on both sides of the neck. On applying my ear to the præcordial region I at once heard a very loud and distinct *to and fro* sound. This was equally manifest when he held his breath. Dr. Sweatman, who was not so much accustomed as I have been to listen to the sounds of the heart in disease, recognised instantly the peculiar character of this sound. I ventured to express my

¹ Vol. xviii. p. 701.

² *Medical Gazette*, vol. xxi. p. 544.

certain conviction, that the patient was labouring under recent and acute pericarditis. I added, that he had also hydrothorax; and that, whatever chronic changes might have taken place in his heart previously to his present illness, dilatation of the right cavities constituted at least one of them.

He had been attacked by his present urgent symptoms three days before I saw him, viz., on the 8th of October. On that day, in all probability, the inflammation of the pericardium commenced.

This was his history. In the spring of the year, having, from indolence, kept the house for months before, he crossed from the Isle of Man to Liverpool, and was sick, and suffered a good deal during the passage. After landing, he had a mile or more to walk. His companions outstripped him, but were called back to him, and found him very pale, breathing with difficulty, and unable for a time to proceed. He attributed all his subsequent complaints to that exertion: and he had been ailing, though not confined to his room, till the 8th of October. I did not venture, in his condition, to open a vein: but leeches were applied to the præcordia, and he took diuretics.

I did not see him again till the 15th of October, four days after my first visit. The rubbing sound was still there, though less loud, less harsh, and less extensive. The leeches had given him much relief. His pulse was very small. A blister was now applied. On the 20th, I saw him for the third and last time alive. The rubbing sound was quite gone. Of this Dr. Sweatman also satisfied himself. There was a dull systolic bellows-sound in its place. The pulse was scarcely perceptible: but he continued apparently improving, making a vast quantity of urine, while the dropsical swellings fast diminished, till the 31st; when, after talking a short time oddly, and in a peculiarly loud voice, he sat up to take some medicine; and having done so, reclined his head against the nurse, and expired. Mr. Shaw assisted in the subsequent examination of the body. I omit giving an account of the condition of the lungs and pleura, which was what we had anticipated; and confine myself to the state of the heart. That organ was large. The pericardium was adherent universally by means of lymph, mottled with blood: and it was easily separated, so that the adhesion must have been recent; as was proved indeed by the symptoms. The right cavities of the heart were very large; and the aorta was diseased.

The existence of the *to and fro* sound in these cases no one can doubt who has once listened for it when present: and the facts respecting it, which have been established beyond the reach of controversy, are these:—1st, That when it occurs *de novo*, it always and surely denotes acute inflammation of the pericardium. I say *de novo*, because (as I stated in the last lecture) a *bellows-sound* may accompany *each* movement of the heart, in consequence of *internal* disease of some standing: and this double, sawing, bellows-sound might possibly be confounded with the alternating noises produced by the attrition of the opposite surfaces of the inflamed pericardium. If any doubt should ever arise in your minds respecting the meaning of sounds which are sometimes thus similar in character, while they result from very different conditions, it may aid your diagnosis to remember, that endocardial murmurs are often plainly traceable along the course of the great arteries of the thorax; and that the exocardial rubbing sound, though it may be audible over nearly the whole of the chest, is not heard with any special distinctness in the arteries. 2ndly, The *to and fro*, or rubbing sound, is never of long duration, but soon terminates in one of two ways. Either the patient dies in a short time, the sound continuing to the last; and then the pericardium is found coated with rough lymph, but throughout the far greater part of its extent, or altogether, *unadherent*: or the sound ceases, never to return, while the condition of the patient improves; or he even seems to himself and to others, to recover his perfect health. In these cases, the sound ceases from a physical impossibility of its continuance, viz., from adhesion of the pericardium over the whole, or the greater part, of the surface of the heart. And in this category of apparent but unreal recoveries, I cannot doubt that many of Bouillaud's cases of "*pericarditis terminating in health*" ought to be included.

It follows as a necessary consequence from these facts that acute and general pericarditis, so far advanced as to occasion the pathognomonic rubbing sound, does not admit of a perfect cure: and that its best event is the adhesion of the membrane, and the obliteration of its cavity.

And even then, I say, the change is not final. An adhering pericardium does so embarrass the movements of the heart as to cause at length, sometimes rapidly, sometimes slowly, further changes, affecting the muscle thus held in its morbid embrace. It has indeed been shown, by Dr. Barlow and Dr. Chevers, that this restricting cover has no direct tendency (as had commonly been supposed, by myself among others) to produce hypertrophy. On the contrary, when the adhesion takes place during youth, as in connexion with acute rheumatism it is very apt to do, it seems to prevent the further growth of the heart, and virtually leads to atrophy of that organ, or a disproportionate smallness of its cavities, vessels, and general size. Dr. Barlow points out a mode in which simple adhesion of the pericardium may indirectly bring about hypertrophy of the right ventricle, through its influence upon the functions and development of the lungs. Instances are numerous, however, of considerable hypertrophy of the left chambers, and of the whole heart, co-existing with an adherent pericardium. But in these cases the hypertrophy is really due to the impediments opposed to the free passage of the blood by valvular or other endocardial disease. It is held, too, and I believe justly, though I am not so sure of this as of some of the other points I have been dwelling upon, that the inflammation which begins in the membrane sometimes dips into the muscular substance of the heart, weakens its elasticity and cohesion, and so leads ultimately to dilatation of its cavities.

I need not occupy much of your time in speaking of the other morbid sound that is audible in these cases, the bellows-sound: which sometimes may be heard before the *to* and *fro* sound commences; which I have frequently heard *through* the *to* and *fro* sound; and which often remains after the superficial rubbing sound has ceased. This depends, no doubt, upon those alterations in the lining membrane, and especially in the valvular apparatus of the heart, which take place from inflammation, at the same time with the alterations of the pericardium. And when it is met with in such cases, it may be set down as indisputable evidence of the existence of endocarditis.

I am anxious that you should take an interest in the disease of which I have been speaking at so much length this evening, and that you should keep it in mind in your future practice: for I am certain that it is a fertile, but often unsuspected source of chronic disease of the muscular substance of the heart, and of its consequences; asthma, dropsy, sudden death. The number of patients that come into the hospitals of London affected with acute rheumatism is annually very large: and I am sure that I do not exaggerate when I say that more than one-half of them have the heart or its membranes implicated. The cardiac affection may easily be overlooked both by the patient and the physician. The recovery may appear to be perfect. But after some time, palpitation begins to be occasionally felt; and, by degrees, other symptoms, marking disease of the heart, declare themselves: but their origin is unsuspected or forgotten. You will be surprised, if you search back into the past history of all the patients who apply to you having disease of the heart, especially among the lower classes of society,—you will be surprised to find how many of them will acknowledge that at some time or other of their lives they have been laid up with rheumatic fever.

It is no part of my purpose to treat at present of that specific disease of the joints to which we give the name of acute rheumatism: but I may as well complete what I have to say of carditis as it occurs in connexion with that disease; and then I shall not need to repeat myself when I come at length to rheumatic fever.

In the first place, then, I would say a few words more respecting the nervous disturbance which is apt to supervene in such cases, and to mask the real disease, and to mislead the unwary practitioner. Patients labouring under rheumatic carditis very frequently become affected with delirium, or violent mania, or stupor and coma, or convulsions, or all of these in succession; and you might suppose that they were labouring under inflammation of the brain, or spinal cord; or of their membranes. Such cases are in fact spoken of as cases of *metastasis* to the brain. It may sometimes be so, nay, I know that it sometimes is so; but not often.¹ Again and again,

¹ The accuracy of this statement has been questioned. It was founded upon the single instance of a female patient of my own, who died in the Middlesex Hospital after symptoms of cerebral inflammation, supervening upon acute rheumatism. Unequivocal pus was found smeared over the hemispheres of her brain. In the 29th volume of the *Medical Gazette*. Dr. Fyfe of Newcastle has related the history of a very similar case; a third example is recorded by Dr. Fuller, as having occurred in St. George's Hospital under the care of Dr. Seymour.

when death has occurred, and the delirium had been extreme, no traces of disease have been discoverable within the skull, nor within the vertebral canal, while marks of violent and intense inflammation have been visible in the pericardium. It may be that the acute cardiac affection interferes somehow with that regulated supply of blood to the head, which is necessary for the due performance of the cerebral functions. It may be that a morbid quality of the blood itself disturbs them. It may be that the cerebral or spinal symptoms are purely reflex phenomena, of eccentric origin, and excited by the irritation of incident nerves, pertaining to the heart. Whatever the explanation, recollect the fact; and whenever, in acute rheumatism, you find your patient flighty and wandering, or more distinctly delirious, or affected with any form or degree of convulsion, examine carefully the condition of his *heart*.

As this is really a point of great importance, and as you will not find much information respecting it in books,¹ I shall take leave to quote, here, some part of a clinical lecture delivered by myself at the Middlesex Hospital, in the year 1835, and printed in the 16th volume of the *Medical Gazette*. My subsequent experience has been quite in conformity with what I then stated.

"The functions of the brain not unfrequently become disordered in rheumatic fever: and disordered in such a manner and degree as would lead, and has led, many to believe in the presence of active inflammation of that organ, or of its enveloping tissues. Yet this affection of the brain is not, I believe, inflammation, but some secondary affection of the circulation therein; resulting from disturbance at the central organ of the circulation; capable of producing a corresponding derangement in the cerebral functions. I can best explain what I mean by reciting a few examples.

"The first case of this kind that I ever saw or heard of, occurred in St. Bartholomew's Hospital, many years ago. I took notes of it at the time, and will read you the substance of them. Charlotte Rankin, aged 17, was admitted there on the 12th of August, 1824, under the care of Dr. Roberts, with acute rheumatism of the joints. Her illness had come on suddenly a week before, after unusual exposure to cold and wet. The pain and swelling had shifted much from joint to joint. She had been bled, on account of pain in her left side, two days before admission. On the 14th, she complained of much difficulty of breathing, and of pain when even slight pressure was made upon the chest. These symptoms were entirely removed by a blister. On the 16th, she was observed to be odd in her manner—peevish, querulous, restless, without sleep, and desirous of getting out of bed. Her pulse was then 100. On the 20th the pulse had risen to 120; it was quite regular. She said she felt no pain, except the soreness occasioned by the blister. She slept very little. On the 21st, the pulse was 128. Some jactitation of the left arm was now observed, which, she said, had never happened before. No sleep. On the 22nd, about nine in the evening, she became furiously maniacal, and it was necessary to confine her by a strait-waistcoat. She continued in that state for upwards of four hours, and then died.

"Twelve hours afterwards the body was examined. The brain was found quite healthy: its vessels seemed, indeed, somewhat fuller of blood than is usual, but there was no effusion, nor any other vestige of inflammation.

"The pericardium was glued to the heart, in several places, by recent adhesions; and it was universally coated, where not adherent, by a layer of rough reticulated lymph, remarkably harsh to the touch.

"Now here the most prominent symptoms were such as we are accustomed to refer, with tolerable confidence, to inflammation of the membranes of the brain; whereas, in fact, the inflammation was strictly confined to the heart. If no examination of the body had been made, the case might have been quoted, with much show of reason, as a well-marked example of metastasis to the brain. It *was* so considered, before the brain was inspected.

"There had, indeed, been symptoms which indicated, and that not obscurely, the cardiac disease. At that time, however, I did not know how frequently carditis is

¹ This statement is no longer true. Dr. Burrows has fully considered it in his valuable essay, published in 1846, *On Disorders of the Cerebral Circulation, and on the Connexion between Affections of the Brain and Diseases of the Heart*. It is gratifying to me to find in Dr. Burrows' observations upon this interesting subject so striking an accordance with and confirmation of my own, which at that time had not fallen under his notice.

combined with acute rheumatism. Auscultation had not yet come much into fashion in this country; at any rate, I knew little or nothing of its use; and I had supposed (and it had been supposed by others who witnessed the case) that the chest symptoms resulted from rheumatism of the intercostal muscles.

"Another instance, in which the course of the symptoms was somewhat different, yet equally calculated to mislead, you have lately seen in this hospital.

"William Wilkins, a post-boy, 28 years old, was admitted on the 25th of last November.

"He complained of pain in most of the large joints, shifting from one joint to another. There was no visible redness or swelling, but he had much fever. The pain was greatest at night. He had profuse perspirations, during which the pain was not mitigated.

"He had been ill eight weeks; and at first his joints (according to the statement of his friends) were both swelled and red. He appeared to be recovering at one time, but relapsed. For three or four days previous to his admission he had coughed a little, and spoken of pain at the pit of his stomach. He lay more comfortably on the right than on the left side, but this was habitually the case. He had never had acute rheumatism before.

"He rambled a good deal during the night of the 26th, and on the 27th he began to refuse to take his medicine, appeared confused and stupid, and answered questions tardily and imperfectly. He was bathed in perspiration, which had the strong acid smell so common in cases of acute rheumatism.

"During the next ten days he remained in a singular state of quiet delirium, rejecting medicine and food, saying he had had enough; getting out of bed, especially in the night, and declaring that he was going home. When questions were put to him, his lips moved, and his limbs began to stir and fidget, as though he were about to answer; yet he said nothing. He understood what was said to him, and put out his tongue when desired so to do; imperfectly, however, and with slowness and apparent difficulty. His bowels were costive, and he passed his stools, when purgatives acted, in the bed. His pulse was small and frequent; and when his wrist was taken hold of that the artery might be felt, he always resisted, and forcibly contracted his arm.

"Then for three or four days he appeared to improve; his countenance became more clear and lively; but he still showed the same restlessness, and maintained the same dogged silence when spoken to, and obstinately refused to swallow medicine. He was somewhat cunning, too, for he would take pills into his mouth, and then, when he thought he was not observed, chew and spit them out again.

"His pulse became at last very frequent, and his strength diminished rapidly. He died on the 18th of December, and the body was examined on the following day.

"The cerebral veins were gorged with dark blood, and there was a considerable quantity of serous fluid beneath the arachnoid, and in the lateral ventricles.

"The pericardium was free from disease; but upon the mitral valve, near its edge, there was a perfect row of small, slender, bead-like warts.

"A few weeks ago I was consulted in a case of a similar nature, which occurred in the practice of a gentleman who was formerly a pupil here, and who was fully aware both of the frequent occurrence of carditis in acute rheumatism, and of the anomalous symptoms with which it is sometimes attended. The patient was a young man, 24 years of age.

"On the 22nd of December he was seized with pain and swelling of several of the larger joints, and with fever. The attack was ascribed to exposure to cold the day before; he had previously enjoyed perfect health. The inflammation shifted rapidly from one joint to another. He was confined to bed for six days: then feeling better, he got up, changed his room, and presently underwent a relapse. Mr. Elwin tells me that, after that time, he was never comfortable about this patient; his countenance was pale, and his aspect unpromising; his pulse frequent; and more than once he complained of slight pain in the epigastrium, increased by a full inspiration. This was removed by a mustard-poultice. No morbid sound was detected upon a careful examination of the præcordial region by the ear. He remained low-spirited, but slowly mending, till the 3rd of January, when in the evening, without any notice or obvious cause, he began to be restless and delirious.

"On visiting him the next morning, Mr. Elwin found him with an anxious countenance; a frequent and irregular pulse, which occasionally intermitted; his mind wandering; the action of his heart strong, and attended towards the sternum with a loud bellows-sound. The next day his breathing was difficult, and 'catching;' the pulse 120, hard and wiry. At that time I had the opportunity of seeing him. He was lying in a sort of stupor, yet not unconscious, for he put out his tongue at my request, and answered pertinently one or two questions, after they had been frequently repeated. He had the air of a person obstinately determined to say as little as possible. He became more distinctly delirious towards evening; and the next day his pulse and breathing were both so frequent (148, and 78, in the minute, respectively) that he was thought to be dying. A distinct bellows-sound was audible near the left mamma. This state continued, with slight fluctuations, till the 8th, when his condition appeared somewhat more hopeful. He was calm, had no dyspnoea, and conversed more readily, — saying sometimes that he felt as if he were 'dead;' sometimes that he was 'burnt up.' He complained, for the first time, of pain in the right temple; his gums were slightly under the influence of mercury; his pulse scarcely exceeded 100; the bellows-sound was very manifest.

"On the 9th he again became, first restless, and then violently and wildly delirious, screaming out, refusing to take medicine, or to open his mouth when it was offered; yet he evidently knew what was said to him. During the night general convulsions came on in occasional spasms, of a tetanic character: in the intervals between them he lay in a state of coma. He survived in this condition till the 12th.

"I was present at the inspection of the body ten hours after death. Some of his family insisted on being in the room with us; but we were able to make an accurate examination of the head, and of the heart.

"The veins of the brain seemed somewhat fuller of blood than is common. The arachnoid was slightly elevated by a clear serous fluid collected in the pia mater. There was but a small quantity of a similar fluid in the lateral ventricles. The lungs appeared quite healthy.

"There was no fluid in the pericardium. Its surface was everywhere exceedingly vascular, but it presented no appearance of lymph, except where it adhered to the posterior side of the heart, over a space of about two inches and a half in length, and upwards of an inch in breadth. The lymph which formed the medium of connexion was firm, but evidently of recent formation; and a very slight degree of force sufficed to separate the adhering membranes. The heart was rather small, and the left ventricle had a singular wrinkled appearance externally. Towards the edge of the mitral valve there was a profuse crop of little wart-like vegetations, of the size of millet-seeds, and numerous red lines converged towards them from the base of the valve. The aortic valves all presented curious festoons of similar excrescences, larger, however, and more prominent, than those upon the mitral valve.

"In the beginning of the year 1832, a girl nineteen years old, Frances Kirk by name, was a patient of mine in the hospital, with acute rheumatism of the joints, and carditis, manifested by many of the most usual symptoms — by pain in the situation of the heart, dyspnoea, great frequency of pulse, and a distinct bellows-sound. She lived two months from the commencement of the cardiac disease. During that period she was at times wildly delirious — at times stupid, taciturn, and almost idiotic — and at times quiet and rational. The brain in that case was found perfectly healthy, except slight serous effusion beneath the arachnoid. The pericardium was everywhere adherent to the heart. By some mismanagement the opportunity of inspecting the inner membrane of the heart was lost.

"In each of the three last-mentioned cases there was more or less serous fluid found in the meshes of the pia mater, and in the lateral ventricles. You may ask, perhaps, whether this effusion was not good evidence of previous inflammation there? whether it did not show that the metastasis, which I have spoken of as seldom happening, really did happen in these very cases?

"I apprehend not; and for the following reasons. — In one only of these cases was the amount of the serous accumulation at all considerable. There was no *other* trace of inflammatory action in any of them; no redness, nor pus, nor lymph; none of the *unequivocal products* of inflammation. What quantity of serous effusion beneath the arachnoid, or in the ventricles of the brain, is requisite to establish its morbid origin

— within what limits such effusion may be considered natural—whether it may not be ascribed wholly, or in part, to mechanical transudation after death; these are questions which have not yet been definitely settled among pathologists. For my own part, whenever I see the veins of the pia mater full of blood, I *expect* to find serum between that membrane and the arachnoid. How much of it may have been poured out before death, and how much afterwards, it would be difficult to estimate. In each of the cases before us there was evidence, not to be mistaken, of cardiac inflammation. Now that acute inflammation, fixing itself upon some portion of the heart, should embarrass its action, and modify the condition of the circulation through the cerebral blood-vessels, is not only conceivable, but highly probable. Any retardation of the venous circulation in the head—any engorgement or congestion of that system of vessels—would be likely, if we may reason from the analogy of other parts, to produce effusion. I have seen, in the brain of a criminal who had been hanged while in a state of perfect health, as much serum collected in the same parts as we found in the patients whose cases I have been relating. It is possible that, in them, the disorder of the sensorial functions depended upon simple disturbance of the cerebral circulation; it is possible that the same disorder depended upon the serous effusion; and it is possible, and (I think) probable, that it depended in part upon both these causes. It is very certain that similar symptoms have occurred in similar cases, when there was no appreciable effusion; and, apparently from mere derangement of the natural circulation of the blood in its vessels. On the other hand, we know that an equal, or a greater amount of effusion, has often been observed, when no such cerebral symptoms had manifested themselves. I conceive, therefore, that the symptoms referable to the brain, and the quantity of serum found effused there (whether these bear to each other the relation of cause and consequence, or not), are both to be regarded as secondary effects of the cardiac disease; that they denoted no inflammatory condition of the brain, or of its membranes, but were the common result of that inflammation of the heart, concerning the existence of which the inspection of the bodies left us no room to doubt.

“That this view of the matter is correct, is the more probable because (as I just now stated) the same symptoms have been known to accompany carditis, although no serous effusion was met with in the head. There was none in the case of the girl Rankin; none in a case related by Dr. Davis; none in a remarkable case detailed by Dr. Latham; none in a striking example of a similar kind which fell under the observation of that accurate and most accomplished pathologist, Andral. I shall take the liberty of citing these two instances.

“‘One of the children of Christ’s Hospital,’ says Dr. Latham, ‘had, in the opinion of all who saw him, the severest inflammation of the brain. The attack was sudden, with great heat and frequency of pulse. He had delirium and convulsions, and pointed to his forehead as the seat of his pain. In three days he died, and, upon dissection, not a vestige of disease was found within the cranium; but the heart was exclusively the seat of the disease, and no other part of the body discovered the slightest morbid appearance. The disease of the heart was not confined to its investing membrane. It was the most intense inflammation pervading the pericardium and the muscular substance.’

“Andral’s case, which is referred to by Dr. Latham, occurs in his *Clinique Médicale*.

“A woman, twenty-six years old, was brought to La Charité, in a state of delirium, and no account could be obtained of her previous condition. The delirium was remarkable for the obstinate taciturnity which attended it. When questioned, the patient turned a fixed gaze upon the person who spoke to her, but made no reply. Her face was pale; her pulse small and frequent. During the two following days the head was frequently drawn backwards, the trunk was shaken at intervals by convulsive movements, and she had subsultus tendinum; but she now spoke, and appeared to comprehend what was said to her, but talked incoherently. The pulse was very frequent, and intermitting. On the fourth day the delirium ceased; she complained of nothing but great debility. The muscles of the face were almost continually agitated by convulsive twitchings, and the arm from time to time presented a sort of tetanic stiffness. On the fifth day the delirium returned; the patient then fell into a state of coma, and died the next morning.

"Neither the brain, nor the spinal marrow, nor their membranes, presented any appreciable morbid appearances. The pericardium was lined with coagulable lymph, and its opposite surfaces were connected, in some places, by recent bands of adhesion. It contained also some ounces of a greenish flaky serum. No other trace of disease was discoverable.

"Now if you are not made aware beforehand of this strange course of the symptoms arising, sometimes, out of rheumatic carditis, you will be apt to overlook the cardiac affection, and to direct your remedial measures wide of the mark. In a second instance mentioned in Dr. Latham's essay, 'the whole force of the treatment was directed to the head, from a belief that the brain was inflamed. Upon dissection, the brain and its coverings were found in a perfectly healthy and natural state; and the pericardium, towards which during life there was no symptom to direct the slightest suspicion of disease, discovered the unequivocal marks of recent and acute inflammation.' Dr. Davis also, in reference to a case published by him so early as 1808, has the following remark:—'The restlessness in the case of Miss H. C. was also attended with delirium, a symptom not previously noticed as belonging to pericarditis by any writer whom I have consulted. It was so prominent a feature of the disease under which this young lady laboured, as to divert the attention of her medical attendants from its actual seat.'

"This occurrence, in the course of rheumatic carditis, of cerebral symptoms calculated to perplex and obscure the true nature of the disease, is probably not so rare as has been supposed. In less than three years three instances of it have fallen under my own notice; and I have been informed by a medical man residing in the neighbourhood, that a friend of his, who has a very large general practice among the middle and lower classes, attended within the last year or two not less than twenty cases of acute rheumatism, in which a metastasis, or an extension, of the inflammation appeared to take place to the brain.

"In all the detailed cases of this kind that I have met with, and in those which I have myself watched, there were certain general points of similarity which you will do well to bear in mind. In all of them the pulse was extremely rapid; the delirium, though violent and active at intervals, was characterized for the most part by a singular, and, as it seemed, perverse taciturnity; even when the patient was evidently able to speak, and understood the questions that were put to him, he maintained a sullen silence. In most of these patients, also, not long before the fatal event, a brief interval of amendment took place, and encouraged some hope of recovery. In many of them various convulsive movements were observed; and in two of the cases the head symptoms, and probably the heart disease also, supervened after a *relapse* of the rheumatism of the joints."

It is a curious and instructive circumstance that rheumatic carditis is sometimes the first step in the whole disease. The cardiac symptoms do sometimes, I mean precede those of the joints; even by two or three days. For example.—A lad was brought to the hospital with acute articular rheumatism, and with unequivocal symptoms, which I need not detail, of carditis. He gave the following distinct history of his illness:—He had been on a visit into the country several days before, and there, after having felt poorly for nearly a week, with a sensation of "sinking within him," he ate largely of oysters, and drank more porter than he was accustomed to. On the same day he was seized with pain in the left side of the chest, and violent beating of the heart. The attack was probably a severe one, for he applied to a medical man, who immediately bled him. In the course of the ensuing night he began, for the first time in his life, to feel some stiffness beneath and about his knees, but he was able to walk about the next day. On the evening of the second day the joints became so painful and swollen, that he could not leave his bed, and the pain of the side and the palpitation diminished. This boy has several times since returned to the hospital with acute rheumatism, and on each occasion presented manifest indications of some permanent affection of the heart,—slight, probably, in amount, but aggravated upon every return of inflammation of the joints.

I have met with one or two other instances in which the cardiac disease appeared to have preceded the arthritic; but none so well made out as that which I have just related. In the *Edinburgh Medical and Surgical Journal* for 1816, Dr. Duncan

gives a case of "inflammation of the heart" which *began* with symptoms of *pectoral inflammation*, succeeded *the next day* by rheumatic affections of the joints. Dr. Fuller, in his excellent book on Rheumatism, states that three instances of this sort have fallen under his own observation: and he refers to several others, recorded by various writers.

One law respecting the connexion between the cardiac and the arthritic symptoms may be stated with confidence, namely, that the *younger* the patient is who suffers acute rheumatism (and I have seen it so early as the third or fourth year), the more likely will he be to have rheumatic carditis. The chance of the combination appears to diminish, after puberty, as life advances. I have known only three persons pass through acute rheumatism with an untouched heart prior to the age of puberty; and in two of these I am by no means certain that the articular disease was genuine rheumatism. In each of the two, the large joints became painful, and swelled, for a day or two only, towards the close of scarlet fever:—a circumstance not, I believe unusual. I was dreadfully apprehensive of carditis, but it did not occur.

I have observed also, that when a patient has come under my care who has had repeated attacks of acute rheumatism, in him I have generally found reason to believe that some organic affection of the heart was present. Probably the disposition to such repetitions of the disease, so remarkable in some individuals, may be kept up by the cardiac complication.

With respect to the period of the actual attack, and the circumstances under which the extension of the disease to the heart occurs, no fixed law has been observed. Sometimes the cardiac affection declares itself as the inflammation of the joints declines. Quite as often, however, they proceed together, and are aggravated or mitigated simultaneously. On this point my own experience nearly agrees with that of Dr. Latham, who says:—"It (the cardiac affection) is incident to all the degrees and all the stages, and all the forms (?) of acute rheumatism. It is not more to be looked for when the disease is severe than when it is mild; more at its beginning than during its progress and decline; more when it is shifting and inconstant in its seat, than when it is fixed and abiding."

There are some other symptoms that I must not omit to mention as occurring in some cases of pericarditis. In one of the fatal instances which fell under my own notice, there was a very strong purring tremor felt by the hand placed upon the region of the heart. This is not a constant, nor even a frequent, symptom; but it has a certain degree of corroborative value when it does occur.

When the fluid products of the inflammation predominate, when there is much serum poured out, the symptoms, as well as the danger, will be different from those which are remarked when there is not so much serous liquid. If the pericardium be distended, percussion will furnish a dull sound over an unusually large space; much beyond the natural limits of the præcordial region: and you may often measure the amount of the effusion, and its daily increase or decrease, very accurately in this manner. But the general symptoms will vary also. The pulse will be feebler, and more disposed to falter, and to become irregular, in proportion as the liquid effusion is large; and at the same time the patient will frequently be fixed in one position, and unwilling or afraid to change it, lest that small exertion should further excite the action of his heart, and hurry his respiration. He will lie, perhaps, always upon one side; or he will remain immovable on his back, with his head elevated; or he will sit up continually, with his body leaning forwards; and he will not dare to alter his posture. But when the solid products of the inflammation predominate; when there is coagulable lymph, and but little serum; when the pericardium, instead of being distended, becomes attached to the heart; then the pulse will retain that force and regularity with which the disease commenced, the dull sound yielded to percussion will not transgress the præcordial limits, and the patient will not in general experience any absolute necessity of accommodating his body to one constrained position.

Of a merely adherent pericardium there are no diagnostic signs to which, so far as I know, we can trust, either auscultatory or general. None, I mean, presented by the body at the time. If we are accurately acquainted indeed with the history of the patient's disease, and if we know that, at any time, a *to* and *fro* sound existed, which *to* and *fro* sound soon ceased, and has never recurred; then our conclusion that the pericardium is adherent will scarcely be open to any source of fallacy.

When the opposite surfaces of the membrane have been once united, they never separate again; the adhesion remains for life. But the lymph interposed between them, if the inflammation be not renewed, becomes less and less thick; until at length, in some cases, a mere layer of firm, but thin, areolar tissue is left, through which the heart is visible.

But when inflammation has stiffened the valves of the heart, or studded them with little wart-like masses of fibrin, or rendered the lining membrane of its chamber thick and opaque, how far do these morbid states admit of perfect recovery? It is not so easy to say. I am not aware of any facts which would forbid altogether the hope that here, as in iritis, the re-absorption or removal of the lymph may be total, and the restoration of the parts complete. On the contrary, the *comparative* infrequency of wart-like excrescences in the slowly fatal cases of rheumatic carditis leads to the opinion that such deposits may disappear as readily and entirely from the valves of the heart as from the iris: and the success of remedial measures directed against recent hypertrophy, dependent apparently upon chronic or subacute inflammation of the endocardium, is corroborative of this opinion.

One perilous way in which these vegetations may sometimes be removed I briefly adverted to when speaking of apoplexy and palsy. Portions of them, large or minute, may separate from the subjacent membrane, or be detached and washed away by the current of the blood. And serious, and even quickly fatal consequences, may result from this accident (as it may be called) of endocardial disease. A fragment of fibrin, thus carried along in the circulating blood, may stick in some blood-vessel which is too small to permit its further progress. If one of the cerebral arteries happen to be thus suddenly plugged, that portion of the brain to which its branches are distributed is deprived of much, or all, of its nutrient blood; white softening ensues, and consequent palsy. Dr. Kirkes, to whose sagacity we are indebted for almost all that we know on this interesting subject, has detailed, in the 35th volume of the *Medico-Chirurgical Transactions*, some well marked examples of this disaster. Analogous evils may follow the blocking up of a considerable artery in other parts of the body. I will mention one instance that I have lately seen. A sickly girl in her fifteenth year was attacked with acute articular rheumatism about Christmas-time, in the year 1854. She was attended by Mr. William Squire, who carefully and constantly examined the condition of her heart. No evidence of its implication occurred for the first ten days of the disease. Then began a mitral murmur, and with it signs of pulmonary congestion. Five or six days after this, an aortic murmur also became audible; and nearly at the same time her right leg, from the hip downwards, turned suddenly cold, without any loss of power, or of sensibility; and it was found that all pulsation had ceased in the right external iliac artery and its branches. She spoke of slight pain in the inguinal region on that side. Presently vesicles, containing a dark fluid, formed upon the great toe, and upon the third toe, of the right foot. Fear naturally arose that the whole leg would mortify; and the girl's life was despaired of. By great care, however, by lapping the limb in wool, and by supporting her strength, she was carried safely through that cold winter. By degrees the temperature of the leg and thigh were nearly restored; but no pulsation could be felt in the larger arteries when I saw her with Mr. Squire in October, 1855. A rough mitral bruit was then very plainly to be heard both in front and at the back of the chest; and also a less distinct aortic bellows-sound. The girl died in the following May. Unfortunately permission to examine the limb could not be obtained: but I cannot doubt that its main artery had been sealed up by solid matter, derived from one of the cardiac valves.

Minuter portions of fibrin thus detached from the lining membrane of the left side of the heart, may pass unchecked through the arteries, yet be entangled and stopped in the capillary vessels, and lead to permanent changes in the affected textures. It is in this way that many of the yellowish or buff-coloured masses of fibrin, which are familiar to morbid anatomists as being of frequent occurrence in the spleen, and in the kidneys, are supposed by Dr. Kirkes to originate.

After a similar manner branches of the pulmonary artery, or parts of the pulmonary capillary system, may be obstructed by portions of fibrin proceeding from the *right* chambers of the heart. The primary effects of these dislodgements of fibrin from the interior of the heart are mechanical: but it is very conceivable that the whole mass

of the blood may in certain cases be contaminated by the admixture of some of the fluid products of endocardial inflammation.

Since I lectured upon this subject last year, two examples of rheumatic carditis have occurred among my hospital patients, differing in some remarkable points from any that I had ever seen. A brief description of them will complete my personal experience of this terrible disease.

The histories of the two cases are curiously similar. The patients were young women; their ages respectively twenty-one and twenty-two. They were admitted during the same week: one a day after the other. Both were suffering under a first attack of rheumatic fever: both had also acute pleurisy, with effusion into the chest; and both died; one of them three weeks, the other a month, after her admission. In both cases there were symptoms referable to the heart: pain, and unnatural sounds; but in neither case was there any friction-sound; nor were any traces of pericarditis discovered after death. But the inflammation had fallen, partially, upon the aortic valves; whence it had extended (so I imagine) to the muscular substance. I show you the morbid appearances represented in these drawings, made by Mr. Lonsdale at the time: and, better still, I show you the parts themselves, which are preserved in our museum.

The whole of one cusp of the aortic valves was, in each case, a mass of ragged ulceration; and the adjacent portions of the two other cusps were, in a slighter degree, implicated in the mischief. What remained of the tattered valve was covered with rough irregular shreds of lymph, or vegetations. In one of the cases, the ulcerating process had penetrated through the valve, and into the muscular substance beyond, and had eaten a hole completely through the septum. A portion of lymph protruded just below the valves of the pulmonary artery through the channel of communication thus formed between the left and right sides of the heart. In the other case, an abscess as large as a hazel-nut was found in the muscular substance of the septum, immediately opposite the disorganized valve.

Till I met with these cases I was not aware that this destructive incrustation of the cardiac valves with wart-like excrescences was ever the result of *acute* inflammation. Chronic changes of that kind are not uncommon. Suppuration in the heart is very rare. With such mischief in rapid progress within the heart, it is easy to see how the blood may be polluted, and charged with a new poison in its very fountain. In these two instances, the cardiac affection was complicated with acute pleurisy. I should have mentioned before, that the pleura very often participates in the inflammation when pericarditis occurs. You will not wonder at this if you consider the close vicinity, and the similarity in texture, of these two serous membranes. Nor will you be surprised when I add, that the pleurisy is often associated with pneumonia also. Upon these accessory disorders the immediate danger of the case not unfrequently hinges.

With respect to the comparative frequency of these various complications of acute rheumatism, I may state briefly the result of Dr. Latham's computed experience, which is in general accordance with my own. Of those who suffer acute articular rheumatism, not less than two-thirds suffer also some form of cardiac inflammation. Of these forms, endocarditis is the least formidable, and much the most common: occurring nine times as frequently as pericarditis. Again, it is with the rarer, and at the same time the most perilous form of cardiac inflammation — with pericarditis, or with pericarditis and endocarditis combined—that pulmonary inflammation (including bronchitis, pleuritis, and pneumonia) is most apt to be associated.

I must defer what I have to say respecting the treatment of acute pericarditis and endocarditis, to the next lecture.

LECTURE LXII.

Treatment of acute Pericarditis, and Endocarditis: blood-letting; mercury; blisters. Chronic and partial inflammation of the Pericardium. Disease of the Aorta. Thoracic Aneurisms; their various situations, and symptoms: plan of treatment.

I TRUST that I made distinctly apparent in the last lecture, the great danger which belongs to every case of acute inflammation of the pericardium. First, there is the danger of *speedy death*. If the inflammation go to the extent of effusion, and the collection of serous fluid be large, and the pericardium be distended by it, the action of the heart is so much oppressed by the liquid surrounding it, that it falters and flutters, and at length stops, and goes on no more. Secondly, there is the danger that (the pericardium having become adherent) other structural changes may soon, or slowly, develop themselves; and first render life burdensome and full of suffering; and then consign the patient to an earlier grave than might else have awaited him. Again, if the endocardium alone be affected, there is the danger of such permanent valvular damage as may obstruct the onward current of the blood, or destroy the natural and necessary bar to its reflux: and therefore the danger of gradual hypertrophy and dilatation, with all their distressing consequences. There is also the further risk, that the seeds of disease may be conveyed to other organs of the body, from the interior of the heart, with the circulating blood. When both the lining and the investing membrane of the heart are involved in the morbid process, the hazard is obviously doubled.

Now what can we do to prevent, or to diminish, these evils? I once thought that if we caught the inflammation at its very commencement, we might calculate upon a perfect cure, by first bleeding the patient freely, and by, secondly, putting him as speedily as possible under the specific influence of mercury. I am sorry to be obliged to say, that the more I see of this formidable malady, the more reason I find for fearing that it is seldom within the possibility of thorough repair. Bring the inflammation to a stop, you perhaps may; or nature will do it for you; and you may greatly assist the natural powers in effecting this. But that alone can be called a cure, which either leaves the structure of the part affected in its original integrity; or, at any rate, leaves no spring or source of further changes for the worse: and such complete recovery as this I seldom dare to hope for, in cases of acute and general carditis.

There can be no use in deceiving ourselves in this matter; but we may very easily deceive ourselves. In a large proportion of cases, whether they be treated well, or ill, or not treated at all, the patients will *seem* to recover. But I say that the recovery is so far unreal, that it involves the germ of future destruction. If any of you have read Bouillaud's heavy, yet instructive, work on diseases of the heart, you will know that he boasts of the success of his treatment in acute pericarditis. He declares that by the bold use of the lancet he *extinguishes* the inflammation; jugulates (as he calls it) or slaughters the disease at its birth; and restores the patient to the full condition of health, or to the state in which he was before the disease came on. You must hereafter judge of this question for yourselves; but it is my duty to caution you against crediting these statements. Not that I would insinuate a doubt of M. Bouillaud's veracity; but I believe that he has been deceived by false recoveries; and I would not have you beguiled, by his representations, into the indiscriminate adoption of that "enlightened hardness" which he endeavours to inculcate.

But if we look closely at his statements, we do not find, after all, any such wonderful success. Of 18 patients, 6 died: a very large proportion, 1, viz. in 3. To be sure, with some ingenuity he makes the proportion to be 1 in 7. For three of the fatal cases occurred, he says, before he took to his heroic plan of blood-letting; and excluding these 3, he has 15 cases, and only 3 deaths; or 1 in 5. But one of these three proved fatal from the supervention of tetanus; therefore setting that also aside, there will be 14 cases of the disease and two deaths. Now, I have not, hitherto, been able to look through my case-books in reference to this point, but I am quite

certain that the mortality in the Middlesex Hospital has been nothing like so great as that—the *immediate* mortality, of course, I mean—either among my patients, or among those of my colleagues; and I know that, until within the last twelve months, Dr. Latham had not lost a single case of rheumatic pericarditis in the course of the first attack of that disease for several preceding years.

But what I most doubt about, is the *true* recovery of Bouillaud's surviving patients. I say such patients do *apparently* get well. In some of them, indeed, a bellows-sound remains, sufficiently indicative of the damage that the organ has sustained: and I have already told you that *any* amount of change, however small, which alters the healthy proportion between the cavities and their outlets, or which interferes with the natural play of the heart, is a seed from which further changes will at length be found to grow. But patients will get so far well that you can detect nothing wrong about them. Follow them, however, in their subsequent lives; and you will learn that many of them very soon begin to find that they are incapable of doing or enduring all that they could do or endure before their illness: and if this do not soon happen, it happens at last. The disease of the heart (if the patient be not cut off by some other malady) becomes at length obvious: and when he dies, the source of the ultimate changes is commonly to be detected. There is adhesion of the pericardium; or there is disease of the valves; of which no other account can be given than that these had continued to exist since the primary symptoms of carditis ceased; and had caused all the rest—the hypertrophy, to wit, the dilatation, or the wasting.

The remarks that I have now been making bear upon the question, to what amount blood-letting should be carried in acute pericarditis. If the general symptoms teach you that it exists, and yet no sound of attrition is heard, you may, in that case, if in any, hope to arrest the inflammation, and to achieve a complete cure, by the early abstraction of blood. But if the *to and fro* sound have been audible, I do not think the consequences of the inflammation can be so abolished. I believe that the best event which can then happen is adhesion. We must, however, in many cases, take blood either by the lancet from the arm, or by leeches from the præcordial region; yet not in the lavish manner recommended by Bouillaud. I know that his treatment has been fairly tried in this country, and has failed. And I think (but this I only offer as an opinion) that there is a peculiar risk in frequently bleeding to syncope in this affection. There is, almost always, endocarditis (in the rheumatic cases at least) coincident with the pericarditis: and there is a readiness or tendency towards a deposit of the fibrin of the blood, in the shape of minute vegetations, upon the inflamed valves: and it is probable that this tendency may be favoured by a retarded movement of the blood over them; and still more so by its temporary stagnation. In the experiments on the ass, referred to before, the circulation (kept up by artificial breathing) became languid and sluggish, and vegetations were deposited upon those valves which had been irritated by the wire. Hence there is, I think, a danger in bleeding to such an extent in these cases, as to bring the heart's action to a pause in *deliquium*. Bleed, therefore (if you deem it requisite to bleed at all), till some effect upon the pulse has been accomplished, and then stop: and renew the venæsection, or refrain from it, according to circumstances. But you may freely cup the præcordial region, or cover it repeatedly with leeches: and to this mode of abstracting blood, from the neighbourhood of the inflamed part, my own experience would assign the preference. In fact, I seldom open a vein in such cases.

At the same time, you will endeavour to get the gums tender with mercury. And it is most unfortunate that, in this disease, the system frequently resists, with great obstinacy, the influence of that mineral. Sometimes, do what you will, you fail to attain your object. A knowledge of this fact may render you less scrupulous than you would otherwise be in the use of the remedy. Not only should calomel be given in frequently repeated doses, guarded, if need be, by opium, but mercurial inunction should also be had recourse to, in these perilous cases, from the first. When the gums do begin to rise, there is always, according to my experience, a manifest subsidence of the distress, and mitigation of the symptoms: less pain, less palpitation, less dyspnoea. The mouth should be *kept* sore for some time together: for supposing the membranes to adhere, it does not follow that the inflammatory process should thereupon cease.

When you learn, from symptoms and signs already described, that the pericardium

is distended by liquid effusion, I would advise you to lay a large blister over the præcordia. The diminution, or complete disappearance of the liquid, under this treatment, is often rapid and striking.

Even when all the symptoms have departed, previous disappointments have taught me not to be sanguine as to the permanency of the recovery. I believe that months, and years even, may elapse before the secondary effects of the mischief left behind by the inflammation begin to be palpable. But in many instances they show themselves very early. Others have noticed all this; especially Dr. Latham, who truly remarks, that "in acute pericarditis there is no medium between complete cure and certain death." He deemed, at the time when the lectures from which I quote were given, that the early and vigorous use of mercury might be equal to the complete cure. But in his more recent work, *On Diseases of the Heart*,—in which the whole of this subject is most instructively treated in detail, and which I cannot too strongly commend to your diligent study—I find, with a melancholy sort of satisfaction, that his final belief is in no respect different from my own. I have several times already expressed my conviction, that when the *to and fro* sound has manifested itself, that is, when the inflammation has gone so far as the effusion of coagulable lymph, if the patient do not die outright, he survives at the expense of an adherent pericardium; and he survives only for a time. *Hæret lateri lethalis arundo*. But I am also of opinion, that by the cautious employment of the lancet and of leeches, and by the early and unshrinking use of mercury, the mischief may be greatly limited, and the consecutive changes staved off to a distant period. The final catastrophe arrives much more slowly, and in a somewhat different way, when there is mere adhesion of the pericardium, than when that condition is complicated with permanent disease of the valves within the heart, or of the lining membrane of the ventricles. The complete cure of pericarditis is less within the scope of well-directed remedies, than the complete cure of endocarditis. On the other hand, abiding changes of the endocardium are more surely and more rapidly fatal, by impeding the current of the blood, and so inducing hypertrophy and dilatation, than abiding adhesion of the pericardium. Dr. Munk's observations suggest the belief that in some of those cases in which the secondary effects of rheumatic carditis become early visible, the endocardial inflammation has insidiously lingered on, after the articular disease had vanished. What seems but a slow convalescence, is really incipient disorganization of the heart. The evidence of such persisting endocarditis must therefore be carefully and continually looked for; and its appropriate remedy, which is mercury, must be as diligently plied and adjusted. The inflammation remaining unchecked, simple hypertrophy will probably ensue when the membrane covering the walls of the ventricle is alone affected—hypertrophy with dilatation when the valves are implicated. I need scarcely say that other, minor expedients, are also to be put in force; strict abstinence I mean, perfect quiet, and an occasional purgative to clear out the alimentary canal.

When the pericardium has once become adherent, if (as is very likely) the patient again suffers acute rheumatism, he may again have heart symptoms. Not of course, the *to and fro* sound; but pain, palpitation, and dyspnoea. Now it is of some practical importance to be aware that this renewal of morbid action does not require that active treatment which the primary inflammation demanded. The effect of such renewal will be to augment the existing mischief; but the morbid process is much less vigorous, and much more easily subdued. It will generally yield to the repeated application of a few leeches, or of blisters, over the situation of the heart, and to the moderate exhibition of mercury.

Although acute inflammation in this, as in other serous membranes, shows generally a strong disposition to spread all over the affected surface; yet does the pericardium seem readily susceptible of slight and partial inflammation. You will very frequently indeed see, upon laying the bag open, a white spot, as big as the finger-nail, upon the surface of the heart. I have examined these spots very often; and I believe they almost always consist of a thin flake of lymph lying sometimes beneath, but oftener upon, the membrane. They may, in fact, be peeled off sometimes, and the subjacent membrane be left smooth and sound. On one occasion, I met with a long riband of lymph passing from the centre of one of these white spots, to connect itself with the loose bag of the pericardium. I conclude, therefore, that these spots are really the result of a very limited inflammatory process: but under what condi-

tions they arise, or whether during their formation they furnish any symptoms, I do not know.

Such is the view which I had long taken, and taught, of these white spots upon the surface of the heart; and I am glad to have it confirmed by the observation of Mr. Paget, who has adduced (in the twenty-third volume of the *Medico-Chirurgical Transactions*) conclusive evidence, both of their frequency, and of their inflammatory origin.

You may ask me whether inflammation of the pericardium, even when it is slight and partial,—such as might account for these spots—can ever take place without giving rise to a friction sound. It is highly probable that it cannot. Yet, as such inflammation is attended with no febrile disturbance or general distress, and probably with no severe or abiding pain, or no pain at all, it is not brought under the scrutiny of the physician, and may be scarcely noticed by the patient himself. So that the friction sounds are not heard, because they are not hearkened for. In this respect the formation of the white spots on the heart has some analogy with the formation of those pulmonary adhesions, which are such common results of dry and partial pleurisies.

I have spoken of acute pericarditis as it presents itself in frequent association with articular rheumatism; and I have shown you how fearfully serious a character is imparted by that complication to a disorder which, however painful, is otherwise devoid of danger. The same complication arises sometimes during the progress of the febrile exanthemata, and especially of scarlet fever. In both cases there is a blood-poison at work: in both cases the course, the clinical history, the gravity, and the appropriate treatment of the supervening disease, are essentially the same. And the patients are of the same class, being mostly young, or not old, and of previously sound health.

But inflammation of the pericardium is far from being uncommon under very different circumstances; in persons advanced in life, and with constitutions broken by previous disease. We trace it, often unexpectedly, in its effects—in the presence of lymph recently effused, and smeared over the surfaces of the membrane—after death by various chronic maladies, and above all, after death from that renal malady which has given to the name of Bright an immortality of reputation. In that disease also the blood may well be said to contain a poison. The frequency of this insidious form of pericarditis at the close of other diseases was fairly brought to light by the laborious and accurate researches of the late Dr. John Taylor. Its comparative lack of interest has been well weighed and set forth by Dr. Ormerod. The importance of the pericardial inflammation in these cases is practically but little. Dr. Ormerod truly observes of it, that it falls more within the province of the morbid anatomist than of the physician. Its symptoms are so slightly and uncertainly marked, that the fact of the inflammation is often recognised only in the corpse. It accompanies, rather than causes, death. It does not destroy life, but takes place because the patient is already dying. Other serous surfaces are apt to suffer inflammation under precisely similar circumstances. This kind of pericarditis scarcely submits itself to treatment. It is proper that you should be aware of its frequency, and of its nature: but it requires no further consideration in these lectures.

So much then for the heart itself, and its membranes. There still remain to be considered the morbid conditions of the great vessels that spring from it, and lie in the thorax, and especially of the aorta; those morbid conditions, I mean, which declare themselves by symptoms, and which become the object of medical treatment.

The aorta is very frequently indeed found diseased; and its disease, as I have already explained, is a common cause of organic changes in the left ventricle of the heart. You will find that its interior surface, instead of being smooth, and of a uniform yellowish white colour, is rendered very uneven by a great number of yellow opaque projections, of cartilaginous consistence, lying immediately beneath the membrane. And in a more advanced stage of the same diseased condition, you may perceive that some of these projecting little masses consist of irregular scales of bone, having sharp edges; and sometimes these plates of ossific matter are quite bare; the inner membrane is gone, and the exposed bone is washed by the current of blood.

Now the necessary effect of these changes is to diminish and destroy the natural elasticity of the vessel; and as there is a perpetually recurring strain upon it, by the blood sent out from the heart, the vessel dilates, becomes larger than it should be. This, if you please, you may call aneurism; but a simpler name is dilatation. In other cases, the enlargement is not general, but partial. A pouch is formed on one side of the artery, and this pouch may be very small or very large. It appears to result from the giving way, the rupture in short, or the ulceration, of the inner and middle coats of the artery, and then the blood, passing through the broken part, presses against the cellular coat of the vessel, and distends it into a sort of bag. There have been curious discussions as to what should be called true aneurism, and what should be called false aneurism; discussions upon which I have neither time nor taste for entering. It is enough for all practical purposes to state, that the artery sometimes dilates only, sometimes throws out a pouch. I know that you have received, or will receive, from my colleague Mr. Arnott, all the information that is requisite concerning the modes in which aneurism may arise. These are matters of the highest interest in surgery, for surgery can cure an aneurism; an achievement which is but seldom within the skill of physick.

I have known three or four cases in which the diseased artery all at once cracked across—its inner and middle coats, I mean—and death very rapidly ensued. In one of these instances, the crack extended round a considerable part of the circumference of the aorta. It looked exactly like a clean cut made by a sharp knife. The blood, in this example, dissected its way (if I may so say) between the middle and external coats of the aorta, and got at last into the pericardium, and coagulated round the heart in a uniform layer: so that a *bag* of coagulated blood was enclosed in the bag of the pericardium. In another case, which I mentioned in a former lecture, the dissecting blood shut up the right carotid artery, and fatal hemiplegia from white softening of the brain was the result. Of course, nothing can be done for such cases as these.

Most commonly the aneurismal tumour goes on enlarging: and often it becomes lined, and sometimes it is nearly filled up, by layers of coagulated blood, which form in its interior. At length the tumour bursts, and the patient perishes.

Fig. 87.

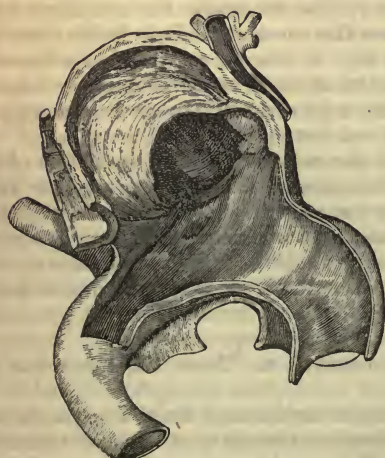


Fig. 88.

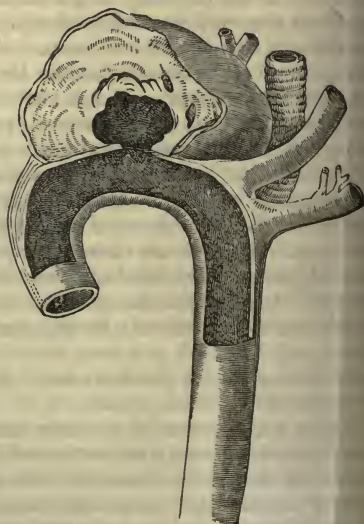


Fig. 87.—Section of the arch of an aorta, with an aneurism arising from its upper part. The cavity of the sac is nearly filled by laminated coagulum, the internal membrane of the artery is thickened. The sac presses against the trachea, the arteria innominata, and the right carotid and subclavian arteries.—St. Bartholomew's Museum, Series xiii. No. 11.

Fig. 88.—Further growth of aneurism prevented by coagulum becoming adherent to the artery around the opening of the sac.—From Hodgson.

Aneurisms of the thoracic aorta are met with chiefly in the earlier portions of that vessel, in its ascending part, and in its arch. There seem to be two reasons for this. One is, that the diseased state of the coats of the artery (to which the rupture and subsequent aneurismal pouch, or the dilatation, as the case may be, are owing) is more common, and more advanced generally in that part of the aorta; and another reason is, that the momentum of the blood, as it is forcibly propelled from the left ventricle, is sustained chiefly by the same part.

Mere disease or dilatation of the commencing aorta affords, as I formerly endeavoured to explain, a physical impediment to the due emptying of the left ventricle. It is a common cause, therefore, of hypertrophy and dilatation of that ventricle; and consequently, the *signs* of hypertrophy and dilatation of the left ventricle of the heart will at length result from disease and enlargement of the aorta near its mouth.

When aneurismal pouches form, as they often do, at the very entrance of the aorta, or in the coronary arteries, they often defy detection. I, at least, know of no sign of their existence upon which a physician can rely, or which can lead him even to suspect such a state of matters. But all at once the patient drops down dead: and upon searching for the cause of this sudden extinction of life, you find the pericardium distended with blood, and the source of that blood you find to be the ruptured aneurismal pouch, so near the root of the aorta, as to project *within the pericardium*. In the preparation which I hold in my hand, an unbroken aneurism actually bulges into the *right ventricle* of the heart.

When the aneurismal tumour occupies a portion of the ascending aorta a little more distant from the heart, or is formed at the arch itself, it sometimes attains a large size, and the evidence of its presence is derived from the effects its enlargement produces on the surrounding textures; and these effects are apt, for a while, to be obscure and equivocal, until an external pulsating swelling makes its appearance, or a sudden gush of arterial blood through the mouth discloses the true nature of the malady.

In these cases we *infer* the existence of aneurism sometimes from peculiar symptoms. Aneurism at the arch of the aorta may come to press upon the trachea, and impede the breathing; or by its effect upon the recurrent nerves, it may cause a very accurate mimicry of laryngitis; producing raucous voice and stridulous inspiration. The operation of tracheotomy, as I told you before, has more than once been performed, to relieve the supposed inflamed condition of the larynx, while the sole disease was aneurism at the arch of the aorta. Such mistakes are always discreditable; and the lesson they furnish should not be lost upon us. Whenever we find that a wheezing dyspnoea has gradually arisen, which no apparent affection of the air-passages satisfactorily accounts for, and the patient has a sense of pulsation within the thorax, we may suspect that an aneurism is at the bottom of these symptoms.

The effect of aneurismal enlargements of the artery in causing *absorption* of the neighbouring tissues, upon which the tumour presses, is very curious. You know that even the solid bone is removed, worn away as it were, before an advancing aneurism. Hence it not unfrequently happens that the trachea, or some of the larger bronchi, are at first flattened, and then give way; the aneurism breaks into the air-passages; and the patient, overwhelmed by a torrent of blood into and from his lungs, perishes in a few seconds. Or the tumour may contract adhesions with the pulmonary tissue, and destroy it to a certain extent, and so cause mortal hæmoptysis. But such cases are not always *fatal at once*. Not unfrequently the blood bursts into the sac of one or of the other pleura.

Again, according to its situation and extent, an aneurism of the thoracic aorta may press upon the œsophagus, and cause the ordinary symptoms of stricture of that tube. Hence cardiac disease, and pulsation within the chest, accompanied at length by the signs of a constricted œsophagus, form strong presumptive indications of the existence of an aneurism; and in such cases, the œsophagus may at last ulcerate through, and then copious and fatal hæmorrhage ensues. Hæmatemesis it may be called, though the blood is vomited not from the stomach, but from the gullet. A patient in the Middlesex Hospital, with symptoms of stricture of the œsophagus, one day brought up from the throat a red mass, which, at the moment, was supposed to be a bit of meat that he had been trying to swallow. It really was part of the clot from an aneurism; and it was speedily followed by a stream of red blood, and by death.

Again, aneurism of the thoracic aorta does frequently obstruct, by its juxta-position and pressure, the vena cava superior; nay, it may even obliterate that vessel, of which

Fig. 89.

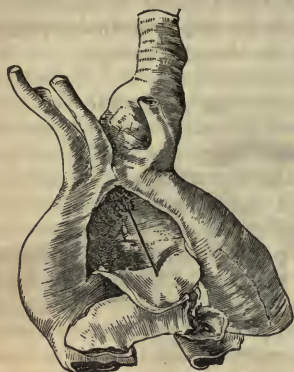


Fig. 90.

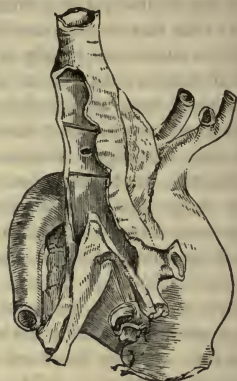


Fig. 89 exhibits a front, and Fig. 90 a back view of an aneurism of the arch of the aorta, which burst into the trachea. The opening into the aneurism from the artery, and the atheromatous patches between the coats of the latter, are well shown.

I have seen two instances. To one of these cases I alluded before, as a most curious example of dropsy. It illustrated exceedingly well the effect of venous obstruction in causing serous effusion. The man was a patient of Dr. Hawkins'. He presented a most extraordinary spectacle. His face, neck and arms were tumid and anasaruous to an enormous degree; while there was not the least trace of swelling or œdema anywhere below the ribs. He looked as if his upper half had been stuffed; and except that it was distressing, his appearance was extremely comical. His countenance was livid; his eyes seemed starting from their sockets; and even the areolar tissue beneath the conjunctiva was œdematous. The integuments of his neck and chest were quite brawny; and his arms were so swollen that they projected from his sides. The surface of the thorax in front was embossed by numerous veins, which were turgid with blood; and here and there patches of ecchymosis were visible. You may form some notion of the degree of mechanical congestion that existed, when I tell you that, upon the scarificator being applied, after a cupping-glass was taken off, upwards of twenty ounces of blood escaped in two minutes. The epigastric veins were visible and tortuous, and a free communication by anastomosis existed between these veins ascending from the inguinal region, and the mammary veins. There was a bellows-sound, which increased in loudness and harshness, from the root of the aorta to the top of the sternum. The patient soon died; and a large aneurism of the aorta was laid open by lifting up the sternum, to which the artery had adhered, and into which, indeed, it had eaten a little. Not far above the right auricle, the vena cava was totally impervious; its sides having been gradually pressed together, as the tumour grew. The other case, of the same kind, which occurred in one of my own patients, I shall have occasion to refer to hereafter.

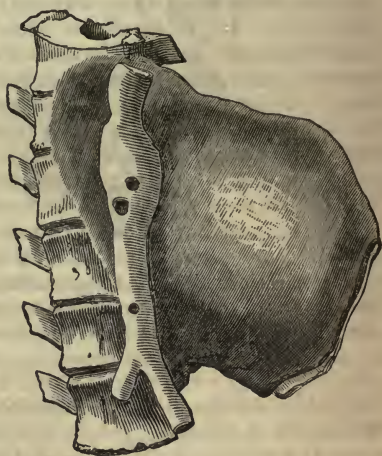
But aneurism of the thoracic aorta, and especially aneurism of its descending portion, may exercise its pressure in another quarter, and wear away the bones of the vertebræ, and cause pain in the back, radiating often in the direction of the intercostal nerves, and ultimately palsy perhaps of the parts below that portion of the spinal cord; so that pain in the back, with pulsation, may justly awaken suspicion of aneurism making its way backwards. I remember hearing Dr. Farre describe a case of this kind, to inculcate the necessity of paying attention to the *sensations* of a patient. A man came to him for advice, having been told by another physician that there was nothing the matter with him—that he was fanciful. But when an adult person makes constant complaint of certain morbid feelings in a part, the probability is that he *has* something the matter, and we must investigate the case with what helps we can get. In the instance in question, there were two signs of disease, and two only; a white

tongue, and pain in the back. The whiteness of the tongue soon disappeared under the use of some medicine addressed to the digestive organs. The pain in the back remained. Dr. Farre interrogated his patient minutely every time he visited him, till at last the man got vexed and tired, and said pettishly, "I know that if you split me down the middle, I am sound on my right side, and diseased on my left." Very soon after, he was found dead in his bed. What he had said was perfectly true: there was an aneurism pressing on the left side of the dorsal vertebræ.

Another consequence of an enlarging thoracic aneurism, sometimes observed, is pressure upon the *thoracic duct*, causing engorgement of the absorbent vessels and glands, and inanition. In short, whatever parts the aneurism may reach, and subject to its pressure, may have their function thereby suspended or disturbed, or their structure spoiled.

One sign, which I have myself frequently verified, of aneurism of the arch of the aorta, is a difference in the force of the pulse in the two radial arteries. The pulse in the one wrist (more commonly the left) will be extremely feeble, or even disappear. This happens when the state and position of the *arteria innominata*, or of the left subclavian artery, become altered in consequence of the enlargement of the aorta; and sometimes the one or the other of these arteries is completely closed up. But inasmuch as a similar difference of the pulses may arise from other causes, we can only look upon this symptom as one which may help to solve an ambiguous case. To give you an example of a difference in the beating of the arteries in the two wrists from other causes, I may mention a case in which the subclavian artery was thrown forwards, and com-

Fig. 91.



Aneurism of the aorta, which induced caries of the vertebræ, and fatal compression of the spinal cord.

Fig. 92.

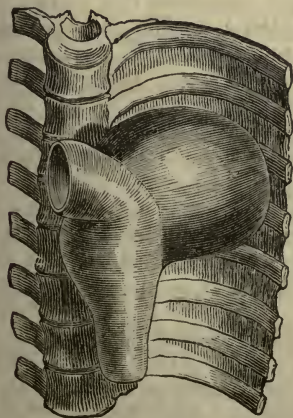


Fig. 92.—Front view of aneurism of aorta.

Fig. 93.

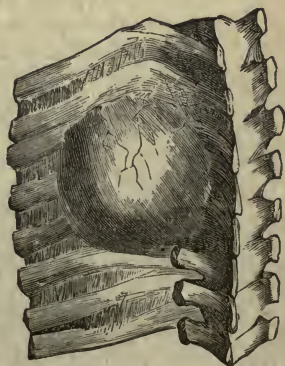


Fig. 93.—Back view of same preparation, showing the aneurism, producing absorption of the ribs, and making its way to the surface. Death was caused by part of the coagulum falling into the artery.

pressed, by an exostosis on the first rib. The case is mentioned in Mr. Mayo's Pathology. It occurred in a patient of mine, the husband of a nurse in my family.

I had a girl for some time in the hospital, in one of whose arms no artery could be found to pulsate. Why, we none of us could make out.

Some time ago, a surgeon from the country came to my house, desirous (he said) to consult me about a sense of discomfort in his head; and particularly about the state of his vision. When erect, he saw things obscurely. At three yards' distance he could see my face, but could not distinguish the separate features. What he thought very strange was that he could see perfectly well when in the horizontal posture.

On my proceeding to feel his pulse, he said, in a careless manner, "By-the-by, that is another thing wrong with me; I have no pulse." Nor could I detect any, in either arm. He then told me that, four or five years previously, a medical friend, intending to feel his pulse in the left wrist, could find none. He was confident that pulsation had existed a short time before. After a while, the movement of the radial artery returned, in a very slight degree; and then finally ceased. Within nine or ten months of this discovery, the right pulse, after growing less and less distinct by degrees, had vanished also. Though somewhat weak, and subject to faintness, this gentleman had not wasted; nor had the muscles of his arms lost either bulk or vigour. Their veins were full enough of blood. His hands were often cold; and he felt altogether worse during cold weather.

Failing to detect any pulsation in the brachial and subclavian arteries, I next felt for the carotids: but I could perceive no beating in the track of their course. I had placed my finger, for a few seconds only, in front of the left sterno-mastoid muscle—when I saw that his head drooped, his cheeks became white, and he was on the brink of fainting. But he recovered immediately. Then I made similar pressure, for a moment, on the right side of the neck, and the same phenomena were instantly repeated, with the addition of convulsive jerking movements of the head and arms. He rallied again directly upon my removing my finger, and was scarcely aware of what had happened. For a second or two he had been unconscious. His femoral arteries throbbed as usual.

I next examined his chest. There was no external irregularity or want of symmetry. Percussion gave a clear resonant sound everywhere in front. The heart was heard, beating with frequency, but without any bruit, over the greater part of the thorax. Its impulse in the præcordial region, below the nipple, was feeble: but a strong jarring impulse was communicated to the ear when the stethoscope was applied to the upper part of the sternum.

The patient complained of pains affecting his shoulders, clavicles, and the back of his neck; and of slight difficulty of swallowing.

From the intelligent physician who had attended this gentleman in the country, I learned the instructive fact that, twenty months before, a loud rasping bruit had been audible, without impulse, at that part of the sternum where he, as well as I, now found no bruit at all, and a very considerable impulse.

I could not doubt that in this painfully interesting case there was aneurismal disease of the aorta, interfering with and lessening, but not absolutely excluding, the stream of blood through the arteries which spring from its arch.

The patient continued to live on, incapable, however, of any exertion, for upwards of two years; when one evening, upon his raising himself from the sofa to cough, arterial blood suddenly poured from his mouth and nostrils, and he was presently dead. I am indebted to Dr. Durrant, of Ipswich, for an account of the morbid appearances discovered upon opening his thorax.

The ribs and sternum being raised, the aorta was seen enlarged, slightly projecting forwards, and overlapped by the lung. When the lungs had been removed, the whole arch became visible, enormously dilated, firm, inelastic, and adherent to the bodies of the second, third, and fourth dorsal vertebræ. More than two-thirds of the interior of the dilated vessel was filled up with dense fibrin, looking like muscle. The lining membrane of the aneurismal part was ossified throughout; the earthy matter lying in separate pieces, many of which resembled concave shells. The bodies of the third and fourth vertebræ, and the left half of the body of the second, were absorbed, the intervening cartilages remaining entire. The vertebral canal was bounded, in part, by the posterior wall of the aneurism. The arteria innominata was slightly dilated, the subclavian and brachial arteries were pervious, but attenuated;

their fibrous coat being softer than natural, and much less elastic. The heart was atrophied; weighing probably not more than five ounces. Both lungs were congested, and their lower borders were emphysematous. The aneurism communicated with the trachea by an aperture about as big as a quill.

It is an interesting fact, deduced by Dr. Sibson from the analysis of nearly 900 instances of aneurism, that those cases which end by rupture of the sac are attended during life with less formidable symptoms than those which kill without such rupture; and sometimes with no symptoms at all. The patient may seem, and may believe himself to be, in perfect health. The reason of this is obvious enough. Rupture is often prevented by some opposing part, upon which the enlarging tumour makes distressful pressure.

In the numerous specimens upon the table, you will find ample evidence and illustration of almost every one of the effects which I have described as apt to result from the pressure of thoracic aneurisms of the aorta. But similar effects would ensue from the same degree of pressure, however caused; and other morbid tumours, cancerous tumours in particular, are not uncommon within the thorax. Hence these same effects, considered as symptoms, are in themselves of equivocal import. If they occur in conjunction with signs of disordered circulation, or of a diseased heart, we may reasonably conjecture that they are produced by an aneurism. But we can seldom be quite sure of this, until the advancing aneurism comes near the surface, and causes an external prominence or tumour which pulsates visibly, or of which the pulsations are perceptible by the touch. And even then it may require some care and tact, to avoid mistaking an enlarged gland or a malignant growth, lying over a sound artery, and receiving an impulse from it, or communicating to it some unnatural sound, for the diseased vessel itself.

The pulsating tumour, if the aneurism have formed in the ascending aorta, makes its appearance, usually on the right side of the sternum. If the aneurism be situated in the fore-part of the arch, it produces a bulging at the sternal extremities of the upper ribs of that side. When it springs from the summit of the arch, the tumour rises above the sternum, and the sternal ends of the clavicles; and when the disease occupies the descending portion of the thoracic aorta, it will sometimes destroy the ribs and the bodies of the vertebræ, and push forward the lower portion of the left scapula: or it may show itself in front, beneath the left clavicle.

When such a tumour presents itself, and is attended with a steady, heaving pulsation, synchronous with the systole of the heart, the doubt and obscurity which may have previously hung over the nature of the patient's disorder is cleared away. A little attention to all the circumstances of the case, will generally suffice to determine its true character.

There are, however, some errors prevalent respecting these pulsating tumours, which errors I shall glance at in passing. In the first place, the pulsation of the tumour is frequently attended with a rough bellows-sound; and some persons rely upon this as distinctive of the nature of the pulsating tumour. They hold that this harsh bellows-sound is always discernible in an aneurismal tumour; and that when such a sound cannot be heard, the tumour is not aneurism. But this is a mistake. There have been in the Middlesex Hospital within the last six months (1837), two instances of pulsating tumours in the fore-part of the thorax, unattended with any bellows-sound; yet they were both ascertained, after death, to be aneurismal tumours. One of the aneurisms is before you. It results from Dr. Sibson's researches that, in reality, a bellows-sound is more often absent than present. In the case of the country surgeon, a bellows-sound was heard at a certain period of the disease, but ceased at a more advanced period. I may say the same of the purring thrill. It is a common, but by no means a necessary attendant upon thoracic aneurisms. Again, much stress was laid by Laennec, upon the circumstance of the aneurismal pulsation being single, being unattended by any second sound. But this is not a true rule if taken universally. In the instance which furnished this very preparation, the sounds were double, just like those of the heart. The second sound heard *is*, no doubt, the second sound of the heart, conveyed from the place of the aortic valves, where it originates, along the course of the vessel, to the aneurism; which often indeed lies in contact with the heart, and could scarcely fail to have the diastolic sound propagated through it. It is a fact not so easily explained, yet it certainly is a fact, that a double sound *may* be

audible in aneurisms very distant from the heart. Dr. Davies states, that he never heard a second sound in abdominal aneurisms; yet I presume that, under favourable circumstances, the sound of the closing of the floodgates at the root of the aorta, may be heard far along its channel. I can account in no other way for the second sound, heard by myself and by many others, in a *popliteal* aneurism. I mentioned before a patient whom I saw in St. Bartholomew's Hospital, and in whom an exceedingly loud diastolic sound, like the sharp whining note of a dog, was audible by the ear placed upon his arm, over the brachial, and even over the radial arteries.

There are some judicious remarks made by Dr. Hope upon the sounds that are apt to be heard in these pulsating aneurismal tumours to the right of the sternum; showing how they may be distinguished from the natural sounds of the heart itself, conveyed to that spot through some dense conducting medium. He observes (and all that I have seen has been consonant with this observation), that the first of the aneurismal sounds, when there are two, the sound that coincides with the pulse, is always louder than the natural systolic sound of the heart, and generally louder than any of the morbid systolic sounds; and that instead of increasing in intensity, as the stethoscope is moved gradually towards the præcordial region (as it ought to do, if it were the conducted sound of the heart itself), it diminishes in loudness, until it is gradually lost in the actual systole of the heart. Whereas the second sound heard over the tumour does augment as we get nearer the heart, for it is, in truth, the diastolic sound of the heart, and therefore is more audible as we approach the point where it is generated. The sounds of aneurisms of the aorta are usually audible in the back also; and if a very loud bellows-sound be heard there, where the natural sounds, if heard at all, are always much abated, that circumstance furnishes a strong additional ground for suspecting the presence of an aneurism, or of some great change in the aorta.

To give you some notion of the course which aneurism of the thoracic aorta may run, I will describe another instance of that disease which occurred under my own observation. The subject of it supplied the preparation to which I last referred.

He was a stout, healthy-looking man, forty years old, a private coachman. He became my patient in the hospital on the 8th of September, 1836. He complained of pain and tenderness around and above the right mamma. The pain was increased by a full inspiration: and when lying on the opposite side, he felt as though he was tied in the painful part.

He had been ill a month only. His illness commenced with severe rigors, and fever, and sudden pain in the side, for which he was bled three times with much relief. He attributed the attack to having lain, upon his right side, in a damp bed.

There was scarcely any projection at the spot where the pain and tenderness were experienced. By careful examination, several times repeated, I satisfied myself upon the following points.

On the right side of the thorax no vesicular breathing could be heard; and the whole was dull on percussion. On the left side percussion gave a hollow sound, and the respiratory murmur was clear and strong. In the tender spot, an inch and a half above the mamma on the right side, a strong pulsation could be felt, and two sounds were distinctly audible, the first of them keeping time with the pulse at the wrist. But there was no bellows-sound. M. Sanson, the celebrated French surgeon, was then in London, and went round with me one day, and examined this patient; and he expressed his opinion that it was not a case of aneurism, because there was no *whiz* or bellows-sound to be heard. Of course his examination was a cursory one, and I mention this circumstance merely to show you what importance has been attributed to the presence or absence of a *bruit de soufflet* in such cases. M. Sanson suggested that the heart might be displaced, and pushed over to the right side. However, it was clear to me that this could not be the case, because the breathing was deficient, not on the left, but on the right side; and, above all, because the apex of the heart could be both seen and felt beating in its proper situation, in the præcordial region on the left side. Also on the left side, percussion made on the edge of the ribs gave a tympanitic sound, indicating the place of the stomach; on the right a dull sound, pointing out the situation of the liver; so that it was not a case of transposition of the viscera, such as had been found, not long before, in one of my patients. In the course of the disease, a slight bellows-sound did become perceptible over the right mamma, when the patient sat up; but even then, the natural sounds of the heart,

without any morbid quality, could be heard in the natural position of that organ. Egophony was audible at the back part of the right side of the chest.

This patient had repeated attacks of pain, dyspnoea, restlessness, and inability to lie down; and these attacks were always most sensibly mitigated by the application of leeches to the diseased part. By the 11th of October he was so comfortable that he wished to go out; and he went to his master's in Connaught Place.

Two days afterwards, he sent to beg that I would go to see him there. He had brought up, on the preceding evening, during a paroxysm of coughing, about a pint of bright red blood; and he had continued to cough, and to expectorate small quantities of blood. I had him again brought to the hospital on the 14th of October, where he remained, apparently much the same as before he went out. But on the 19th he suddenly expired. The whole duration of his illness had been nine or ten weeks.

We found the heart natural in size and in appearance; the pericardium healthy, and containing no more than the usual quantity of serum. All the cavities were natural in their dimensions, and in the thickness of their walls; and all the valves healthy, excepting one white spot on the mitral valve, which could not have interfered with its motions.

The aorta at its origin was also natural in size; but it began to dilate just before it escaped from the pericardium, and the dilatation continued to the giving off of the left subclavian, where the vessel resumed its proper capacity. The arteries arising from the aorta did not partake of the dilatation; but the sac overlapped and adhered to the external surface of the innominate, for about a quarter of an inch from its origin. This explained a symptom I omitted to mention, viz., that the right radial artery beat much more feebly than the left.

The pouch formed by the aneurism adhered in front, for the space of two inches, to the inner surface of the third rib; and close to the edge of this adhesion there was a small irregular aperture about two lines in diameter, by which the interior of the pouch communicated with the right pleural cavity. Nearly a pint of loosely coagulated blood was found in that cavity, together with a greater quantity of serous fluid than could have belonged to the coagulum. Just above the adhesion to the rib, the pouch adhered to the substance of the lung, over a space about an inch square; and here the parietes of the artery seemed wholly wanting. This doubtless had been the channel of the copious hæmoptysis a week before his death; and it is interesting to observe that the opening of the aneurism into the lung was not immediately fatal.¹ The artery was much diseased, in the usual manner. The right lung was nearly all of it "carnified" by the compression it had undergone.

If this case had not terminated as it did, no doubt the aneurism would have made its way outwards through the ribs, as happened in the very remarkable specimen before you; in which you see that the sternum and five of the ribs have disappeared before the pressure of an aneurism in the ascending portion of the aorta. Sometimes, the tumours that form in this manner, project and attain the size of the head of a full-grown foetus before they burst.

What can we do in these melancholy cases? Not much. Certain points of practice are so obvious that it is almost superfluous to mention them. I mean the observance of quiet, and the religious avoidance of everything likely to excite or quicken the circulation: bodily exertion, therefore; straining of all kinds; mental emotion; stimulating food and drink. These are not only likely to aggravate the existing mischief, but prove often the immediate cause of the rupture of the aneurism, and of sudden death.

I mentioned, in describing the morbid anatomy of aneurism, that when the diseased vessel begins sensibly to dilate, and more especially when it is protruded into a sac or pouch, the blood begins to coagulate upon the diseased membrane. And it continues to do so, from time to time, in successive layers, so that upon dividing the aneurismal

¹ In the case of the late Mr. Liston, who died of aneurism of the aorta, the fatal issue of the disease was delayed for more than six months after a single occurrence of profuse hæmorrhage from the mouth. The aneurism, springing from the arch of the aorta, was in contact with the trachea, the front of which was thinned, and in three or four places perforated by apertures each large enough to admit a pea. These openings had been effectually stopped by laminæ of coagulated blood; with which one-half of the cavity of the aneurism was filled.

sac, you will see concentric laminæ of firmly coagulated blood. This is clearly a strengthening of the weak place—a reparatory and compensating process analogous to others which we have already had occasion to notice. And our object, here as in other cases, must be not to interfere with the natural attempts towards repair, but to assist and promote them, if we can: till, peradventure, a spontaneous cure has been performed.

This principle has long been distinctly recognized in the treatment of aneurisms that are incapable of relief by surgical means.

But it is much to be doubted whether the principle, so sound in itself, has been judiciously followed out. You have probably heard, or will hear, a good deal of Valsalva's and Albertini's mode of treating aneurisms. It was simply that of bleeding the patient repeatedly, and keeping him perfectly still, and upon as low a diet as was barely enough to prevent his perishing of inanition. The object of this plan of treatment was to facilitate the coagulation of the blood by diminishing its force and velocity, in the hope that at length such a solid barrier might be built up and organized, as would, in some sort, furnish a new wall to the artery in the dilapidated part. When this object had had the best chance of being accomplished; when the patient had been so reduced as to be scarcely able from weakness to raise his hand from the bed, to which he was strictly confined; then Valsalva increased his quantity of nourishment by degrees, until the necessary strength was restored.

Now I quite agree with Dr. Copland in thinking that this practice may be carried, and has been carried, to a hurtful extent. He says that he has seen cases "in which aneurismal tumours had existed for some time without any increase, so long as the patient avoided any marked vascular excitement, and continued his accustomed diet; but when repeated depletions, and vegetable or low diet were adopted, great augmentation of the tumour, and fatal results soon followed."

In truth we shall perceive reason to expect that this would be the case, when we consider, first, that the starving system, and the frequent abstraction of blood, diminish the quantity of red corpuscles in that fluid, rendering it more watery, and less disposed to coagulate; and, secondly, that what is called reaction—or a violent palpitating action of the heart—is very apt to follow repeated losses of blood; and this forcible action of the heart must tend rather to sweep away the existing coagula, than to cause an additional deposit.

A more reasonable and hopeful plan of management, therefore, would, in my opinion, be one which should keep the action of the heart gentle and moderate, and the motion of the blood as slow and languid as possible, without impoverishing that vital fluid. We should husband the materials of repair, and promote the deposit of them where they are wanted. A nutritious but unstimulating diet, consisting chiefly of solid food: perfect repose of mind and body; and a due regulation of the natural functions; with the abstraction of so much blood only as may be necessary to alleviate pain, or to subdue *excessive* arterial action, or to unload vessels which are manifestly oppressed by their contents: these, I humbly conceive, constitute the most rational means of furthering the endeavours of nature towards a cure. Few cures, indeed, can be hoped for in any way. Yet life may be prolonged in these cases, by great care; and the extension of existence even for a month or two, or a week, or a day, may sometimes be an acquisition of the greatest moment.

I have little to say, concerning particular drugs. Digitalis may, perhaps, be sometimes of use; and the acetate of lead is well spoken of by those who have tried it. I have not had sufficient experience of either of these remedies in the treatment of aneurism to enable me to state anything to you, confidently, in respect of their value.

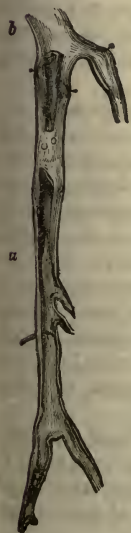
LECTURE LXIII.

Diseases of the Veins. Phlebitis; adhesive, and suppurative: consecutive scattered Abscesses. Treatment of Inflammation of Veins. Effects of the gradual obstruction of large Venous Trunks.

YESTERDAY I concluded what I had to say, as a physician, respecting diseases of the *arteries*: and this seems as fitting a time as any for taking a final notice of some of the morbid conditions of the *veins*—especially of their inflammation. Already, more than once, brief reference has been made to this important subject; important, whether we consider the large amount of mortal disease which it comprehends, or its wide and intimate relation with general pathology. I should have done better if I had given you, in an earlier part of the course, a more complete and connected account of *phlebitis*, and its consequences. It is this malady which gives to many fatal injuries, and to many, nay to most, of the fatal operations of surgery, their mortal character: it is of surpassing importance, therefore, to the surgeon. The same malady lies at the bottom of the deadliest cases of puerperal fever: it is consequently of the deepest interest to the accoucheur. It occurs also, not seldom, in the practice of the physician, appalling him by its insidious, its rapid, and too frequently its resistless course. Moreover, its pathology, which has been successfully investigated only within these few years, furnishes a key to that of other morbid conditions of great moment.

The first effect of inflammation of a vein is to impede, or to arrest, the passing blood, which, coagulating upon the inflamed surface, adheres to it.

FIG. 94.



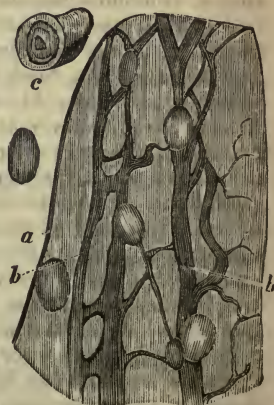
Fibrinous Phlebitis. *a.* The femoral vein, occluded by solidified contents. At *b*, the saphena enters; and consolidation ends abruptly there.

In some instances the inflamed coat of the vessel is merely (as Mr. Hunter said) furred over: in others its channel is completely dammed up. The obliteration of a small vein in this manner can seldom have any serious consequence; but much suffering, and distress, and even death itself, may result from the sudden and continued obstruction of one of the large venous conduits. For example, the painful disorder, called *Phlegmasia dolens*, is caused by a stoppage of the blood in the *femoral vein*. A similar arrest of its current in the *sinuses of the brain*, is a mortal change.

This adhesive form of phlebitis is a local disease. Whatever ill effects it may produce are purely mechanical; and depend upon the closure of the canal. If the organ mechanically affected by it be not a vital organ;—if the system can await the development of a collateral venous circulation;—all, at length, may end well. Sometimes, indeed, as the inflammation gradually subsides, the coagulum is softened and partly reabsorbed, the blood drills for itself a fresh passage through the centre of the plug, and the circulation is restored in its accustomed channels.

This is, fortunately, the commoner form of phlebitis: but sometimes the inflammation advances beyond the adhesive, and into the suppurative stage. Even then the disease may remain a local one. The adhesive process may bound and isolate the suppurative in both directions; and an abscess in the part is then the usual result.

FIG. 95.



a. Broad ligament of uterus
bb. Uterine veins containing phlebotites; *c.* Phlebotite sawed open, exhibiting its concentric arrangement.

But if the suppurating surface of the vein be not so shut off, and pus mingle and circulate with the blood, the disorder is no longer merely local. The contaminated blood is conveyed to distant parts, and the whole system tainted. The malady has become general, and of the most formidable character.

It had long been noticed, as a matter of fact, that collections of pus were not uncommon in various parts of the body, when death had followed mechanical injuries, or great surgical operations. Abscesses of the liver, in particular, were known to be associated with mortal fractures of the skull. Very fanciful reasons were assigned for this coincidence. By degrees it was ascertained that these scattered collections of matter—occurring most commonly in the lungs and liver, but not unfrequently in or near the joints also, in the serous cavities, among the muscles, in the brain, in the eye, and elsewhere—were connected with the introduction of some vitiating secretion, and especially of pus, into the current of the venous blood.

And this step having been gained, fresh speculations arose, concerning the manner in which the internal collections of pus were formed. In the viscera they were usually small, well-defined, surrounded by the healthy tissue of the organ, and several in number. Some maintained, that the pus, in substance, was carried to the parts in which it was found, and there simply deposited. Others were of opinion that the tainted blood created in the system a general tendency to inflammation, which was developed in many places simultaneously. Neither of these suppositions was quite true, neither of them quite false. The pus discovered in the serous cavities was accompanied by unequivocal traces of inflammation in those parts. This alone rendered it probable that the smaller purulent collections were not merely dropped there by the blood in its course (a thing very difficult to conceive), but were the products of actual inflammation, excited somehow in those very spots. And it is now believed that these abscesses *of*, as well as *in*, a part, proceed from suppurative inflammation, provoked by the presence of particles of pus, brought thither with the circulating blood.

I told you before, that minute foreign matters entering the blood, and failing to pass out of it again through the natural emunctories of the body, are liable to be stopped when they arrive at the first network of capillary vessels that lies in their course. Now the blood, circulating in the veins, reaches (much of it at least), in each of its circuits, two such great networks, the hepatic and the pulmonary. Through the pulmonary network all the blood must pass, through the hepatic some of it; and it is there, in the capillary tissue of these organs, that particles of pus, and other material substances, foreign to the blood, and incapable of being eliminated with the customary excretions, are apt to stick, or to be entangled, and to excite inflammation. Some of them, however, in general, pass on, and arriving at the left side of the heart, are transmitted, with the arterial blood, to various parts of the body, there to exercise a similar deleterious influence.

Such was, and is, the theory: and it has been tested and confirmed by direct experiment. Inasmuch as the conveyance of the *pus* cannot be traced by the eye, nor the manner of its being collected into an abscess demonstrated, except by inference, Cruveilhier introduced *quicksilver* into the veins of animals; a metal which is liquid, and divisible into very minute particles, and which exerts no chemical agency upon the vital fluid. When the mercury was inserted into the veins which concur to form the vena portæ, the whole, or the greater part of it, was arrested in the liver. In that organ, the animal being killed a certain time after the introduction of the metal, small, roundish, red spots were always discoverable, which passed gradually into little abscesses surrounded by a halo of inflammatory redness; and in the centre of each red spot, and of each abscess, lay a minute globule of mercury. A few similar points of suppuration were usually to be seen in the lungs also. But when the quicksilver was put into the blood in its direct course towards the vena cava, then it was in the lungs that these points were either exclusively detected, or at any rate most numerous.

You must, I think, perceive how strictly these experiments bear upon the rational humoralism acknowledged at the present day. If pus, and mercury, may thus be distributed to particular organs, and thus excite circumscribed inflammation, so doubtless may other extraneous impurities—introduced by the poison of what is called good living, by the respiration of foul air, and in various other ways—reach, and settle in

different parts of the body (the liver, the lungs, the kidneys, the joints), and there produce, if not inflammation and pus, yet such changes at least as spoil the texture of the organ, and pervert its healthy office. That cancer is propagated in this way we have heretofore seen reason to believe. In all probability the deposit and increase of tubercles fall under the same law. There is however this remarkable difference between tubercles and phlebotic abscesses, that the former occupy chiefly the upper portions of the lungs, while the latter are generally most numerous in the lower lobes.

Suppurative phlebitis — with its horrible effects — is liable to arise, not only after severe but also after slight injuries; from the trivial as well as the grand exploits of surgery; nay, spontaneously, as it were, without any local hurt, under the agency of natural causes, such as exposure to cold. And the part in which the phlebitis occurs has some influence, as you will now understand, in determining the principal seat of these scattered abscesses. When they succeed amputation of a limb, or fracture of the skull, or the interference of surgery with varicose veins, or (as they may) even the simple operation of phlebotomy, they are likely to be most numerous in the lungs. But they are more conspicuous to hasty observation in the liver than in the lungs; and that is why hepatic abscess was supposed to have some special connexion with injuries of the head. Morgagni, however, long ago pointed out the fact, that other parts also were affected in those cases. Again, we may expect to find these disseminated abscesses chiefly in the liver, when suppurative phlebitis occurs in any of the tributary veins of the vena portæ; when it supervenes, therefore, upon operations involving the intestines — operations for the release of hernia, for healing fistula in ano, for the cure of piles.

It is, however, very common for the poison to pervade the whole body, and for abscesses to form in various other situations, as well as in the lungs and liver. I once saw a young woman die, in the Middlesex Hospital, from phlebitis, with large abscesses in many parts, and especially in the joints, after the simple excision, with scissors, of some small spongy irritable growths about the orifice of her urethra.

Two or three instances of suppurative phlebitis, unconnected with any known hurt, and originating apparently in exposure to cold, have fallen under my own observation: but I prefer giving you the following short case, with the details of which I have been favoured by Dr. Malden, of Worcester.

Miss ———, a teacher in a ladies' school, was attacked, after exposure to wet and cold, with acute pain, heat, and redness, in the front of the left fore-arm. Mr. Cole, an eminent surgeon, of Bewdley, by whom she was at first attended, discovered inflammation following the course of the cutaneous veins. Upon its subsidence the veins were left like hard cords. Soon after, the right arm was affected in a similar way: and next, both the lower extremities, which became anasarcaous. All this was attended with paroxysms, simulating those of tertian ague; exhausting sweats; diarrhoea; and a frequent feeble pulse. At the end of a month, deep-seated fluctuation was detected in the right thigh, three inches below Poupart's ligament. The abscess gradually approached the surface, and was opened, and more than three pints of very fetid pus was discharged. The wound never closed, and she sank, exhausted, a month after it was made.

There was no pain, premonitory or attendant, connected with this formation of matter.

The abscess was traced, after death, upwards, behind the muscles of the pelvis, as far as the sacro-iliac symphysis, where the bones were extensively carious.

Many of the superficial veins, both of the upper and the lower extremities, were found to be completely obliterated by adhesive inflammation, or sealed up by coagula of blood.

This spreading and morbid inflammation of veins is sometimes so remarkably prevalent, as to partake of the character of an epidemic disorder: and this, its occasional prevalence, appears to be owing to some peculiar condition of the atmosphere — or rather to some predisposition of the human body, engendered by the operation of influences which are probably atmospheric. During such periods prudent men refrain, if they can, from the performance of surgical operations.

The view which I have now set before you of suppurative phlebitis, and of its distant effects, involves some apparent difficulties, and some curious questions. A short consideration of these may serve to throw a clearer light upon the main subject.

First, then, how is it—if indeed the disseminated abscesses result from the introduction of pus into the blood—how is it that we do not meet with them oftener? Pus is absorbed, in numberless instances, without the occurrence of any such formidable consequences. We see great abscesses disappear spontaneously, and yet no other smaller scattered abscesses ensue. Does not this fact invalidate the theory of the cause and formation of such distant points of suppuration? No. It seems that, for their production, pus as such, pus in substance, pus in the mass, must be received into the veins, and circulate with the blood. The pus which is taken up by ordinary absorption, is altered, probably by that process, before it reaches the blood: at any rate it has not the same mischievous and fatal effect.

You may here inquire in what manner pus gets into the circulation, in consequence of an amputation? Is it not absorbed from the suppurating stump? I conceive not. Supposing the amputating knife to cross and sink into an existing abscess, and to divide a vein—then, indeed, pus might be sucked into the vein, and the usual consequences follow. But the veins that lead to, or rather from, a stump, become blocked up, and impervious, from adhesive inflammation, or from mere coagulation and adhesion of the blood in them, before the stump has had time to suppurate. How then does the pus ever find admission? No doubt it is a product, in this case also, of phlebitis. The interior of a vein inflames, and goes on to suppuration: and the pus which it pours forth mingles, as pus, with the circulating stream.

Indeed these scattered abscesses appear to originate almost always in phlebitis. Such is the opinion of my colleague, Mr. Arnott, who has contributed a valuable paper on this subject to the *Medico-Chirurgical Transactions*. Such is also the opinion of M. Cruveilhier. It has been objected that, in some fatal cases of this kind, no phlebitis could be detected; that the principal veins have been diligently traced, yet no vestige of suppuration, nor even of adhesive inflammation, has been visible. To make this objection valid, *all* the veins throughout the body should be scrutinized; and that has seldom, I fancy, been done. I have known several instances, in which most of the larger trunks were searched in vain, till at length a short track of one of them, an inch or two perhaps, was found, bearing marks of having been inflamed. You must not conclude therefore against the antecedent existence of phlebitis, until you have examined every vein in the soft parts of the body: no, nor even then. You must go deeper than the more obvious veins. There is good reason for believing that the *bones* and their veins are often the seat of the primary mischief, the fountain from which the pus, which thus renders the blood a poison, first proceeds: the veins, for example, of the *diploë* of the skull, when scattered abscesses ensue upon injuries of the head; the veins of the bones of the extremities after unsuccessful operations. There is yet another explanatory supposition applicable to some cases. All local traces of the primary inflammation may vanish before death, while the vital powers are being undermined, and about to sink under its secondary effects.

Cruveilhier repeatedly performed the following experiment, and always with similar results. He introduced crude mercury within the hollow shaft of the thigh bone of a living dog. When the quantity was considerable, death occurred in a few days, and the metal was found strewed thickly through the lungs, each globule occupying a capillary branch of the pulmonary artery, and surrounded by a small sphere of inflammatory redness. When the quantity was minute, the animals lived longer, and little *abscesses*, enclosing each a particle of mercury, were then discovered in the same organs. The mercury he supposes to have found a direct entrance into the blood, in these cases, from the cancellous portion of the bone, and through the same channel it seems to be that pus often enters the circulation. You may remember my relating some fearful examples of scattered abscesses, supervening upon chronic disease of the bones of the ear. Cruveilhier states that, having been present at the examination of the body of one who had sunk after amputation of the leg, and whose lungs were full of little abscesses, he sought, without success, for some inflamed vein: but upon dividing the tibia and fibula, he found the spongy extremities of these bones infiltrated with pus. Here, beyond question, had been the source of the visceral mischief.

Occasionally, however, these disseminated abscesses may acknowledge another source than inflammation of a vein. I showed you, in the last lecture but one, that

suppuration may occur within the heart, and pus be poured directly into the circulating stream of blood. But this can seldom happen.

The local phenomena, when a superficial vein of some magnitude is inflamed, are pain and tenderness in the course of the vessel, which, in the adhesive variety of the complaint, is soon converted into a tangible, hard, and sensitive cord. Whether the vein be near the surface, or deeply seated, there is usually more or less œdema of the areolar tissue of the part. Phlebitis of this kind has been sometimes confounded, I believe, with inflammation of the lymphatic absorbent vessels. You distinguish the latter by the slenderness of the painful cord; by its position, which is still more superficial than that of a subcutaneous vein; by the number of little knots which diversify its course; and by the streaks and patches of bright inflammatory redness which appear along the same track. Dr. Graves remarks (*Clinical Medicine*, p. 454), that inflammation of the lymphatics "is seldom continuous, but is developed at certain insulated points." Velpeau has laid down the following aphoristic distinction between the two. "Angioleucitis is seen, but not felt, while phlebitis is felt rather than seen: so that the complaints might be discriminated even with closed eyes."

In the suppurative form of phlebitis the general symptoms take the lead. A sudden change in the aspect and manner of the patient often marks the commencement of the constitutional affection. The formation of pus in separate and often distant parts is rapid, and frequently unannounced by any local pain. When however the joints, or parts near the joints, are the seat of suppuration, much soreness is complained of, and the malady is liable to be mistaken for rheumatism: and when the serous cavities are implicated, the pain is sometimes severe. Suppurative phlebitis is commonly attended in its progress with repeated shiverings, which are sometimes periodical; and with profuse sweats, and occasionally with copious and very unnatural discharges from the bowels. These last have been noticed in animals soon after the introduction of pus, or of putrid matters, into their veins. Nature seems to attempt to eliminate the poison in this way: and where the quantity of pus so introduced has been small, the attempt is now and then successful. But in general there is a continual supply of the noxious substance, and the system is irrecoverably infected. The skin acquires a yellowish hue, as if the patient were faintly jaundiced. Here and there upon the surface of the body patches of erysipelatous inflammation are apt to appear; and sometimes, of superficial gangrene. The pulse is almost always rapid. In most cases, but not in all, symptoms occur resembling those which mark low forms of typhus fever. Very constantly there is great agitation, and a signal disturbance of the nervous system.

I have called this purulent infection of the blood a formidable disorder: in truth it is almost always a fatal disorder. Yet that it is not inevitably mortal I know by a case which has recently occurred in Mr. Arnott's practice at the Hospital, and which he permits me to mention. He had occasion to amputate the fore-arm of a man whose hand had been crushed by machinery. Two or three days after the operation, the patient's pulse quickened, and he had a severe rigor. These two circumstances led Mr. Arnott to apprehend the supervention of phlebitis; and accordingly one of the large, superficial, visible veins of the fore-arm became swollen, hard, and tender. Leeches were applied along its course; and the parts were kept covered with the water dressing. In no long time an abscess formed in the other arm: next, a large one in the back, from which twenty ounces of pus were evacuated; then one beneath the glutei muscles of the buttock, on both sides—each of these two contained about sixteen ounces. In short, dating between the beginning of October and the middle of December, no less than seven collections of matter presented themselves in various places. The last of them was in a very unusual part, beneath the man's tongue, in the ordinary situation of ranula, for which, indeed, it was at first mistaken. In every instance the pus was let out as soon as possible, and the main feature in the general treatment was the administration of opiates, and of wine, with a liberal allowance of good beef-tea in the earlier stages, and of meat afterwards. This man recovered; and was seen in the month of May following, in perfect health. The case is extremely interesting. It shows, I say, that suppurative phlebitis, even when it strews consecutive inflammation and suppuration throughout the body, is not absolutely and hopelessly fatal. Whether abscesses, from this cause, distributed in the lungs or liver, are capable of repair, I cannot tell you. Under the treatment employed, the

inflammation of the vein in the arm gradually subsided. All outward evidence at least of its existence, all induration even, disappeared; and presumptively all inward evidence too. So that, had this patient sunk, late in the course of his disorder, under the multiplied secondary abscesses, his venous system might probably have been searched in vain for any remaining traces of phlebitis: and yet we know that at one time he *had* phlebitis, more severe and extensive than belongs to the natural and kindly healing of every stump.

The treatment found most suitable in phlebitis has just been briefly indicated; local depletion when the inflamed vein is accessible; regulation of the bowels; strong animal broths and wine to support the strength; opiates to tranquillize nervous irritability and restlessness. Our object is in the first place to subdue and resolve the inflammation: or at any rate to prevent its passing beyond the adhesive stage. To this end, the vein being obvious and superficial, we apply leeches, cold lotions, or fomentations.

[In cases of phlebitis from venæsection, the late Dr. Physick recommended the application of a blister over the orifice in the arm, as soon as the inflammation commences, the orifice itself being first defended by a plaster of simple cerate spread on linen. He found this to be invariably beneficial in causing the arrest of the inflammation. Dr. Dorsey also found a blister thus applied to produce always a good effect; and in a case related by Dr. Evans, (*Transac. College of Physicians of Philadelphia*, vol. ii., p. 106,) the good effect of blisters in a case of spontaneous phlebitis, was strikingly evinced.—C]

During the progress of the malady, especially when suppurative phlebitis is prevalent, it would be unsafe to cut into a large vein, lest, by that slight violence, we establish a fresh local phlebitis. Indeed, after the suppurative form has once been set up, general blood-letting does no good; but, on the contrary, impairs the power of the system at large to struggle against the disease.

The *obliteration* of a large vein, whether by adhesive phlebitis or in any other way, is perilous in proportion to its magnitude, and to the rapidity with which its complete occlusion has been effected. The gradual stoppage of even the largest—the primary venous trunks, the *venæ cavæ*—admits of some degree of compensation. In one instance of this kind, which I briefly described yesterday, and which I myself witnessed, the *superior* cava was flattened, and its channel completely effaced, by the pressure of an aneurismal tumour: in another, which I mentioned formerly, on Mr. Kiernan's authority, an immense varix of the superficial veins of the abdomen supplied to the returning blood the passage denied to it, in its natural course, by the partial obliteration of the *inferior* cava. To impress upon your recollection the ordinary phenomena that result from these grave derangements in the hydraulic machinery of the body, I will state here, from my hospital case-book, the outlines of two additional examples of a similar character.

James Buck, aged thirty-three, was admitted on the 6th of March, 1838. The appearance of this man was very remarkable. His countenance was swollen and livid; his eyeballs projected; his lips, the end of his nose, and the rims of his ears, were of a deep purple colour. It was manifest that the blood did not freely descend from the head. Further evidence of this became apparent when the trunk of his body was uncovered. The throat was very broad, full, and tumid, like that of a goitrous person, yet the swelling was not owing to enlargement of the thyreoid gland, nor to œdema; but felt firm and fleshy. The jugulars were distended; and the whole surface of the thorax in front, with that of the shoulders, and of part of the abdomen, was thickly overspread with a network of prominent veins. The external mammary veins were seen to communicate freely with veins proceeding from the neck on each side, with the veins of both the upper extremities, and with the epigastric veins from beneath. Here and there were patches of minute purple varicose branches, crowded closely together.

He told us that whenever he stooped down, to tie his shoe-strings for instance, he became giddy, his head swelled, and his face and ears grew black: that he was very nervous, easily flurried, and dreamed much, thinking that he was flying in the air, falling down precipices, and the like. He had not noticed any swelling of the face

or throat until three weeks previously; and he had never, he said, had a day's illness before. He knew of no cause for the complaint; had been making no extraordinary bodily effort; had never suffered rheumatic fever. He professed, also, temperate habits, but he had been a soldier, and afterwards a pugilist, and his wife informed me that he had led an irregular life.

The evidence I say was strong, of some obstruction to the return of the blood through the superior cava. Now such obstruction is most commonly produced by intra-thoracic tumours—sometimes by carcinomatous, much oftener by aneurismal tumours. There were no circumstances to make it likely that malignant growths existed; but there were circumstances which corroborated my first suspicion, that the symptoms were dependent upon aneurism of the aorta, or of one of its primary branches.

There was indeed no external prominence, no pulsative or other swelling, no aneurismal whiz, to guide us to this diagnosis. Upon careful and repeated auscultation of the chest, the murmur of respiration was found to be in some parts feeble and unequal. This might consist with the presence of any kind of tumour. The heart's action was heard, and felt, strong and heaving, in the proper place, beneath the left nipple. To the right of the sternum also, and near the middle of its upper portion, one's ear was distinctly jarred at each systole of the heart, though with less force than in the præcordial region. But in the space intermediate between these two spots, no such jarring sensation was perceptible, although the heart could be heard, beating with a slight bellows-sound. Moreover, the right radial artery was considerably weaker and smaller than the left. This showed that the innominate was interested in the disease. The symptoms, taken together, left no doubt on my mind that there was an aneurismal pouch beneath the sternum, where the jar was experienced. I have gone somewhat into particulars to show you how confidently sometimes, by close observation, you may pronounce upon the condition of parts which you can neither see nor touch.

All that could reasonably be hoped for from medicine, was postponement of the evil day. To relieve the oppressed blood-vessels by taking away part of their contents, by freely purging the patient, and by setting his kidneys at work—this was what was to be attempted; and this was done. He was repeatedly cupped, and always with most sensible relief to his feelings, the blood flowing copiously. Purgatives and diuretics also acted well; and so much was the man benefited by these measures, that *twice* he left the ward, and became an out-patient.

About the middle of June a new symptom arose—severe pain extending from the right collar-bone across the shoulder. He died on the 10th of July. Three or four days before his death, he had rigors and extreme dyspnoea, complained of pain over the whole thorax, and declared that his “heart seemed on fire.” These symptoms were caused by the supervention of pericarditis, which speedily proved fatal. A thin layer of recent, reticulated lymph was found covering a considerable extent of the surface of the heart.

The body was examined by Mr. Shaw, after injection of the veins, and of the thoracic duct.

There was a large aneurism of the arteria innominata; of which I omit all particular description, my present object being to draw your attention to the state of the veins. The two great trunks that, coming from either side, unite to form the vena cava superior, were completely closed up, as well as the corresponding portion of the cava itself, which was lost and confounded in the walls of the aneurismal sac. The subclavian veins were pervious up to the point where they joined the internal jugulars, but no further. The preparation of these parts, which is before you, and the rough diagrams which I here exhibit, will aid your comprehension of the mode whereby the blood, descending from the head, found its way at length, through many circuitous channels, to the heart. The larger deep-seated compensating veins were not greatly magnified, but the number of the smaller branches was much augmented. The *vena zygos*, for example, was very little above its usual size; yet it was apparently provided with a greater number of considerable branches than are commonly observed under natural circumstances.

As the veins into which the trunks of the absorbent vessels discharge their contents were obliterated, it became interesting to examine the state of these vessels, and

of the lymphatic glands. But the condensation and confusion of all the parts around the tumour rendered it difficult to trace the thoracic ducts. The glands were remark-

FIG. 96.

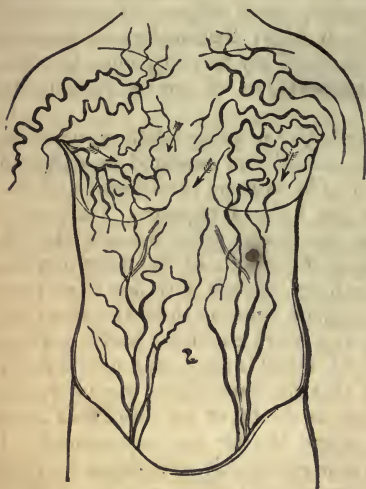
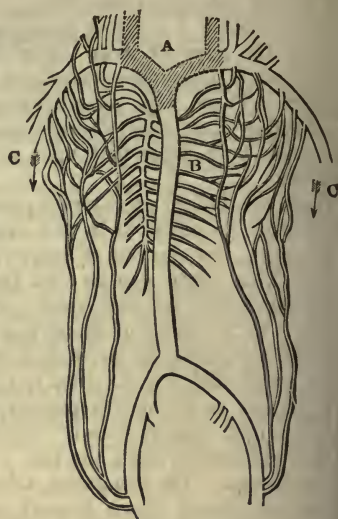


FIG. 97.



- A Obstructed veins.
- B Seat of the right auricle.
- C Thoracic, pectoral, and mammary veins, conveying the blood in a contrary course to its usual direction, and anastomosing with the intercostal and epigastric veins.

ably large, of a purple colour, and gorged with bloody serum. The fulness of the neck, noticed during life, was occasioned by this turgescence of the *glandula concatenata*. Large glands were seen studding the walls of the aneurism, and adhering to the great vessels connected with it: *i. e.* in situations where, under ordinary circumstances, such glands, from their minuteness, can scarcely be detected at all.

With this case, contrast the following:—

Harriet Baldwin, thirty-three years old, was admitted, December 29, 1840, anasarous as high as the hips, and with an enlarged abdomen. The swelling had begun, she said, a fortnight before.

She complained of cough, and of expectoration, which was sometimes tinged with blood. She could not lie down in bed for dyspnoea. Her urine was scanty and dark-coloured.

She told us that she had dropsy, quite as bad, five years ago, of which she was cured in St. Bartholomew's Hospital.

All this we learned in the admission-room. The next day, when she was in bed, we learned a good deal more.

The large abdomen did not owe much, if any, of its bulk to ascites. It contained a palpable tumour, filling the right hypochondrium, and extending thence far beyond the umbilicus to the left, and into the right groin. This tumour, from its situation, and from the continuous dullness elicited by percussion from the right mamma downwards over its whole extent, was evidently formed by the liver, much enlarged, and out of its place.

But, besides the tumour, the abdomen presented on its surface a very singular appearance. Two zigzag lines of large, varicose veins ran up its middle, near the right edge of the linea alba. These, which were evidently the epigastric veins, inosculated above with the mammary. Large but straighter veins, wandered over the front of the thorax on both sides.

The swollen legs of this woman were quite purple from innumerable clusters of small varicose veins.

Other symptoms also there were, but I pass them by as irrelevant to my present subject. It was plain that the current of the blood along the vena cava inferior was suffering impediment. That vessel was presumably compressed, perhaps rendered totally impervious, by the superjacent tumour. The existence of the tumour; the great œdema of the legs, compared with the slight amount of liquid in the cavity of the belly: the varicose state of the cutaneous veins of the legs; and, above all, the remarkable condition of the superficial veins of the abdomen:—these were the evidences. The blood from the lower extremities passed mainly by the way of the intercostal and subclavian veins, to the heart. Death took place on the 19th of January, 1841. A part of the liver appeared perfectly healthy; another part contained a prodigious quantity of hydatids. When removed from the cyst which had contained them, they filled a large wash-hand basin. The sides of the inferior cava were pressed together by the tumour, and its channel was thus completely closed up for the space of three inches.

In each of these two cases, the closure of the great venous trunk was effected gradually, as the compressing tumour augmented; and time was afforded for the development of collateral supplementary channels. In both cases the superficial veins of the thorax and abdomen contributed largely to supply the growing needs of the system: but the stream of returning blood ran oppositely in the two cases; from above downwards in the first, from below upwards in the second. The direction in which the blood in the veins is moving can, of course, be always readily ascertained; and this might furnish a test, were other tokens wanting, whereby to determine whether the obstruction lay in the superior or in the inferior cava. And there is another circumstance worthy of remark, and of which the same use might be made. In the first case, the dilated veins of the thorax were tortuous, those of the abdomen direct. In the second this was reversed; the epigastric veins were singularly sinuous, the mammary veins were straight. In other words, those veins were, in each instance, contorted and winding, in which the actual course of the blood was retrograde. The vessels were bent and twisted as the current forced its backward way against the opposing but ineffectual barrier of the valves.

It is impossible, I think, to find more clear evidence than these interesting cases exhibit, of the power inherent in the animal body of rectifying, to a certain extent, its own accidental derangements. You cannot, under such circumstances, overlook the existence, or mistake the tendency, of a *vis medicatrix naturæ*.

FIG. 98.



LECTURE LXIV.

Asthma: its nature; complications; exciting causes; and treatment. Diseases of the Œsophagus: Inflammation; Stricture; Spasm; Dilatation.

I MUST not leave the subject of thoracic disease without saying a word or two respecting *asthma*; a complaint which might have been properly arranged among the *erubus spasmodic diseases*, in a former part of the course. But I purposely deferred speaking of it, because, though in many instances purely spasmodic, and independent

of any discoverable faulty structure, it is still more often connected with organic diseases of the heart, or of the lungs; which diseases had not then been described.

I scarcely need caution you against the vulgar error of calling all kinds of difficult breathing by the name of asthma. You will be constantly meeting with persons who, labouring under some permanent embarrassment of the respiration, tell you they are asthmatic. They conceive that asthma is simply an inconvenient, and not at all a dangerous affection; and they please themselves with the notion—consumptive patients and their friends do this continually—that they are *merely* asthmatic. Asthma *is* dyspnœa, but dyspnœa is not *necessarily* asthma.

Asthma may be defined as being—great difficulty of breathing: occurring in paroxysms; accompanied by a loud wheezing sound of respiration; passing off, after some hours, with more or less mucous expectoration; and unattended with fever. And these paroxysms of dyspnœa are believed to depend upon a spasmodic constriction of the bronchial tubes.

To go rather more into detail: the phenomena which constitute and characterize a fit of asthma, are somewhat as follows:—The patient, if he have previously suffered under the disease, has usually some well-understood warnings that an attack is hanging over him. Loss of appetite; frequently much flatulence and eructation; languor, irritability, drowsiness, oppression, chilliness; and he goes to bed ill and uncomfortable. The dyspnœa comes on generally after midnight, about two or three o'clock in the morning; often during sleep; and the patient wakes with a sense of tightness and constriction about the chest, and an inability, as it seems to him, freely to expand it. He is obliged at once to rise up; and he sits, leaning forward, with his knees drawn up, his elbows on his knees, and his head supported by his hands, labouring for his breath, and making a wheezing noise so loud as to be audible at a considerable distance. He experiences a strong desire or necessity for fresh air; opens the door of his room and goes out upon the staircase, or flies to an open window, even in very cold weather; and remains there, with his head out, sometimes for hours. That he can do so with impunity furnishes a strong presumption that it is the nervous system which is principally affected in these cases. His extremities at the same time are usually cold, and his countenance is distressed and haggard: while the trunk of his body may be wet with perspiration. Sometimes the face is a little flushed and turgid; but more commonly it is somewhat pale, and shrunk. The pulse is often small, feeble, and even irregular; and in many instances there is much palpitation of the heart. At other times the pulse remains undisturbed. If urine be passed, as it frequently is, at the beginning of a fit of asthma, it is copious and watery, pale, clear, and without smell, like the urine of hysterical women. The bowels are also sometimes relaxed, with “something (as Sir John Forbes observes) of the impatient hurry and imperfection of spasmodic action.” There may be some propensity to coughing, but the patient can hardly achieve a cough; and is so engrossed with his breathing, that he can speak in an interrupted manner only, with difficulty and uneasiness. He has not, however, in general, any misgivings about the event of the attack, but looks forward with hope to the expected termination of the paroxysm.

“These symptoms often continue for many hours together; and particularly from midnight till morning is far advanced. Then, commonly, a remission takes place by degrees. The breathing becomes less laborious, and more full: so that the person can speak or cough with more ease. And if, as is usually the case, the cough brings up some mucus, the remission becomes immediately more considerable, and he falls into a much-wished-for sleep.”

Paroxysms of this kind will often continue to recur for many nights in succession; remitting at length in their severity: and ceasing for a period, altogether.

During the intervals between these paroxysms, in the daytime, the patient *may* be perfectly well; but he seldom *is* so; though so great is the difference between his condition during the remissions, and his condition in the paroxysms, that he declares, and perhaps fancies, that he is quite well. You will mostly find, however, that he is short-winded; that he does not utter many words of a sentence before he pauses to take breath; that slight bodily exertion hurries his respiration; and that he is not easy in a horizontal posture, with his head low.

Although the dyspnœa is thus intermittent, or remittent, you are not to suppose that the paroxysms recur with the regularity of those of ague. The interval is of

uncertain duration; and the circumstances of the paroxysm differ in different instances. I may remark also, that when the paroxysm ceases, with little or no expectoration, the case is said to be one of *dry* asthma: when the expectoration is copious, it is *humid* or *humoral* asthma.

Now this, I say, is looked upon as being essentially a spasmodic affection. Upon what grounds?

Why, in the first place, the patients have a *sensation* of constriction in the chest. An old gentleman whom I saw lately, and who is subject to fits of asthma, made use of the term *cramp* when he described what he felt about the thorax; and his attacks were always accompanied or succeeded by *actual* cramp of the muscles of the calves of his legs. This is no uncommon circumstance, this co-existence of decided spasm in other parts; and it throws some light upon the nature of the disorder. Again, the rapidity with which the dyspnoea comes on, and the suddenness with which it often abates, resemble the caprice of spasm. The supervention of extreme, sometimes enormous flatulence, and the secretion of hysterical urine, mark also the nervous character of the symptoms. So likewise do the *juvantia* and the *lædientia*, as I shall presently explain further; the affection being suddenly produced by certain causes of irritation, and even by mental feelings—suddenly relieved, sometimes, by medicines which are reckoned antispasmodic. If we add to these considerations the fact that the dead bodies of asthmatic patients have often, on being examined, presented no vestige whatever of disease, either in the lungs or in the heart, we obtain very strong presumptive evidence, that the phenomena attending a fit of asthma may be the result of pure spasm.

But if this be so, what are the muscles thus fixed in spasmodic contraction?

You are doubtless aware that the air-tubes are encircled with a series of little fibres, or bundles of fibres. I have more than once shown you these, exaggerated by hypertrophy, in the larger bronchi. They have been traced, by Reissessen, in tubes of very small diameter. Laennec states, that he had distinguished them in bronchial ramifications less than one line across. Now, supposing these circular fibres to be muscular, it becomes at once, and *à priori*, likely that they, no less than other muscles, should be liable to spasm. And the phenomena of asthma prove, to my mind, that they are so. Analogy would say that the fibres, thus disposed, are slender muscles, similar to those which surround the intestines and the urinary bladder: and the microscope, scrutinizing their minute texture and appearance, asserts that they are actually muscles, of the unstriped kind; like other involuntary muscles subserving the organic life. This fact—which, I am aware, has been doubted—I state upon the authority of Professor Todd and Mr. Bowman; both known to you all, as faithful and expert observers. But a test, less fallible than the microscope, has practically settled the question. Dr. Williams has demonstrated, by a set of ingenious and satisfactory experiments, that the lungs and air-tubes are actually *contractile* to a very considerable degree, under electrical, chemical, and mechanical stimuli. The contractions take place steadily and slowly; and are followed, as soon as the stimulus is withdrawn, by an equally gradual relaxation. This is very like tonic spasm. The contractions were rendered apparent by means of a bent glass tube, containing coloured liquid, and adapted to the windpipe of an animal just deprived of life. The column of liquid in the glass tube would of course be readily moveable by any contraction of the lungs and air-tubes, causing pressure of the included air against it. In one of the experiments, “on passing a galvanic current from the margin of the lungs to the insertion of the tube in the trachea, the fluid rose quickly, but gradually, nearly two inches; sank speedily on breaking the contact; again rose upon completing it; but fell slowly when the current was continued for some seconds;” *i. e.* when the irritability of the tissues was temporarily exhausted. Temporarily, I say, for on waiting two or three minutes between each application of the galvanism, the liquid was raised again and again for upwards of an hour; till, in fact, the organic life was extinct. Is not all this exceedingly like the behaviour of parts acknowledged to be muscular, under similar influences?

The phenomena were not occasioned by any general shrinking of all the pulmonary issues. For when the lungs were cut across by sharp scissors, at right angles to the air-tubes, and the open sections of these tubes were galvanized, they were *seen* to contract to one-half of their former diameter; and even to become smaller than that.

The contraction was the most distinct in the middle-sized tubes, they being about the bigness of a straw; but it was sensible enough in the trachea, which was sometimes so far reduced in dimensions, that the ends of its cartilaginous rings came together.

A foreign experimenter, M. Valentin, carries us a step nearer to the full solution of this interesting question. He found that the rings of the trachea could be made visibly and distinctly to contract, *by irritating the par vagum*.

The natural function of the contractile fibres is probably (as Dr. William Gairdner suggests) that of gradually propelling outwards, by a kind of peristaltic movement, the mucous secretion which is constantly oozing, in small quantities, into the smaller air-tubes.

Upon the whole we may safely conclude that asthma is one of the spasmodic disorders of the excito-motory system of nerves. I believe, moreover, that, as in most other disorders of the same class, the spasm may be of centric, or of eccentric origin. In the eccentric form, the par vagum is doubtless the afferent nerve; and the impression it conveys to the medulla oblongata is reflected, through associated motor nerves, upon the bronchial muscles. The centric variety results from a similar impression originating in the nervous centres: which respond, mysteriously, to certain feelings of the mind.

I have never had a favourable opportunity, since I became aware of the value of auscultation, of listening to the sounds of the breathing during a paroxysm of pure spasmodic asthma. But they who have enjoyed such opportunities declare that no respiratory murmur, or very little indeed, can be heard. And an attentive inspection of the outside of the chest shows, that amidst all the tugging and heaving for breath, the expansion of the thorax is very limited. The patient cannot open his lungs as it were: and what air does get in, has a difficult and narrow passage, as the wheezing noise demonstrates. Laennec affirms, that if the patient, after holding his breath nearly as long as he can, attempt a quiet and gentle inspiration, the spasm may be often overcome as if by surprise; and, for a few seconds, the entrance of the air into the cells may be heard in a clear and even puerile sound. If this be true, it is a strong additional proof that the obstruction to the admission of air was really owing to a tonic contraction of the little muscular fibres of the bronchi and their ramifications.

The hereditary nature of asthma is perfectly consistent with the same theory. It is one of the maladies which are distinctly transmitted — the disposition to them, I mean — from parents to children. And, like other spasmodic disorders, it facilitates its own return. When it has once occurred, it seldom fails to happen again and again.

But though I believe, for the reasons I have now mentioned, that asthma, in the restricted sense of that term, is purely a spasmodic affection; yet I know also that it is very frequently indeed combined with organic alterations within the thorax. These changes of structure are to be regarded as so many strongly predisposing causes. They induce a readiness to take on spasmodic action: and some of them are perhaps aggravated, or even produced, by the fits of asthma, upon which they afterwards react injuriously. Judging from my own experience, I should say that genuine uncomplicated spasmodic asthma was *rare*.

The organic diseases with which spasmodic asthma is often found connected, are principally emphysema of the lungs, and structural changes in the heart and great blood-vessels. It is extremely probable that the first step towards the production of the spasm, consists in some altered condition of the *circulation* through the lungs. The chilliness of the surface, and the sensation of want of air, make it likely that the blood accumulates in the lungs at those times: that there is congestion of the membrane, as well as spasm of the circular fibres. And it would seem that, in the humoral asthma, the congestion is relieved by a copious secretion of mucus; and that, with the congestion, the spasm also subsides and disappears. You will observe that very generally the paroxysms come on during the first sleep: at which time, as Dr. Alison has suggested, "the blood is perhaps in fullest quantity, its movements slow, and its congestion in internal parts easiest, because it is least solicited to the organs of sense or locomotion." But there seems to be another reason for this remarkable circumstance. Respiration is mainly an automatic act; yet it also obeys the will. During sleep this moderating influence of the will is suspended. Those changes of posture, and those voluntary alterations in the rate of breathing, which are wanted to balance and correct the commencing derangement of the pulmonary circulation, and

which are prompted at once during the waking state, do not occur; until at length the derangement reaches that pitch at which it provokes spasmodic contraction, and rouses the sufferer.

This same congestion, leading to spasm, sometimes passes into a slight form of inflammation; and we have symptoms of bronchitis. And these symptoms may remain manifest even during the intervals of the paroxysms.

Many of these asthmatic patients have just healthy lung enough to breathe with, in tolerable ease and comfort, under ordinary circumstances; and dyspnoea is brought on whenever even a slight additional demand upon the respiration any how arises. Hence, as I stated before, flatulent distension of the intestines, undue repletion of the stomach by an excessive meal, the recumbent posture, all of which cause pressure against the under surface of the diaphragm, may suffice to bring on the fit. Hence also, probably, in part, its frequent occurrence in the night-time.

In like manner, any extraordinary task imposed upon a heart which is barely equal to its functions while the body is in repose, may induce a paroxysm of asthmatic dyspnoea.

Asthma is a disorder which is incident to both sexes, but it is much more common in men than in women. It is incident to all ages also; but it belongs more to adolescence, and to the middle portion of life, than to its extremes. It is not, I think, a common disease prior to the age of puberty: yet instances of it do occur at an earlier period than that. I have lately seen a boy of eight or nine, who has had several well-marked attacks of pure asthma. Nor does it often *begin* to show itself in old age. Sometimes, after plaguing the subject of it for several years, it leaves him altogether. The chronic dyspnoea, with occasional irregular exacerbations, which is so frequent a disorder among old people, and which always depends upon organic disease, is not to be confounded with true asthma. It is said that asthmatic persons are exempt from phthisis: and I understand that one physician in this town, who announces that consumption is curable, maintains the doctrine of the incompatibility of phthisis and asthma; and endeavours to bring about the latter, that he may protect his clients from the former. It *may* be that persons affected with genuine asthma seldom become the victims of pulmonary consumption: but I am sure the rule is not universal. One of my earliest friends had from time to time, while we were school-fellows, and long afterwards, the most exquisite fits of spasmodic asthma. At length, when he was between thirty and forty years old, they wholly ceased: whereupon he greatly congratulated himself. But they only yielded before a worse disease. He began in a few months, to spit blood: and in a few months more he died of well-marked phthisis. Our lamented principal, the late Mr. Rose, afforded another sad example of the same sequence. And I have known two or three families in which one individual was subject to asthma, while others were scrofulous and phthisical.

The exciting causes of the asthmatic paroxysm are manifold; and some of them curious. They seem to be reducible to two classes. 1. Particular states of the atmosphere, which irritate or offend the mucous surface of the air-passages: or rather, some of the fibrils of the *par vagum*. 2. Certain subtle influences which affect in a peculiar manner the nervous system. All the known exciting causes of catarrh are therefore likely to bring on attacks of asthma in the predisposed. But there is a singular caprice in asthmatic patients in this respect. Some persons, subject to the disorder, are unable to breathe in the thick smoky atmosphere of London; require a high and clear situation; and respire easiest in the difficult air of the keen mountain-top." Others can nowhere breathe so comfortably as in low moist places: in some of the streets by the water-side in the city, for instance. The friend to whom I lately alluded lived at Newmarket, a most exposed and bleak spot. But if he left it, and attempted to sleep in a strange place, he never was certain that he should not be assailed in the night by his well-known enemy. So that there were towns in which, after experiencing the effect of their atmosphere, he dared not sleep; and there were others in which he knew he might go to bed in security. It would have been difficult, I believe, to point out any essential difference between some of those localities. His lungs, however, formed an infallible eudiometer. Another college acquaintance of mine, much tormented by asthma, is equally sensible to these inscrutable influences. Two inns in Cambridge are named respectively the Red Lion, and the Eagle. He can sleep in one of them, and not in the other. Nay, he is thus variously affected

within much narrower limits. He assures me that, when in Paris, he never escapes a fit of asthma when he attempts to sleep in the back part of Meurice's Hotel, and never suffers when he sleeps in a front room. Dover Street suits him; Clarges Street does not. He cannot rest in Manchester Square. This he attributes to its being built upon piles. Whether it really has such a foundation I do not know. And agencies still more slight and subtle are enough to set the springs of these seizures in motion. The mere absence of light, for instance. Laennec tells of a man who invariably was roused from his sleep by a paroxysm of asthma, if his lamp were extinguished; or if his chamber-door were shut. The consciousness that the customary preventive remedy was not at hand, has, apparently, sufficed to bring on a fit.

There are many persons who never fail to become asthmatic if they inhale certain effluvia. Particles of ipecacuan floating in the atmosphere, or (what is perhaps the same thing) its mere odour, are insupportable to many. They are thrown into a paroxysm of dyspnoea if they enter an apartment where that drug is under preparation. I think I mentioned before a certain laboratory-man at St. Bartholomew's Hospital who possessed this peculiar and inopportune susceptibility: he was obliged to fly the place whenever ipecacuan was about. Most persons, probably, who have had much experience in druggists' shops, are acquainted with similar examples: so that the influence of ipecacuan in exciting fits of difficult breathing, resembling asthma, is undoubted, and common to *many* constitutions. We might as well speak of ipecacuan asthma, as of hay asthma, which is a precisely analogous affection. Dr. Marshall Hall calls attention to the familiar but interesting fact, that the same drug, ipecacuan, acting upon the *gastric* branches of the par vagum, excites the reflex spasmodic act of *vomiting*.

I have said, that the relief afforded by antispasmodic remedies affords presumptive evidence of the spasmodic nature of these attacks. If asthma supervene upon manifest bronchitis, or if there be any signs of congestion about the head, it may be prudent to abstract blood: but this measure will not in general be requisite; and when not requisite, it should be avoided: for whatever tends to debilitate the patient, or to lower his vital powers, tends, at the same time, to augment his susceptibility to the exciting causes of the disease. The dyspnoea may frequently be moderated or altogether assuaged by some form of narcotic. Now opium is the narcotic to which we most trust for the mitigation of spasm in general: and opium *is* of vast service in paroxysms of asthma. But there is also another of the vegetable narcotic substances which has obtained an especial repute for its effect in quieting the difficult breathing in these cases; and that is *stramonium*. This herb, the *Datura stramonium*, and another species of the same genus, the *Datura ferox*, had long been employed in India as a remedy for asthma. And when it was introduced into this country, about the beginning of the present century, it was cried up as a specific; and everybody who called himself asthmatic began to smoke stramonium: for that is the way in which it has been chiefly employed. The leaves and stalks are cut and put into a pipe, and smoked like tobacco. Stramonium cigars, too, are fabricated. The smoke descends, of course, into the lungs: and when the saliva is swallowed, the remedy is introduced into the system in that way also.

Stramonium thus used, sometimes fails altogether: sometimes calms the paroxysms like a charm. The late Dr. Babington told me of a patient of his who had been grievously harassed for a series of years, by asthma, but who declared to him, after he had made a fair trial of stramonium, that he no longer "cared a fig" for his asthma; which he could always stop in a moment. So a Mr. Sills, in a collection of communications relative to the *Datura stramonium*, published in London in 1811, states, that he had been a great sufferer from asthma: that the fits usually continued, with short interruptions, from thirty-six hours to three days and nights successively; during which time, he had often, in the seeming agonies of death, given himself over, and even wished for that termination of his miseries. But having at length discovered the virtues of stramonium, he uses this strong language:—"In truth, the asthma is destroyed. I never experienced any ill effects whatever from the use of the remedy; and I would rather be without life than without stramonium."

This, then, is an expedient which it will always be well to suggest, for relieving the urgent distress of the paroxysm of asthma. But most patients subject to that complaint, try it of their own accord. We have still to learn why it is so efficacious

in some cases, and so entirely useless, or even hurtful, in others. This probably depends something on the presence or absence of organic disease in the lungs or heart; but more accurate observations are wanted on this point.

Some of the animals upon whose lungs Dr. Williams performed his experiments had been killed by poison. In two instances stramonium was the poison employed; and it is interesting to know that scarcely any contraction of the air tubes could be produced by the galvanic apparatus. The trachea, at the same time, was lax. It is requisite to notice the condition of the windpipe in these experiments; for the same want of contractility would be *apparent*, supposing the parts to be already in a state of tonic spasm, from the operation of the poison. This seems to have been the case when conium was used: the fluid in the glass tube indicated scarcely any compression of the air contained in the lungs; but then the windpipe was so far contracted, that the extremities of its rings met. There was but little contractility, and a lax trachea, after death by belladonna; and after death by the meconate of morphia. The contractility was slight when life had been destroyed by strychnia; but the condition of the trachea is not reported.

Experiments of this kind appear to be well worthy of careful repetition.

It has been suggested, as one clue towards determining the particular kind of case to which the stramonium is applicable, that it succeeds when it causes *expectoration*; and not otherwise. But I doubt about this. The relief is sometimes too sudden to admit of its being so explained. Sir John Forbes quotes the following passage of a letter from an old and intelligent asthmatic to himself. "Smoking tobacco or stramonium is sure to give relief, if it produce expectoration; and it will generally do so if, the moment I awake (*i.e.* in the incipient paroxysm), I begin to smoke, and continue to do so for three or four hours. Smoking, I am able to say, after fifteen years' practice, and suffering as much as mortal can suffer and not die, is the best remedy for asthma *if it can be relieved by expectoration*. I have been in the hands of all the doctors of the place for fifteen years; and still I say, *smoke*."

Of calming vapours thus applied by inhalation to the very parts affected—and operating either immediately upon the nervous fibrils there distributed, or mediately after admission into the blood—the vapour of chloroform is likely, in respect of its soothing power, to supersede all others. Inspired in moderate quantity, far less than is requisite to produce general insensibility, it has already been found of singular efficacy in allaying at once the spasmodic distress of an asthma-fit. But it is likewise manifest already that this is a remedy too potent and subtle to be entrusted to the discretion of the patient himself. We must know more about it than we have yet learned before it can be safely self-prescribed, or self-administered.

Less hazardous certainly, less costly, less difficult therefore, of attainment by the many, but less sure also and less rapid in its result, is the respiration of air impregnated with the fumes of burning nitre. The mode of effecting this is simple enough. Pieces of blotting-paper, each as big as one's hand, are to be dipped in a saturated solution of the nitrate of potash—and then dried. One of these papers, being placed on an earthenware plate, and ignited—the fumes presently diffuse themselves throughout the room; and within a quarter of an hour their influence, in many cases, is rendered evident, in "clearing the passages, and gradually opening the air tubes."

The compound spirit of sulphuric æther, Hoffman's anodyne, swallowed into the stomach, is an old and improved remedy. Combined with opium, I have found it of great service in tranquillizing the breathing in asthma. And in one case, which was under my care for some time, I made comparative observations respecting this opiate and stramonium. Both gave much relief; but stramonium the most, and most certainly, *if it were applied in time*. This patient was in the habit of being roused from sleep by the supervention of the paroxysm: and if he had the means of lighting his pipe *instantly*, he could stave the fit off. But when once it had attained its full intensity, he was *unable* to smoke. Under such circumstances, he could swallow the morphia and æther; and the effect of this depended also in a great measure upon the period at which it was taken. It would stop a commencing paroxysm, but had little influence over one that was fully formed. The chloric æther is a more agreeable, but less potent remedy than the sulphuric.

Of late the lobelia inflata has been much lauded for its beneficial operation upon dyspnoea of all kinds, and upon asthma in particular. I believe its virtues have been

overrated. It sometimes, like stramonium, has an almost magical effect; but frequently it fails to do the smallest good; and I know that sober practitioners who have employed it more than I have, have thought that it may occasion dangerous symptoms. Of my own knowledge, I have nothing worth communicating to you of this drug.

Strong coffee is a common domestic remedy for asthma. The friend and schoolfellow already mentioned used to take it in considerable quantities, and, he assured me, with very great benefit. It is a safe, and simple, and grateful remedy, and has numerous testimonies from medical practitioners in its favour. But it is much less sure than the stronger narcotics.

Ipecacuan, which is so frequently the *cause*, has also been recommended for the cure of asthma; and a host of drugs besides, with which I have no intention to weary you. Among the rest, the application of galvanism was once in great vogue. In the only patient who ever tried it under my own eye,—and he insisted upon being galvanised when his fits were quite absent,—the galvanism brought one on immediately.

If we can shorten or mitigate the paroxysms we do our patient a most essential service, and spare him a great deal of suffering. And during the intervals between the fits, we must endeavour to prevent their recurrence.

For this purpose, I can only just hint at the principle on which we should go. In the simple form of the complaint, when it is apparently uncomplicated with organic disease, we must caution the patient against whatever has a tendency to disturb his general health. He must be temperate in all things; he must be careful in regulating his digestive organs; he must set himself to discover what localities suit him best; and avoid those which experience has shown to disagree with him. And if any one remedial measure be likely to fortify him against his malady, I believe that measure will oftenest be found in the shower-bath, employed in the way which I formerly recommended.

When the familiar preliminary feelings have threatened an attack of asthma, I have often, I think, prevented its accession by causing the patient, just before he addressed himself to sleep, to swallow a pill containing a quarter of a grain, or half a grain, of the extract of stramonium.

If the asthma occur in connexion with any obvious pulmonary or cardiac disease, we must, in addition to the means I have now been adverting to, apply ourselves to the mitigation of such superadded disease. And in respect to this I have nothing more to offer.

I go next to the morbid conditions of the *œsophagus*, so far as they concern the physician; and these morbid conditions are not many. The *œsophagus* lies partly in the chest, and partly in the belly, and therefore may very properly close the subject of thoracic diseases, and introduce those of the abdomen.

The *œsophagus* is less liable to disease than any other part perhaps of the alimentary canal. It differs somewhat in structure (as you know) from all other parts. Its mucous membrane is provided with a thick epithelium, which extends a little beyond the cardiac orifice of the stomach. Beneath lie a dense web of areolar tissue, and two layers of muscular fibres; the one layer being disposed circularly around the tube—the other longitudinally, in the direction of its axis. In some cases there are pouches found in the sides of the *œsophagus*, formed apparently by a kind of hernia of the mucous membrane, between the separated fibres of the muscular coat. These are not common, however, and scarcely worth mentioning, except that their existence has been supposed to have a possible connexion with a curious phenomenon, peculiar to some persons; the power, viz., of *ruminating*; the power of bringing into the mouth again, by a voluntary effort, food which has been for some time swallowed, as cows, and the rest of the *ruminantia* do. There are but few individuals of the human species who possess this faculty; there are but few who have appendices to their *œsophagus*. Whether the phenomenon in question belongs to these last few has never been determined; but as the possible connexion of the two circumstances has been suggested, it is well for you to be aware of it, that you may refute or verify the notion, in case you ever have the fortune to examine the dead body of a person who had the power of ruminating.¹

¹ An instance of the concurrence of a large pouch in the *œsophagus*, with frequent regurgitation of undigested food, has recently (1847) been detailed, in the 30th volume of the *Medico-Chirurgical Transactions*, by Mr. Worthington of Lowestoft: who refers to another example of a like combination, recorded by Mr. Ludlow, of Bristol, in the 3d volume of the *Medical Observations and Inquiries*.

The covering of cuticle protects the œsophagus from the injurious influence of matters passing over it, which might otherwise be hurtful. The morbid state for which we are most often consulted is stricture, *actual* or *spasmodic*. I believe that the œsophagus is very little subject to inflammation, except from mechanical violence or chemical injury. I have seen a few cases, however, in which I inferred a spontaneous inflammatory condition of the tube, from the symptoms complained of; which were a sense of heat and pricking exactly in the course of the œsophagus, felt between the shoulders, and in front precisely in that track (the patients said), where a potato, swallowed too hot, gives pain while it is descending into the stomach. With these symptoms there was some degree of dysphagia, not explained by any thing visible in the throat or pharynx; and some degree of fever. In all the instances of this kind that I have met with, the symptoms have yielded in a few days to abstinence, purgatives, and the application of leeches along the track of the œsophagus.

This part of the alimentary canal often suffers severe injury from the deglutition of certain poisons, especially the corrosive poisons; the strong mineral acids, for example; or the caustic alkalis. We have, in the museum of the College, some very interesting specimens of the effects of these destructive substances. Sometimes, when the quantity of the poison has been small, and its transit rapid, the cuticular lining alone of the gullet is destroyed. It is shrivelled up, broken into fragments, abraded. At other times, the subjacent textures are affected, and ulceration takes place, which at length heals, and leaves a permanent, and generally a progressive constriction of the œsophagus: and sometimes the whole of the internal membranes slough away, and are discharged in one continuous tube, from the mouth; and yet the patient survives for some time. My colleague, Dr. Wilson, had a case of that kind. The patient, a young woman, swallowed about a table-spoonful of oil of vitriol. A week afterwards, she brought up, during a paroxysm of choking cough, a complete cast of the gullet, with ragged ends; or rather the gullet itself. Some of the muscular fibres of the œsophagus were plainly visible on the outside of this tubular slough, in its recent state. She lived eleven months afterwards, swallowing all that time with difficulty and pain, and subsisting on slops and soft food. Yet at one period she certainly gained flesh. After her death, the channel, as it remained after the injury, was taken out of the body and examined. They are both before you;—the original slough, and the ultimate gullet. The latter was formed by a surface which consisted of an irregular cicatrix. The tube was contracted considerably in the lower two-thirds of its course.

When patients have suffered inflammation and ulceration of the œsophagus from these causes, and do not perish at the time, they are very liable indeed to have their existence abridged by the occurrence of stricture of the gullet, which goes on slowly increasing, until no food can pass it, and then of course the patient dies of starvation. I show you here an œsophagus taken from a man whose case I had opportunities of observing from the beginning. He was under the care of Dr. Macmichael, in the Middlesex Hospital. He was brought there in November, 1830, having swallowed, half an hour before, a solution of the impure carbonate of potass, which had been made for the purpose of cleaning paint, and which he had mistaken for beer. Not more than a table-spoonful passed the fauces, and probably none of the poison reached the stomach. He suffered severely, and was in considerable peril for several days, in consequence of inflammation of the fauces and epiglottis; but this gradually subsided, and he went out apparently well. From what I knew, however, of the result of such cases, I ventured to predict that this man would, sooner or later, come back with stricture of the œsophagus. He had always pointed out a spot about half way down the sternum, where he said the oil of tartar had caused him extreme pain, at the very first, and below which he had not felt it.

Accordingly, I was not surprised to see the poor fellow at the hospital in February, 1834, attending as an out-patient. He came there, he told me, because in eating some soup, he had accidentally swallowed, without chewing it, a piece of carrot, which lodged in its way down, and which it became necessary to push onwards into the stomach by means of a probang. Morsels of food had stuck in the same spot before, and it was the very spot where he felt the effects of the caustic at the time of the accident.

He looked tolerably stout and healthy; but said that, since swallowing the potass, he had never been the man he was before.

He continued to make his appearance, from time to time, at the hospital, with similar symptoms, till the 5th of last Décembre (1836), when he was brought there insensible, and evidently dying. We could obtain no satisfactory account of his recent symptoms. He had the mark of a blister, however, on his left side; and upon closely examining him it was plain that that side was full of fluid. It was perfectly motionless in respiration; it was palpably larger than the right side; it yielded everywhere a dull sound on percussion; and no vesicular breathing whatever could be heard there by the ear. The respiration on the right side was puerile; and the beating of his heart, with a systolic bellows-sound, was audible on the right of the sternum.

Although I was certain that the left pleura was full of liquid of some kind, I did not have the thorax punctured: because, in the first place, he was manifestly *in articulo mortis*, and I thought that his death, which was certain, might be attributed to the operation; and secondly, because he was not dying of *suffocation*. His breathing was not laborious or much distressed; but he was dying of coma, and his extremities were already cold, and his pulse was fluttering. I conjectured that an ulcer of œsophagus had made its way into the pleura, and caused inflammation there. But my conjecture was wrong.

I will mention the main particulars of the examination of the dead body, because the case was, in several respects, an interesting one.

There was a considerable quantity of serous fluid in the meshes of the pia mater, beneath the arachnoid; and there was some liquid of the same kind in the lateral ventricles. No other diseased condition could be detected in the brain. The effusion was sufficient, supposing it to have come on suddenly, to explain the coma.

I had the ribs sawn away on the right side, leaving their cartilages attached to the sternum; and then we saw plainly that the heart and mediastinum were thrust over, about four inches by measurement, beyond the mesial line on the right side. The body was on its back. It was easy to perceive how a pleura thus full of fluid must oppress the lung of the other side, especially when assisted by the force of gravity. The left cavity was distended by a greyish coloured and most offensive fluid, of the consistence of gruel; the pleura pulmonalis was covered by a layer of coagulable lymph; and the lung was flattened against the vertebral column. We could not discover any communication between the cavity of the pleura and the œsophagus or air-tubes.

Fig. 99.



Stricture of the Œsophagus.

About the middle part of the œsophagus there was a distinct stricture, occupying about half an inch of the tube. Through this portion it was impossible to push one's little finger; which elsewhere found a loose and ready passage.

In this case, the man did not die of the stricture; but he would have done so had not another disease carried him off. I do not know why the constriction, after it has once taken place, should go on continually increasing; yet it seems to be so. In his *Surgical Observations* Sir Charles Bell mentions three cases like that just described. In one of them, where soap-lees had been the substance swallowed, death took place by starvation from stricture of the gullet, twenty years afterwards; and Sir C. Bell had no doubt that the stricture had originated in the chemical injury inflicted by the soap-lees.

When the symptoms of stricture come on in these cases, physic can do almost nothing. Surgeons pass bougies into the gullet, and attempt to dilate the strictured portion, or to prevent any further narrowing. But this expedient is usually of temporary benefit only; and the patient dies at last of inanition. His miserable existence may

perhaps be protracted a little, by injecting nutritive enemata into the rectum. Sometimes the œsophagus ulcerates through, and a communication is formed between it and the neighbouring parts.

But the œsophagus, like the urethra, and like the bronchial tubes,—like every canal, indeed, in the living body that is surrounded by circular muscular fibres,—is liable to temporary constriction and closure, by the spasmodic action of its own muscles; and this affection is, of course, a far less formidable one than the last.

Patients who are subject to spasmodic stricture of the œsophagus experience occasionally, in some point or other of that tube, a sensation as if there were a knot; or sometimes a feeling as if some solid substance were ascending from the stomach towards the pharynx. If they happen to be then engaged in eating, the morsels of food, after mastication, readily pass the pharynx: but, at a certain distance down the gullet, they stop, and occasion pain, which is felt between the shoulders, or distinctly in the passage itself. Great anxiety and distress accompany this stoppage: and the food is often ejected by a reversed action of the œsophagus.

The symptoms, in fact, are identical with those which result from permanent stricture of the gullet, except that *they* are *not* permanent. When the stricture is organic and abiding, the symptoms occur during or after every meal. When it is simply spasmodic, they come and go, capriciously, we often cannot conjecture why or wherefore, after the fashion of other spasmodic ailments.

Spasmodic stricture may be independent of any disease of structure in any part of the body; but it is of some importance to be aware that it may also be symptomatic of very serious organic changes. Mr. Mayo relates the case of a young man “who had difficulty of swallowing; he could get down liquid food only; and that not without an effort. A bougie being introduced, some resistance was found at the upper opening of the œsophagus, but it yielded; the resistance was spasmodic, and depended upon neighbouring irritation caused by ulceration in the interior of the larynx. The use of the bougie for a few days, with appropriate remedies to the larynx, removed the dysphagia.”

The purely spasmodic cases occur principally in persons of a moveable constitution; in young women whose uterine functions are deranged, and who are liable to hysteria. The remedies for hysteria will prove remedies for the spasm of the œsophagus also. And whatever is calculated to excite ordinary hysterical symptoms, whatever tends to render the system weak and irritable, will tend to aggravate the œsophageal stricture. I alluded to such cases in a former lecture. I give you another, related by Sir Benjamin Brodie. A lady consulted him, unable to swallow the smallest morsel of solid food; and swallowing liquids not without great difficulty. The symptoms had been coming on upwards of three years. A full-sized œsophagus bougie being introduced, entered the stomach without meeting the slightest impediment. This lady's face was pale and bleached: her feet were œdematous. She had long laboured under internal piles, from which repeated discharges of blood had taken place. Under the use of remedies which relieved the piles and the bleeding, the difficulty of swallowing went away.

It is a singular, and it might, if more frequent, be a puzzling circumstance, that very nearly the same symptoms which occur when the œsophagus is permanently or temporarily constricted, happen also sometimes under a totally opposite condition of that tube; I mean its dilatation into a large, inelastic, inert bag. One remarkable example of this I witnessed, in a woman whom I attended in conjunction with Mr. Mayo, some years ago, in the Middlesex Hospital. The case has been fully described by Mr. Mayo, in the third volume of the *Medical Gazette*; and more briefly in his *Outlines of Pathology*. She was thirty-three years old. She was brought to the hospital in a state of extreme feebleness and emaciation. They who brought her said that for the preceding month she appeared to keep down nothing. What she took as food seemed to her to stop in the gullet; and, after a few minutes, it returned. A large œsophagus bougie passed readily into the stomach. She could swallow liquids more easily than solid food. When she took a small quantity it did not feel to her as if it reached the stomach; and in three or four minutes it was invariably rejected. The vomiting was not preceded by nausea, although in its progress it had the appearance of ordinary retching. She craved for food and drink, and seemed literally starving. The complaint had begun ten years before, during her pregnancy, and had

gradually got worse. The belly was so shrunk that the umbilicus was not more than an inch distant from the spine. There was no enlargement or hardness about the stomach; no particular tenderness on pressure of the epigastrium; nor any uneasiness there. She died, utterly attenuated, sixteen days after her admission.

The stomach was found small, and contracted at its middle to the breadth of an inch and a half. The upper part of the duodenum was but half the ordinary size of the ileum. The œsophagus I show you, turned inside out.

It was enlarged to an extraordinary degree of amplitude, as you perceive. At and near each extremity it was healthy, and of its natural size. Intermediately the lining tunic was thickened and opaque, with numerous depressions in it. The muscular fibres, which appeared to have multiplied with the expansion of the canal, were of their natural colour and thickness.

Here is another preparation: a dilated œsophagus with cancerous degeneration of the cardiac orifice of the stomach. I do not know its history; but the mechanism of such dilatation is intelligible enough. The food, unable to pass out of the gullet into the stomach—or passing slowly and uncertainly—the tube behind it is habitually distended, and loses at length its proper contractility. I saw last summer, in consultation with Mr. Mayo, an old gentleman, of seventy, who, for two years, had experienced difficulty in getting food into his stomach. He would eat a few mouthfuls very well; and, then, of a sudden, the next mouthful, after passing the pharynx, would stop just short of the stomach; and a sensation of swelling would arise in the lower and middle part of the œsophagus; and presently up the mouthful would come again. Sometimes, by waiting quietly a little while, the morsel would go on; sometimes he could wash it forwards by a gulp or two of drink: but if once the food got fairly into the stomach he had no further trouble with it. This gentleman had no discoverable disease of the heart or lungs. He gradually grew worse. At last he began to vomit grumous matters, resembling coffee-grounds, and soon died. He was at some little distance from London at the time, and the body was not (I believe) examined. I have no doubt that he had malignant disease of the cardia; and I think it probable that his œsophagus was dilated. I had a female patient about two years ago in the hospital with very similar symptoms; and her stomach was found to be full of cancerous disorganization. The state of her gullet is not recorded. We are apt, in such cases, to satisfy ourselves with ascertaining the gastric disease, without carefully examining that part of the alimentary canal which lies above.

For maladies like these medicine has no cure. Opiates may give comfort, and promote the euthanasia: and that is all.

[During the period of infancy, the mucous membrane of the œsophagus is not unfrequently the seat of inflammation, of a diphtheritic, follicular, or erythematic character. The disease, however, is not very often detected during the life-time of the patient, in consequence of the symptoms by which it is accompanied being those common to many of the affections of the earlier months of existence. In fact, almost the only symptom in many cases of œsophagitis in the infant is frequent vomiting;—immediately after the deglutition of drinks, or of aliments of any kind, these are discharged by vomiting, without having undergone any change. The infant usually refuses the breast and fluids generally, evidently from the pain excited by the act of deglutition; occasionally, however, he will swallow, without much difficulty, half solid aliment when given in small quantities at a time. If the stomach be affected with inflammation at the same time with the œsophagus, the undigested food and drinks discharged by vomiting will be, occasionally, mixed with the morbid secretions of that organ. The disease is seldom attended by any degree of febrile excitement.—The infant is, usually, fretful and restless, and becomes rapidly emaciated. Whenever an infant rejects the breast, or any species of fluid aliment, vomits frequently, and discharges whatever he swallows in the same state nearly as when it was taken, it is the duty of the practitioner to institute a minute examination, in order that he may detect the inflammation of the œsophagus if it exists. It is probable that pressure along the whole track of the œsophagus will excite the cries of the child, when we can have no hesitation as to the diagnosis. If this does not happen, we may nevertheless be tolerably certain as to the existence of the inflammation, if the infant be labouring at the time, or has very recently been labouring, under one or other of the forms of stomatitis. If the

inflammation is confined entirely to the mucous membrane of the gullet, and is not aggravated by improper food or drinks, it will often subside, after a few days, spontaneously; but when it is connected, as is very generally the case, with inflammation of the stomach or intestines, it is an affection difficult to control, and usually fatal. In perhaps the majority of cases, œsophagitis in the infant is preceded by erythematic, diphtheritic, or follicular stomatitis.

The frequency with which inflammation attacks the mucous membrane of the œsophagus in the infant, is evidently due to the predisposition which exists, at this period, to disease of all the mucous surfaces from their constant state of hyperæmia. It is liable to be excited by too much or improper food, or that which is too hot or stimulating. In infants confined to the breast, it may be occasioned by the bad quality of the mother's milk, or by some temporary morbid change occurring in the latter—and which may result from a variety of causes. Œsophagitis has been known to result from the reprehensible nursery practice of administering to infants infusions of spices, or even gin and water, for the purpose of expelling wind, or to relieve the colicky pains with which they are so frequently affected. — In common with all the affections of the mucous membrane in early life, inflammation of the œsophagus occurs most frequently, and is the most difficult to manage in infants exposed to the baneful influence of personal and domestic filth, and a confined and impure air.

The pathological changes detected after death in cases of œsophagitis, are stated by Billard to be vivid redness, with destruction of the epithelium, of some portion of the mucous membrane of the œsophagus, especially of its upper portion; in some cases, a curdlike exudation, more or less extensively diffused, is present; in others follicular ulcerations; a separation of portions of the epithelium, often in large shreds, is occasionally met with; numerous red or dark-coloured striæ occurring where the epithelium is not destroyed; large, irregular eschars of a black colour, with intervals of deep, bright-red excoriations, are occasionally met with, and, in some cases, gelatinous softening of nearly the entire thickness of the œsophagus.

When an infant is affected with œsophagitis, it should not be allowed to take any stimulating or hot drinks—it will be proper, however, to supply it, at short intervals, with a small quantity of some perfectly bland mucilaginous fluid, as gum water, or an infusion of the pith of sassafras, or of the inner bark of the slippery elm, given cold; the throat should be covered externally with a soft, emollient poultice, especially when the inflammation of the œsophagus has succeeded to stomatitis; a few grains of calomel may also be administered, and followed by injections of milk with the addition of sugar. If it be found necessary, the strength of the infant may be supported by injections of milk and broth, or milk with rice flour, tapioca or arrow root. We have derived advantage in cases presenting the symptoms of œsophagitis, from a few leeches applied along the sides of the neck, and internally a teaspoonful, every three hours, of a solution of four grains of the acetate of lead in one ounce of water, with the addition of a few drops of strong vinegar. — C.]

LECTURE LXV.

Diseases of the Abdomen; sometimes difficult to identify. Method of investigating these diseases; by the eye, the hand, the ear. Inflammation of the Peritoneum; its symptoms; and causes. Puerperal Peritonitis. Peritonitis from Perforation.

I AM about to consider the diseases of the abdomen. The organs contained in this cavity of the body are not vital organs in the same sense in which the brain, the heart, and the lungs, are vital. That is to say, the *functions* of the abdominal viscera will bear to be suspended for some considerable time, without the extinguishment of life. But these parts are subject to numerous diseases, some of which are apt to be quickly fatal, and others carry with them a vast amount, and very severe kinds, of suffering.

The parietes of the fore-part of the belly being soft and flexible, you might naturally suppose that the physical morbid conditions of the organs they cover would submit themselves to an easy diagnosis; that the sense of touch, exercised through these yielding walls, would detect alterations of bulk, of form, or of place, in the subjacent viscera, with much facility and exactness. But the truth is, that since the discovery of the method of auscultation, the diseases of the abdomen are much more hard to discriminate than the diseases of the thorax. The reason of this is to be found in the number and complexity of the parts contained in the abdomen; the loose manner in which some of them are packed; and the consequent readiness with which they pass out of their proper and natural situations. It is necessary that I should say a few words, but I shall not detain you long, respecting the *mode of examining* the abdomen, with the purpose of investigating its diseases.

In the description of symptoms, we are often obliged to speak of particular portions of the abdomen: and it will be of future convenience to us if we make ourselves acquainted, at starting, with such a superficial map, marking out the topography of the belly, as I exhibited to you some time since, in reference to the chest. Draw a horizontal line round the body, touching the extremity of the ensiform cartilage; and this will form the superior boundary of the abdomen, thus roughly defined for practical purposes. Draw another such line round the body, horizontally, touching the lower edge of the last false ribs: and a third touching the crest of each ilium. We then have three horizontal *zones* formed. These must be further divided by vertical lines: one on each side from the anterior spinous process of the ilium perpendicularly upwards. Each zone will thus be subdivided into three regions. The middle region of the upper zone is the *epigastric* region; on either side are the *hypochondria*. The middle region of the middle zone is the *umbilical* region; the *iliac* regions or the *flanks* lie to the right and left of it. The *hypogastric* region is the middle region of the lowermost zone; and the *inguinal* regions are contiguous to it. This is all the division which is necessary.

Now, independently of the *general* signs of disease that have their seat in the abdomen, we are greatly assisted in many cases by the *physical* signs. I shall take a very brief survey of the modes by which these physical signs are collected. They are derived from the exercise of the three senses of sight, of touch, and of hearing.

The sense of sight supplies, occasionally, very valuable information; and in all serious and equivocal cases we must not dispense with its use. We are not, indeed, to make an ocular inspection of the *naked* abdomen *unnecessarily*: and I hold it superfluous to admonish you that when we *do* avail ourselves of that mode of investigation, especially in the case of females, we are bound to do so with the most careful attention not to offend the patient's delicacy. We may sometimes ascertain all that is required concerning the *movements*, *size*, and *shape* of the abdomen, without removing the under garments.

This rule applies, indeed, to all parts of the body that are ordinarily covered by the dress.

I was lately consulted by a lady, who told me she had, on the rear of her person, a painful boil. She thought any physician ought to be competent to prescribe for a boil, without wanting to see it. But she seemed very ill, and her sister told me that the boil had lasted a fortnight, and was a very large one; so that I was obliged to press for an inspection. And I found—a boil sure enough, but of that gigantic and formidable species which we call *carbuncle*.

M. Rostan relates a case still more in point. Going round the wards of his hospital, he came to an old woman, who was complaining of a severe pain in the abdomen, towards the left iliac region. Her face was flushed, her skin hot, her pulse strong and frequent, her tongue dry; and she was very thirsty. The abdominal pain was exasperated by pressure, and by the movements of the patient. Upon these data, Rostan founded his diagnosis. He concluded that the case was one of acute abdominal inflammation; and he prescribed accordingly; and with befitting energy. One of the pupils, however, lingered behind him: and having removed the woman's chemise, in order to examine the seat of pain, he discovered that all the symptoms proceeded in reality from a very harmless, though troublesome, disorder, *herpes zoster*; what is vulgarly called *the shingles*.

Vestis adempta est,
Quâ positâ, nudo patuit cum corpore crimen.

In the second place, we gather very important intelligence by the sense of *touch*. We learn the existence and the size of *tumours*; we approximate to a knowledge of their quality, whether it be solid or fluid; we determine whether they are moveable or fixed, painful or indolent, hard or soft, smooth or uneven, pulsating or not. We ascertain whether the surface be hot or cold. In order to make palpation most effectual, the patient should be placed in the most favourable posture for its performance, *i. e.* he should lie on his back, with his head a little raised by a pillow, and with his knees up. In this position, the abdominal muscles are relaxed and unstrung: and the patient is to be cautioned not to do anything which may make them tense. Sometimes, in spite of this caution, and in spite, probably, of the patient's endeavours to obey it, the recti muscles remain so tightly contracted as to prevent any satisfactory examination of the parts beneath them. The very occurrence of this instinctive striving against the pressure of our hand may be taken as a ground of suspicion that those parts are not in a healthy state. We must take care, when the muscles are thus obstinately rigid, not to mistake the swelling central portions of the recti, or their well-defined edges, for tumours, or for indications of an enlarged stomach or liver. By a peculiar management of the palpation, we often satisfy ourselves at once of the presence of liquid in the cavity of the peritoneum, or in a cyst: we obtain that sensation which we call *fluctuation*.

The exploration by the sense of touch is very much aided—often confirmed, sometimes corrected—by evidence which addresses itself to the sense of hearing. Sometimes we listen to the natural sounds through a stethoscope: and we may thus decide the important question, whether a pulsating tumour be or be not an aneurism; or the question, sometimes scarcely less important, whether or no a different kind of tumour encloses another living being. But, for the most part, our information respecting the maladies of the abdomen, collected by the sense of hearing, is obtained by listening to sounds which we ourselves produce; in one word, by *percussion*: and mediate percussion, percussion performed through the finger as a ready pleximeter, is *particularly* applicable to the disorders of the abdomen. By this expedient we can tell whereabouts the intestines lie; whether the parts beneath the place percussed be hollow and filled with air, or solid; or, though naturally hollow, distended with liquid. By making the patient change his posture, we are enabled often, through the aid of percussion, to trace fluid effusions hither and thither, when they have changed their relative situation in obedience to the force of gravity; and then we know that they occupy the cavity of the peritoneum. All these points I pass over cursorily, because I must advert to them again when speaking of particular diseases. And I shall proceed, on that account, without further delay, to the consideration of those special diseases.

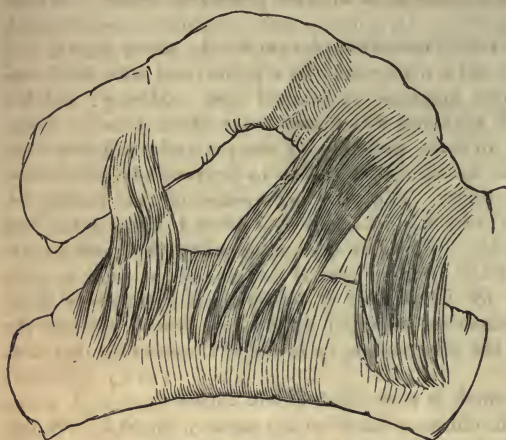
Consulting your convenience, and my own, rather than any scientific order, I shall take, in succession, the several parts and organs contained in the cavity of the belly, and inquire separately into their diseases; inflammatory, organic, and functional. And I begin with the *peritoneum*; the great serous sac which lines and constitutes the cavity of the abdomen, and in which most of its viscera are wholly or partially folded.

Like the serous membranes in general, the peritoneum is very *ready* to take on inflammation, upon the operation of certain exciting causes. Acute inflammation, beginning in one spot, is almost sure to transfer itself to any other spot that happens to lie in contact with the first; and is very apt to extend itself rapidly to the whole membrane. The inflammation tends to the effusion of serum, and of coagulable lymph; it is of the adhesive kind: and its effects are those of distending the peritoneal cavity with fluid—or of gluing its opposite surfaces together so as to obliterate that cavity—or of forming *partial* attachments. In all these respects, the analogy between inflammation of the peritoneum and inflammation of the serous membranes of the thorax—the pleura, and the pericardium—is perfect: and therefore these are points which I shall not dwell upon, except where specific differences arise, from original diversities of structure or of function in the parts affected. I may observe at once, that the morbid conditions which are apt to remain *after* peritonitis, are sometimes, like those which follow pericarditis, *inceptive of further disease*: sometimes, like those of the pleuræ, *final*, and limited to their *immediate* influence upon the health and comfort of the individual; or even *protective* against some worse evil.

Acute inflammation of the peritoneum is characterized by pain in the abdomen, in-

creased on pressure, and attended with fever. But as these symptoms are common to almost all the inflammatory conditions of the parts contained in the abdomen, we

FIG. 100.



False membrane of peritonitis. From a specimen in Dr. Gross' cabinet.

must look for more distinctive circumstances. Cullen defines the disease in this manner: "Pyrexia: dolor abdominis, corpore erecto auctus, absque propriis aliarum phlegmasiarum abdominalium signis." He concludes that it is the peritoneum simply that is inflamed, when the specific symptoms that indicate inflammation of particular organs are wanting. It is not inflammation of the liver, for there is no pain of the right hypochondrium in particular, increased by lying on either side, no pain of shoulder, no jaundice, no vomiting perhaps: neither is it inflammation of the bowels or stomach, there is no disturbed function of the alimentary canal to denote such inflammation.

The pain, Cullen says, is increased when the patient sits up. He might have added, that it is increased also by drawing a long breath, by coughing, sneezing, or straining, and by pressure made with the hand upon the belly. All these circumstances resolve themselves into the same obvious principle; viz., that of pressure aggravating the pain of an inflamed membrane. The erect posture throws the weight of the viscera upon the peritoneum, and tends to stretch parts of it. The pain occasioned by pressure is often excessive: the patient cannot bear even the weight of the bed-clothes. Though the pain is, *at first*, sometimes confined to particular spots, yet it generally soon extends over the whole abdomen, and this is a circumstance of some importance as respects the diagnosis. But *before* the inflammation has become universal, while it is yet restricted to particular spots, the pain is often much increased by pressure made on *other* parts of the abdomen. In truth, in a shut sac of that kind you cannot compress any one part without exercising pressure indirectly upon every other part. The patient cannot sit up, nor, usually, lie on his side; but remains always upon his back: in which position you will perceive that the pressure made by the viscera upon the peritoneum is a *minimum*: is the least possible. He draws up his legs too. And he lies *still*: for *movements* cause pressure, and therefore pain. The descent of the diaphragm in inspiration presses also upon the membrane; and the patient not only complains of the pain thus produced, but, in order to avoid it, gets into a way of breathing by means of his ribs only. So that upon *inspection* of the abdomen, it is perceived that, instead of rising and sinking alternately in respiration, it remains motionless. The phenomenon of *thoracic* inspiration is a symptom of peritonitis. The breathing is necessarily shallow in these cases, and less air being admitted at each movement of respiration, the number of those movements is increased therefore: the breathing is quick as well as shallow: there are perhaps forty, or even sixty respirations executed in a minute, instead of eighteen or twenty. When we find a person lying only on his back, with his knees up, breathing in this manner, and complaining of tenderness of the belly on pressure, and feverish withal, we may be tolerably sure (unless that person be a hysterical girl) that the peritoneum is inflamed, whatever else may be the matter.

The pain in peritonitis is generally sharp, cutting, or pricking in its character. And independently of any pressure made from without, or caused by any change of posture, this pain is apt to be much aggravated at intervals. This, when the inflammation is general, is sometimes owing to the passage of flatus along the bowel, partially distending it, and stretching the inflamed membrane; so that here, also, it is really *pressure* which augments the pain.

When you explore the abdomen by pressure, take care not to make the examination *unnecessarily* a source of pain. Press first gently, with the open flat hand; and keep your eyes on the patient's face at the same time. You will perceive by the expression of his features, whether you are hurting him; even before he takes to verbal complaining.

Acute peritonitis generally sets in with well-marked symptoms: sharp rigors, and high fever, with a hard and strong pulse, which very soon becomes frequent, and often becomes feeble, and is sometimes small from the very first. After the disease has continued for a certain time, it is attended with tension and swelling of the belly. The tension and swelling are tympanitic in the earlier stages. You learn this with certainty by mediate percussion. As the disease advances, the enlargement is sometimes occasioned, in part at least, by the effusion of serum: infallible indications of the presence of which may be obtained by the joint employment of the finger and the ear; by palpitation and auscultation; and by noticing the difference, as to the results of percussion, caused by alterations of posture.

When the disease is advancing towards a fatal termination, the abdomen often becomes greatly distended; the pulse is exceedingly frequent and feeble; the countenance (which in all the stages of the disorder is expressive of anxiety) becomes pinched and ghastly; cold sweats ensue; and the patient dies at length by asthenia: death beginning at the heart. The mind is often clear to the very last.

Such is the ordinary course of peritonitis. But other symptoms, which I have not mentioned, do sometimes accompany it; arising out of the peculiar circumstances of different cases. Thus sickness and vomiting occur very frequently; and these symptoms are supposed to denote that the peritoneal covering of the stomach is especially implicated: but I question whether this is always a correct inference. When strangury happens, which is not uncommon, that part of the membrane which is reflected over a portion of the bladder is probably involved in the mischief. Inflammation of that part of the peritoneum which lies in the immediate vicinity of the kidneys, may cause, Dr. Abercrombie thinks, suppression of urine.

Peritonitis is apt to arise under the influence of cold, like other internal inflammations; especially when cold combined with moisture is applied, under certain conditions, to the surface of the body. It is occasionally produced by mechanical injuries inflicted upon the abdomen. It often prevails epidemically, and produces great mortality, among parturient women: and there is ground for believing that this form of the disorder is propagable, and often propagated, by contagion. Besides this, a very terrible kind of peritonitis is a frequent result of the extravasation of the contents of the alimentary canal, or of urine, or of bile, into the cavity of the membrane; through apertures that are sometimes made by external violence, but more often are the consequences of the progress of previously-existing disease.

I shall make a few observations in respect to one or two of these points, and but a few.

That awful disorder, *puerperal fever*, is more frequently accompanied with *inflammation of the peritoneum*, than with any other inflammation. This variety of peritonitis necessarily engages the attention of the accoucheur; and it doubtless is more fully considered in the lectures of the Professor of Midwifery than I propose to consider it. Indeed, if you would understand puerperal fever as a whole; its shifting aspects, its single source, and its appropriate management; you must study Dr. Ferguson's masterly and conclusive essay on that subject.

Of forty-four fatal cases of well-marked puerperal fever which fell under the observation of Dr. Robert Lee, and in which the bodies were carefully examined, the *peritoneum and uterine appendages* were found inflamed in thirty-two: *i. e.*, in eight cases out of every eleven. The inflammation commences, no doubt, in the uterine portion of the membrane, and spreads thence over the larger part of its surface. Now this peritoneal inflammation, occurring in women after childbirth, may be accidental and sporadic; or it may prevail in a district epidemically. And a most dreadful and deadly affection it may then become. In either case, the peritonitis may commence a few days, or even a few hours, after parturition. The pain generally begins low in the abdomen, in the situation of the uterus; which may be felt through the abdominal parietes, and is tender on pressure: but soon a universal swelling takes place, and the womb can no longer be distinguished. Cases of this kind arise sometimes, appa

rently, from cold; and exhibit no peculiar feature. It is just what we might expect when a female, in a state of weakness and irritability, happens to be exposed to the exciting causes of inflammation. In such a state, a less degree of the exciting cause would be sufficient to produce the inflammation: and probably a less degree of inflammation may prove fatal.

But when peritonitis is frequent among women after childbirth in a particular neighbourhood, or in a lying-in-hospital, it is marked by greater depression of the vital powers, and runs a more irregular course. The nervous system suffers, the sensorium is apt to become affected, and the complaint assumes rather the character of typhus fever than of simple inflammation of the peritoneum. And no wonder; since this variety of peritonitis forms part of a disease which, like typhus fever, is a *general* disease, and results from contamination of the blood. This Dr. Ferguson has clearly established. The contamination may originate in the body of the patient herself; the noxious material being supplied by putrid coagula, or portions of placenta, remaining in the uterus. Or some of the products of inflammation may enter the blood-vessels, and constitute the poison. And this it may be difficult, or impossible, to prevent. But, on the other hand, the contamination may arise in the way of *contagion*; this horrible malady may be communicated from one lying-in woman to another by the intervention of a *third person*; and doubtless it is so carried and propagated, in many instances, by midwives and accoucheurs. Now this source of the disorder may be obviated; and therefore it is of the utmost importance that it should be clearly recognized, in order that it may be carefully provided against.

You must know, however, that great differences of opinion have existed, and, I believe, still exist, in respect to the contagious quality of certain forms of puerperal peritonitis; just as great differences exist as to the contagiousness of continued fevers, of cholera, of the plague. There are persons who regard the whole notion of contagion as a mere bugbear: and there are others who embrace in their allegations of contagion many more diseases than can be *proved* to be so caused. The same strong assertions are made, the same kind of eagerness is displayed (the same party-spirit, I had almost said), as mark the strife of ordinary politics. It is our serious duty, however, to inquire what is the truth in this matter: for the safety of the individuals, and the happiness of whole families, may often hang upon our opinions. I must trouble you, therefore, with a few facts that bear closely upon the subject.

We possess some valuable and highly instructive accounts of epidemics of the kind I am alluding to. One by Dr. Gordon, on the epidemic peritonitis after childbirth, which took place at Aberdeen, in the years 1789, 1790. Another by Mr. Hey, on that which happened at Leeds from 1809 to 1812. And a third by the late Dr. Armstrong, on that which was observed in Sunderland and its neighbourhood, in 1813. Dr. Robert Lee has also collected some very interesting facts in reference to the spreading of the peritonitis by contagion. Dr. Gordon had, he affirms, unquestionable proof that the cause of the disease was a specific contagion, and that it did not arise from any noxious constitution of the atmosphere. The disease seized such women only as were visited or delivered by a practitioner, or taken care of by a nurse, who had previously attended patients afflicted with the same disorder. And Dr. Armstrong observed that forty out of the forty-three cases that happened in Sunderland, occurred in the practice of one surgeon and his assistant.

From among other histories—all tending to the same conclusion—brought together by Dr. Lee, I take the following:—

“On the 16th of March, 1831, a medical practitioner, who resides in a populous parish on the outskirts of London, examined the body of a woman who had died a few days after delivery, from inflammation of the peritoneal coat of the uterus. On the morning of the 17th of March (*i. e.*, the *next* morning), he was called to attend a private patient in labour, who was safely delivered on the same day. On the 19th, she was attacked with severe rigors, great disturbance of the cerebral functions, rapid feeble pulse, with acute pain of the hypogastrium, and a peculiar sallow colour of the whole surface of the body. She died on the fourth day after the attack, on the 22d of March; and, between that period and the 6th of April, the same practitioner attended two other patients, both of whom were attacked by the same disease in a malignant form, and fell victims to it. On the 30th of March he bled a young woman who had pleurisy: the wound became inflamed after a few days; erysipelatous red-

ness, and swelling, extended from it up the arm; and in four or five days that patient died of phlebitis."

Mr. Robertson, of Manchester, states the following facts, in a paper in the *Medical Gazette*.—From the 3rd of December, 1830, to the 4th of January, 1831, a midwife attended thirty patients for a public charity. Sixteen of these were attacked with puerperal fever, and they all ultimately died. In the same month, 380 women were delivered by other midwives for that institution; but none of the 380 suffered in the smallest degree. All the sixteen had inflammation of the peritoneal surface of the uterus. So, also, Dr. Robert Lee tells us that, in the last two weeks of September, 1827, five cases came under his observation. All the patients had been attended in labour by the same midwife: and no example of febrile or inflammatory disease of a serious nature occurred during that period among the other patients of the same dispensary, who had been attended by the other midwives belonging to the institution.

Statements of this kind—and they could be largely multiplied—furnish irresistible evidence, that the peritonitis, which prevails epidemically among lying-in women, is of a specific nature, and communicable from one person to another. It is observed, also, to reign as an epidemic especially in *Lying-in Hospitals*, and that it occurs at *irregular intervals*, sometimes leaving them quite exempt from its ravages for years together.

Indeed, I believe that these cases of puerperal fever occurring in succession to the same practitioner, are examples of something more than ordinary contagion, operating through the medium of a tainted atmosphere. I believe them to be instances of direct inoculation. Recollect, that the hand of the accoucheur is brought, almost of necessity, into frequent contact with the uterine fluids of the newly-made mother. Recollect,—those among you who have examined the interior of the dead body with your own hands,—recollect, with what tenacity the smell, which is thus contracted, clings to the fingers, in spite even of repeated washings; and, whilst this odour remains, there must remain also the matter that produces it. Recollect how minute a quantity of an animal poison may be sufficient to corrupt the whole mass of blood, and fill the body with loathsome and fatal disease. Illustrations will occur to you in the inoculated smallpox, in hydrophobia, in the viper-bite, in the scratches and punctures of the dissecting-room. Recollect the raw and abraded state of the parts concerned in parturition; the interior of the uterus forming a large wound, and presenting, as Cruveilhier has observed, an exact analogy to the surface of a stump after amputation; the more external soft parts bruised and sore. Bear in mind the remarkable fact, that this contagion does not affect other persons, but only lying-in women. Reflecting upon these facts, you will see too much likelihood in the dreadful suspicion, that the hand which is relied upon for succour in the painful and perilous hour of childbirth, and which is invoked to secure the safety of both mother and child, but especially of the mother, may literally become the innocent cause of her destruction; innocent no longer, however, if, after warning and knowledge of the risk, suitable means are not used to avert a catastrophe so shocking.

I need scarcely point to the practical lesson which these facts inculcate. Whenever puerperal fever is rife, or when a practitioner has attended any one instance of it, he should use most diligent ablution; he should even wash his hands with some disinfecting fluid, a weak solution of chlorine for instance: he should avoid going in the same dress to any other of his midwifery patients: in short, he should take all those precautions which, when the danger is understood, common sense will suggest, against his clothes or his body becoming a vehicle of contagion and death between one patient and another. And this is a duty so solemn and binding, that I have thought it right to bring it distinctly before you.

In these days of ready invention, a glove, I think, might be devised, which should be impervious to fluids, and yet so thin and pliant as not to interfere materially with the delicate sense of touch required in these manipulations. One such glove, if such shall ever be fabricated and adopted, might well be sacrificed to the safety of the mother, in every labour. Should these precautions all prove insufficient, the practitioner is bound, in honour and conscience, to abandon, for a season, his vocation.

All this I have thus taught in these lectures, from the first. Subsequently to their publication, additional evidence to the same purpose has been promulgated, in the

Fifth Annual Report of the Registrar-General; to which I refer, because the collector of that evidence (Mr. Storrs of Doncaster), points out a wider range of danger than I had indicated, and extends his sound admonitory counsel beyond the cases of puerperal peritonitis, or childbed fever. He shows that the mischief does not always *originate* in the practice of midwifery. The infecting virus is liable to be carried, not only from one parturient woman to another, but from various other sources of animal poison; the circumstances of childbirth rendering the mother peculiarly susceptible of such contagion. More than one series of these fatal maladies have been traced back to the attendance of the accoucheur, at the same period, or just before, upon some case of erysipelas, of sloughing sores, of external gangrene, of typhus fever, and even to his recent presence at the examination of some dead body. In the great general Hospital at Vienna, there are three compartments appropriated to lying-in women. Here upwards of 6000 births take place annually. One of these compartments is assigned to the instruction of medical men and midwives: another to the instruction of midwives only. In the former of these two, fatal puerperal fever was so very much more prevalent than in the latter, as to attract the notice and the intervention of the Government. It appeared upon inquiry that the male students busied themselves with the investigations of the dead-house, and the dissecting-room. A regulation was therefore made and enforced, that every student should wash his hands in a solution of chlorine both before and after every examination of the genital organs in the living subject. From that time the excessive mortality from puerperal fever declined, until it became the same in each of the two compartments. I give these facts on the authority of Dr. Routh. The hazards which I have been pointing out are hazards which the practitioner in midwifery, especially in the country, cannot always avoid; but it is most important that he should be aware of them, and should strive to defeat the risk by the most scrupulous observance of every conceivable precaution.

[PUERPERAL FEVER.—A disease which prevails as an epidemic, or as the endemic of particular localities; suddenly making its appearance among a community, prevailing for a shorter or longer period, and as suddenly ceasing, without our knowing why it came or being able to determine the cause of its cessation;—a disease to which all parturient females within the sphere of the epidemic or endemic influence are alike liable—the young as well as the more aged, the strong, the weak; the lady in her well-appointed comfortable lying-in apartment; the poor daughter of toil in her comfortless, miserable garret; she who has passed through a short, natural, favorable labour, as well as the poor, downcast, exhausted parturient, who has just passed through a tedious, difficult, agonizing labour, with a womb strained and bruised, and in a state, already, of incipient inflammation;—a disease which is ushered in by the usual phenomena of fever, which phenomena do not always follow, but most generally precede the indications of local disease. Does it consist simply in inflammation of the peritoneum, or of one, or several, or all of the pelvic viscera? How happens it that these inflammations, if they be the primary and sole cause of the disease, are so rife under a particular epidemic constitution of the atmosphere, requiring no other predisposition than merely the act of parturition, while in the absence of such epidemic influence the uterus may be tried to its utmost by the efforts of childbirth; it may be ruptured, laid open by the Cæsarean section, have the hand inserted into it in the act of turning—it may be inverted, and subjected to the manipulations necessary for its replacement, and yet no inflammation of the organ shall result, or if it does, it will not be accompanied by phenomena in the least resembling those characteristic of true puerperal fever. Let it be also recollected that the latter disease is not confined to the parturient female. That it may, and often does attack the pregnant female, is admitted on all hands.

There is no other mode of explaining this apparent paradox, than by a recognition of the true character of puerperal fever. That it is not a simple inflammation of the pelvic or abdominal viscera, but the effect of a morbid impression made upon the entire organism, causing a general disturbance of its functions, of which the local lesions are merely the result. In short, that it is a true idiopathic fever.

The fact that, in nearly all the fatal cases of puerperal fever in which a *post-mortem* examination has been made, the evidence has been revealed of inflammation of the

womb or its veins, or of the peritoneum, or of several or all of these parts, is assumed as an incontestable proof of the position, that puerperal fever is purely a phlegmasia, and nothing more.

This fact, however, is freely admitted by all those who see in childbed fever something more than a mere local inflammation—who believe that, in one form of the disease at least, it is a true idiopathic fever, the local lesions being the result of, and not themselves constituting the disease.

It appears to us that attention has been too exclusively confined to the indications of inflammation detected after death from puerperal fever, in the pelvic organs and the peritoneum. These are not the only lesions met with in the bodies of those who have been destroyed by the disease. Rokitsansky describes as frequently present a slight reddening, with investment, of the entire track of the intestinal mucous membrane, by a secretion of a thin serous or viscid gelatinous, or more or less purulent character, softening of the mucous and infiltration of the mucous tissues; a dysenteric exudation on the mucous membrane of the colon, resembling that found on the internal surface of the uterus. A similar exudation is also met with on the mucous surfaces of the respiratory, urinary, or œsophageal tracts. The pleura are almost constantly found to contain exudations similar to those met with in the peritoneum; less frequently they are met with in the pericardium. The articulations very commonly exhibit exudations of a fibrinous or purulent character. The dura mater often presents a slight reddening, with a thin, soft exudation. Rokitsansky describes a black softening of the mucous membrane at the fundus of the stomach or of the œsophagus, indicated during life by black vomit, as a frequent occurrence. According to the same authority, the blood exhibits various changes, its fibrinous coagula present a viscid, greenish-white appearance, or the coagula are scanty, gelatinous, and soft. The blood is of a dirty brown red, or chocolate colour, and glutinous, or it is much attenuated, and transudes all the tissues. Vegetations on the valves may form from mere mechanical deposition.

In the dissections made in Philadelphia, during the epidemic of 1842, the liver, spleen, and kidneys were found softened, as in cases of malignant fevers. In one of the cases, the stomach contained a fluid resembling coffee-grounds, and probably the same as the black vomit of yellow fever.

That the disease is not essentially a local inflammation, of which the fever is merely a symptomatic or sympathetic effect, is disproved from its having been found, as remarked by Dr. Simpson, that: "1st. There is no general uniformity of relation and sequence between the degree and intensity of their supposed cause—the local inflammatory lesions—and the degree and intensity of their supposed effect, the attendant fever. 2d. Sometimes the supposed cause—in the form of simple peritonitis, or metritis, &c.—may exist, without these inflammations exciting the usual phenomena of their supposed effect, namely, the symptoms of puerperal fever; and, 3d. We see occasionally cases of true and fatal puerperal fever, without discovering on the dead body any traces or evidence of the local inflammation which had been considered the origin of the disease. In other words, under this last class of cases we have the existence of the supposed effect without the existence of the supposed cause. And this observation holds good with regard not only to the individual local inflammations, which have been illogically dogmatized into the alleged invariable origin of puerperal fever, but it holds good with regard to the whole class of local inflammatory causes. Some authors, while they maintain the disease to be a fever entirely symptomatic of some local inflammation, at the same time hold that this local inflammation may be seated in different parts in different cases, and different epidemics, and that the disease originates, in one case, in metritis, in another, in ovaritis, in a third, in peritonitis, and so on. Without remarking on the illogical nature of imagining that the same disease may have such varied origins, we may, once more, pointedly observe that—as sometimes happens in continued fever—occasionally, though very rarely, no inflammatory lesions whatever can be traced upon the bodies of patients who have died of puerperal fever. Dr. Locock has observed several cases of this kind; and, in the practice of the late Dr. Beilby, I saw one very marked and rapidly fatal case of puerperal fever, in which my colleague, Professor Bennett, was unable to detect anywhere in the abdomen, or in the uterus, its appendages or vessels, any traces of inflammatory action or

effusion. The great rarity of such instances is no sufficient argument against their important bearing upon the question of puerperal fever."

Dr. Meigs, in his recent treatise on childbed fever, repudiates the existence of an idiopathic puerperal fever, inasmuch as the term fever excites "a certain material idea of zymosis in the mind of the hearer of it;" but he, nevertheless, substantially admits all that would be necessary to include puerperal fever in the list of zymotic diseases.

"Childbed fever," he remarks, "becoming epidemical, may prevail so extensively as to implicate almost all the women who are brought to bed under its reign; or the force of the cause may be so slight as to produce illness in only here and there an unfrequent example, so that the epidemic cases may be very rife or not."

And again: "It is quite clear," he says, "that the malady may break out, and rage with violence in certain circumscribed spaces; and on the other hand, that it may prevail, at one and the same time, over extensive districts, and even whole nations and countries, and yet be ever one and the same disorder."

"There are, then," he observes in another place, "atmospheric causes, that render pregnant and lying-in women, at particular times, and in certain places, uncommonly liable to attacks of childbed-fever inflammation. What the real principle of this epidemy is, I believe there is no man can say. Be it what it may, one of the most extraordinary conditions connected with it is this: *that it should not poison men, nor boys, or girls, or non-pregnant women, but only the pregnant or lying-in portions of society.* This appears to me to be its greatest mystery."

Now, is it strictly true, that the atmospheric cause of epidemic puerperal fever, whatever its nature may be, poisons only "the pregnant or lying-in portions of society?" If we examine the histories that have been furnished us of the several epidemics of erysipelas that have prevailed in different portions of the United States, we shall find that a certain morbid condition of the atmosphere may occur, which, while it produces in some of those subjected to its influence an erysipelatous affection of the skin, in others it gives rise to inflammation of the mouth and fauces, or of the lungs and pleura; in others, again, to inflammation of the peritoneum; and, in pregnant and parturient females, to puerperal fever.

Dr. Drake makes the following statement, based upon an analysis of the several accounts given of epidemic erysipelas as it occurred in the Interior Valley of North America:—

"The peritoneum in men and non-parturient women was obnoxious to the inflammation, but not in as high a degree as the pleura. Pregnant, and especially lying-in females were, however, peculiarly liable, and the most fatal cases were the puerperal."

In the terrible epidemic of erysipelas which prevailed near Norristown, Pennsylvania, in the autumn of 1847, "old and young, male and female, fell before it," says Dr. Corson, "and yet there seemed to be one class that it preferred. The mother, as she lay helpless and exhausted from the labour and agony endured in giving birth to her child, was marked as a victim. The deadly poison was infused into her veins, and, in many instances, a few hours sealed her doom." "I lost more puerperal women during the epidemic than for twenty years before." "This epidemic produced in one class of patients well-marked erysipelas, in another inflammation of the mucous membranes lining the fauces and nasal cavities, and in a third, diffused inflammation of the serous tissues; while yet others were met with, in which all these conditions followed each other, or existed simultaneously." "In females, the serous membranes were affected generally, while in males the mucous or cellular tissues were almost the only parts involved."—(*Trans. Pennsylvania State Med. Soc.*, vol. ii.)

In the winter and spring of 1851–52, epidemic erysipelas again made its appearance in the upper portion of Montgomery County, Pa. "The disease," says Dr. Vanbuskirk, "seemed first to attack the throat, and afterwards the surface of the body. In females, it was especially liable to attack the peritoneum, and one or other of the serous tissues in the male. When the peritoneum became affected, there was much hiccough from the disease extending to the diaphragm. In some cases, symptoms of arachnitis, followed by coma, presented themselves." It is further added, that many cases of puerperal fever occurred during the prevalence of erysipelas; and, as far as information was obtained, "these cases of puerperal fever were confined

chiefly to the same localities as the latter disease." — (*Trans. Penn. State Med. Soc.*, vol. ii.).

Speaking of the epidemic erysipelas as it occurred in Montgomery County in 1852, Dr. Geiger informs us that, "it spared neither age, sex, nor condition." "It marked the parturient woman for its especial victim. Not a single woman living within the range of the disease, who was delivered during its prevalence, escaped an attack." "Besides those cases of puerperal fever which were evidently erysipelatous, males were frequently attacked with symptoms indicating inflammatory disease in one or other of the internal organs, as the brain, lungs, heart, intestines, and their serous investments." — (*Trans. Penn. State Med. Soc.*, vol. iii.).

In the latter part of March, 1852, epidemic erysipelas made its appearance in Palmyra County, Pa. "Few lying-in women," says Dr. Gloniger and Breitenbach, "escaped its attack, and the ratio of mortality, we have been informed, was quite large." — (*Op. citat.*, vol. ii.).

Dr. Bennett, in his history of the epidemic erysipelas which prevailed in Danbury, Connecticut, during the winter and spring of 1847–48, says: "The serous membranes were a frequent seat of the disease, especially the pleura and peritoneum. Three cases of puerperal peritonitis are included in the list." — (*Trans. Amer. Med. Assoc.*, vol. ii.).

Dr. Mendenhall, in his report on the epidemics of Michigan, &c., tells us that "erysipelas has prevailed as an epidemic for the last two years (1851 and 1852), usually affecting the head and face"—"puerperal peritonitis prevailed contemporaneously with erysipelas in this region. In some cases the erysipelas attacked the labia and vagina, and was soon followed by puerperal peritonitis." — (*Trans. Med. Assoc.*, vol. v.).

In 1853, erysipelas prevailed as an epidemic in Dayton, Ohio; Dr. Sutton informs us that females advanced in pregnancy were exceedingly prone to premature labour, and the period of accouchment was looked to by both patient and physician with the deepest anxiety and solicitude. But one parturient female within the range of Dr. Sutton's information escaped an attack of puerperal fever—and every one that was attacked died.

Non-pregnant females suffered in many instances from inflammation of the peritoneum and of the pelvic viscera, and males from inflammation of the respiratory mucous membrane, or of one or other of the serous surfaces. — (*Op. citat.*, vol. v.).

But it is unnecessary to multiply evidence to prove that the same epidemic cause which gives rise to erysipelas may also produce in the male, and in the non-pregnant and non-parturient female, peritoneal inflammation, and in the pregnant and parturient woman the disease termed puerperal fever. Every historian of the epidemics of erysipelas that have occurred of late years, with scarcely a single exception, bears testimony of the fact. The intimate connection between epidemic erysipelas and childbed fever—a connection that had been already recognized by Gordon, Beatty, Nunnely, Kneeland, Holmes, and others—is now, indeed, very generally admitted. Dr. Hutchinson and others have seen both diseases in the same patient. Dr. Simpson, of Edinburgh, has recently advanced the opinion that erysipelas and phlebitis are diseases in "the same category as puerperal fever."

Of the intimate connection between typhous fever, erysipelas, phlebitis, and puerperal fever there can be no doubt.

Much of this, to use the words of Dr. Ormerod, is explicable on the supposition of the existence of the same atmospheric condition affecting all who cannot resist it, in the same way; but however this may be, as far as general impressions, in the absence of notes, will justify the assertion, simultaneous with the occurrence of some cases of fever in the medical wards, phlebitis and troublesome sores are more commonly met with in the surgical wards of this hospital, and erysipelas of the head and face in both.

The force of the facts just referred to, has been attempted to be evaded by a denial of the pathological identity of puerperal fever and erysipelas. But this is a mere play upon words—a mere evasion of the very question at issue. No one ever pretended that the affection of the surface denominated erysipelas, and the collection of morbid phenomena constituting puerperal fever are one and the same disease. Nor is it necessary to prove that the two diseases are identical in all their pathological characters before admitting their production by the same morbid condition of the atmos-

phere. The difficulty in the mind of those who deny the relationship between epidemic erysipelas and childbed fever has originated from the supposition that the epidemic malady, one of the most frequent manifestations of which is an erysipelatous inflammation of some portion of the surface, and the disease known as puerperal fever, are essentially local phlegmasiæ. May we not, with Mr. Nunnely, admit it to be highly probable, if not certain, "that there is," in erysipelas, "some change produced in the state of the blood, which change may depend upon alterations we are unable at present to appreciate, but which, it is likely, occur in many tissues, and may thus affect the mass of blood more or less quickly, and to a greater or less extent, according to the influence they have upon, and the connection they have with, the blood in a state of health."

Dr. Meigs tells us, in his recent treatise, that he is not prepared to say "the epidemy might not have the power over the nervous mass, so to qualify its operations as to determine, in one individual, an inflammatory attack of the corpus mucosum of the skin, and, in another, an attack of inflammation of the serous coat of the belly." But he cannot conceive of a case of pure metritis or metrophlebitis being produced by the same cause as that productive of erysipelas, or possibly of peritonitis.

"Erysipelas," he remarks, "is a disease of the skin, and although, in some instances, it does take on a phlegmonous character, by extending perpendicularly downwards into the connecting areolar texture, it does so only by accident, and not as a normal process of that special phlegmasia. Erysipelas is, therefore, a membranous, but not a visceral disorder, and one of its chief characteristics is found in its propensity to expand its areas of phlegmasia far and wide over the plane of the membrane. Puerperal peritonitis, pure and simple, is also a membranous disease, and possesses the same propensity to expand its areas over the entire plan of the peritoneal membrane. In this, erysipelas and peritonitis are alike; but erysipelas and metritis are not alike. Peritonitis is also like erysipelas in this, that it has a tendency to plunge or descend vertically in the basement textures of it, and so destroy the epiploon, or gangrene the bowel, or produce ramollescence of the exterior stratum of the uterus, or exo-metritis. In erysipelas, this vertical plunge or downsinking of the morbid states of the corpus mucosum often carries it quite through and below the corium, and far down into the substratum of areolar tissue, where it may become either phlegmonous or œdematous erysipelas, as the case may be.

"In like manner, when you shall hereafter examine the mortal remains of individuals who have died of pure childbed peritonitis, though you shall not, in general, observe any other than the results of a purely membranous inflammation, or inflammation of the peritoneum, yet, in some specimens, you may find the epiploon softened and suppurated, the ovaria reduced to a pulp, or the outer stratum of the womb completely reduced to a state of ramollescence or softening. In so far, then, as I have drawn a parallel between the two disorders, you discern a very great similarity between them."

"Why should you vex yourself with this foolish question, when you are already so well informed in your profession as to know that childbed fever is puerperal peritonitis, and nothing else; and that peritonitis, in numbers of the cases, does not come in question at all, the disorder being pure womb-phlebitis alone, or an oophoritis, &c. &c. If you must insist that erysipelas and childbed fever are one, then pray leave out of question all the pure metrites and phlebites, and confine your alliance between the two to the serous and dermal *identities*, if they must be so considered."

We agree with Dr. S. Holmes, in the opinion advanced by him in his recent very able paper on erysipelas, (*Trans. Amer. Med. Assoc.*, vol. vii.) that pathologists have committed an unfortunate error in their efforts to find some one tissue on which the inflammation in erysipelas is expended, while we are convinced that the lesion of several tissues is common, even in the milder forms of the disease. With Dr. Holmes, we hold "that the peritoneum, the pleura, or the arachnoid may take on the erysipelatous inflammation as certainly as the lining membrane of the fauces; if the disease be constitutional, it, like many others, shows preferences to particular parts, but is not confined to those parts; it can no more be called 'a dermal disease' than it can be called a peritoneal disease. In its signs, it is a peculiar form of inflammation, with characters as strong as an inflammation where lymph is thrown out for adhesions, or pus for a covering or protection. Its pathological exudation is like that of many

others, merely a deficiency in its physiological exudation; but, in proportion to the potency of the cause, so will be the power of the exudation to assume the pus formation, or the fibrinous or the simple agglutinative lymph. The pus may show a greater tendency to form on mucous than on serous textures, but that does not exclude the serous, and in proportion to the gravity of the cause will be the result."

We can readily understand, when we consider the condition of the pelvic and most of the abdominal viscera in the female immediately after parturition, why these should be particularly predisposed to the action of the *materies morbi* by which the inflammation in epidemic erysipelas is produced.

Let this be as it may, we have the fact incontrovertibly established that, during the time and in the same place at which erysipelas is prevailing epidemically, males and non-pregnant and non-parturient females are especially liable to suffer from peritoneal inflammation, while pregnant and lying-in women are particularly exposed to an attack of the so-called puerperal fever; and, in the examination of the bodies of those who have died of the latter disease at such periods and places, it is not the peritoneum alone to which the results of inflammation are confined, but the uterus, its veins, its ligaments, the ovaries, and neighbouring intestines are as frequently found involved in disease.

Were this the proper place to enter upon a discussion of the subject, we should have no hesitation to assume as true, and we think we should be able very clearly to demonstrate the actual identity of the pathological character of erysipelas, phlebitis, and puerperal fever. The supposition of the formation of pus in consequence of an inflammation of certain veins, and this pus finding its way into the circulation, giving rise to purulent deposits or secondary abscesses, and the morbid condition known as pyemia, has been shown to be unfounded by Lebert and Rokitansky. The whole of the phenomena in cases of pyemia depend upon a general poisoning of the blood—as the result of which we have local phlebitis—often in several parts of the body widely remote from each other, and true suppuration—the result of circumscribed inflammation within the substance of many of the organs.—It would not be difficult to show that precisely the same dyscrasy of the blood occurs both in erysipelas and in puerperal fever.

The low adynamic form of childbed fever which so generally prevails in over-crowded and ill-ventilated hospitals, is supposed by some to be either purely typhus fever without implication of the generative organs, or a combination of phlegmasia of these with typhus fever. Of the former cases nothing need be said, as it is not to be supposed that any well-instructed physician would confound typhus or typhoid fever with puerperal fever.

Puerperal fever is, confessedly, the especial endemic of the lying-in wards of hospitals, and it there presents itself, usually, in its most malignant and intractable form. Now, when it occurs in these institutions simultaneously with typhus fever, erysipelas, and hospital gangrene, are we to admit the conjoint presence of three distinct morbid states of the atmosphere, the one productive of typhus fever, another of erysipelas, and a third of childbed fever, or subscribe to the opinion of Dr. Walsh, that puerperal fever is not a disorder *sui generis*, confined to lying-in women, but "merely an unusual form of a very common disease," being, "in reality, no other than the common infectious fever, complicated with more or less extensive inflammation of the peritoneum;" and, we would add, the womb and its appendices.

Dr. Meigs would appear to admit that the endemic cause of typhus fevers may give rise to the very lesions in which he considers the so-called puerperal fever to consist.

"I beg of you," he remarks in his late work, "to understand me as asserting that, while childbed fever is a phlegmasia, and that while there is not, in our nosology, such a thing as a true idiopathic childbed fever, I yet admit the possibility of typhus, jail, hospital, and ship fevers occurring in our class of patients." Some of the cases proceeding to their solution, in recovery or death, without interesting in a particular manner the child-bearing organs, or the peritoneum: while there are others that early establish areas of phlogosis, which may or may not take up the mastery in the subsequent progress of the malady."

We can understand the foregoing language, vague and cautious though it be, to imply nothing, more or less, than that the epidemic or endemic cause of typhus fevers is capable of giving rise, in pregnant and parturient females, to puerperal fever. If

it do not mean this, it means nothing. Now, taken in this sense, in connection with the admission that childbed fever may be the result of an atmospheric poison, or some malign condition of the surrounding air — everything is admitted that we contend for — namely, that some general morbid cause — we shall not dispute about its nature, probably this will be forever hidden from us — by its impression upon the nervous system — upon the endangium — or by its gaining an entrance into the blood and modifying its crisis — the question as to its primary mode of action upon the organism being a matter of indifference as to the main fact at issue — so disturbs the general functions of the living body as to give rise to those morbid phenomena, which constitute the disease we call idiopathic fever — one of the consequences of which general disturbance of the functions — under certain circumstances and in certain individuals — is, in addition to various other lesions, inflammation of one, or several, or all, of the pelvic viscera, and of one or other of those of the abdomen.

As Mr. W. Tyler Smith remarks, in his Lectures on Puerperal Fever, published in the *London Lancet*, “The more puerperal fever is investigated and tracked, as it were, to its elements or origin, the less satisfactory does any partial or local explanation of its origin become. In the progress of such an examination, it appears more and more evident that there is a puerperal (febrile) poison to which the lying-in woman is liable, and which produces all the varied phenomena of puerperal fever met with in different epidemics, localities, seasons, and constitutions. In one time or season, peritonitis is produced, in another, metritis, in another, phlebitis, in another, mammary or other abscesses; in another, low fever, in another, intestinal irritation, in another, dissolution of the blood, without a trace of local inflammatory disorder, and so on throughout the list of local and special disorders which have been described by authors on puerperal fever. It may be questioned, even if phlebitis ever occurs without a poisonous condition of the blood, produced either as the result of contagion, epidemic influence, or the absorption of putrid matter from the uterus. Thus, in the earliest pathological arrangements, a great number of disordered states were grouped together as puerperal fever, without attempt at discrimination or analysis; next came a laborious separation of the different forms and manifestations of the disease; and the subject seems, at the present time, ripe for allaying the numerous affections met with in puerperal fever together, in their origin from a common cause — namely, some animal poison, or zymotic influence.”

Under this view of the case, puerperal fever loses the anomalous character which so long has been ascribed to it — it ceases to be a mystery. It is no longer an epidemic disease whose subjects are alone parturient females; but one of the forms of a prevailing epidemic fever; its peculiar features in the recently-delivered woman, not being due to a specific virus to which she alone is liable, but to the condition of the uterus and its appendages immediately after child-birth predisposing them in an especial manner to become the seat of disease, amid that general disturbance created in the living organism by the morbid influence of the prevailing atmospheric poison, the malaria, the epidemic constitution, or whatever other name may be given to the reigning epidemic or endemic cause. But of which, let it be recollected, the influence is not experienced solely by the inmate of the childbed, but is experienced by the community at large, producing, in males and females, a fever accompanied in its course by erysipelatos inflammation of the surface, or by inflammatory affections of the mucous or serous tissues, and often, in the unpregnant female, by nearly all the leading features that characterize it when it occurs in the parturient woman. — See note by the Editor to Churchill on the Diseases of Women, Last American Edition, Philada., 1857. — C.]

The cadaveric venom here referred to, is well known, and is justly dreaded by us all: and especially by those among us who are oftenest engaged in examining the interior structure, whether healthy or diseased, of the dead human body. Admitted through a cut, a puncture, a mere abrasion of the cuticle, it gives rise to ill-conditioned sores, tenderness of the absorbents, diffused inflammation of the reticular tissue, and great constitutional disturbance. Many promising students, many valuable members of our profession, have perished prematurely of this accidental inoculation. There is reason to think that the virus is produced in the early stages only of the process of decomposition: that when putrefaction has reached a certain point, the point at which

sulphuretted hydrogen is evolved, the special poisonous quality ceases. It is during a particular stage of their decay, that German sausages, bacon, and cheese, are sometimes found to acquire deleterious and even fatal properties. The claws of carnivorous beasts, and the beaks and talons of carnivorous birds, are apt to be charged with the same kind of poison; which gives to their bites or scratches a peculiarly dangerous character.

[The connection between puerperal peritonitis—or, to speak more correctly, between *puerperal fever* and erysipelas, has recently attracted a good deal of discussion. The facts in support of such connection are too numerous and too well authenticated to allow of any reasonable doubt being entertained upon the subject. It has been shown that the two diseases prevail very generally simultaneously in the same localities, while one of the most common lesions attendant upon constitutional erysipelas, in the male subject and in non-pregnant females, is peritoneal inflammation. Facts have also been adduced, which prove that a contagion of some kind may be communicated either directly, or through the intermedium of a third party, from a patient laboring under erysipelas to the parturient female, and induce in her an attack of puerperal fever. — C.]

That variety of peritonitis which results from perforation of the stomach or intestines, and the effusion of their contents into the cavity of the belly, is full of interest. The inflammation is violent in degree; universal (generally) in extent; and almost always fatal. The attack is characterized by its *suddenness*. All at once intense pain arises in some region of the abdomen, which soon becomes tender in every part. The pain is incapable of removal, and usually even of mitigation, by medicine, and death takes place in a short time. These are the general features of such cases. Occasionally, the symptoms follow some different order. Thus, I have seen a case in which no pain was complained of, and the source of the inflammation was not suspected until the dead body was examined. This was a case of *fever*; and it had been attended with much stupor, which was probably the reason that no indication of suffering was made by the patient. Occasionally, but that is uncommon, the pain intermits. For the most part, however, it resists all treatment, and ceases only with life, or a short time before life is terminated.

Most of the instances of this kind of peritonitis that I have witnessed, have resulted from perforation of the ileum, in the progress of continued fever. You are already aware, that the glandulæ agminatæ, which are found only in that bowel, and the glandulæ solitariæ, which are scattered over nearly the whole inner surface of the alimentary tube, are very liable, in one species of continued fever, to inflammation, sloughing, and ulceration: and sometimes the ulcers *go through*: the contents of the gut are poured into the cavity of the serous membrane, and intense inflammation is lighted up. I purposely abstain from going into any particulars respecting these sloughing ulcers. We are no further concerned with them at present than as they furnish the channel by which the cause of the peritoneal disease is introduced. Once, and once only, as I stated before, have I known perforation occur from the extension of scrofulous ulceration of the same glands in *phthisis*. In general, in that disease, the ulcer runs a much slower course. As it approaches the peritoneum, circumscribed chronic inflammation is set up in that membrane; lymph is thrown out; and the bowel becomes adherent to some other portion of the canal, or to some of the other viscera of the abdomen. In this way the perforation is prevented; or, it should take place, the escape of the contents of the bowel into the peritoneal sac is prevented. Occasionally, when two portions of the tube thus adhere together, a communication is formed between *them*, and the contents of the intestine either reach an advanced point of their stated journey by a short cut; or are carried back again perhaps to a point which they had already passed.

A very singular instance of this latter event has been described by Dr. Abercrombie. A man, fifty-six years old, who had shown no signs of serious illness, but had laboured under impaired appetite, languor, and occasional pain in the abdomen, for two or three weeks, was suddenly seized, while taking a walk, with vomiting; and he observed that what he brought up was stercoraceous; and this occurred again and again, at various intervals; the matter vomited being distinctly fecal, and sometimes

so solid that he was obliged to swallow warm water to soften it, that it might be expelled from the stomach more easily. He never vomited his *food*: and no tumour, nor any other sign of organic disease, could be detected by external examination. He lived about three months, and died at last of exhaustion: and then it was discovered that the stomach and the transverse arch of the colon were adherent to each other, and that a ragged aperture of communication between them existed at the place of adhesion.

I have learned of late, from Dr. Brinton, whose attention has been specially directed to this subject, that there are on record at least twenty cases of a communication, thus formed by adhesion and ulceration, between the stomach and the colon.

It is a curious fact, that the vermiform appendage of the cæcum is not unfrequently the seat of a penetrating ulcer. I have traced little groups of glands in that slender tube: and I have known perforation to happen from the specific ulceration of typhoid fever; and from the accidental ulceration caused by a cherry-stone lodged there in one instance, and by a pellet of hard fæcal matter in another.

Sometimes it is the *stomach* that is perforated, either by a common or by a specific ulcer; and the symptoms are exactly the same as when the *bowel* gives way. Sudden, unremitting pain; tenderness, and tympanitic distension of the abdomen; and early death.

Perforating ulcers of the stomach are of various kinds. It is not uncommon to find one small roundish hole, somewhat larger generally on the inner than on the outer surface, the edge of the mucous membrane being as smooth and clean as if a disk had been cut out from it by a punch, and without any surrounding hardness, or other mark of disease. Occasionally the orifice is more irregular, and occupies the centre of a thickened and indurated patch of the mucous membrane. Several instances of this sort of perforation have occurred under my own eye; two within the last fourteen months (1839). Almost all the patients have been young unmarried women, plump, and in good condition; who, up to the moment of the fatal seizure, either seemed to enjoy perfect health, or, at most, had complained of slight and vague feelings of dyspepsia. Ulcers of the stomach are, however, mostly chronic; and the diseased viscus is commonly fenced about and protected by adhesion to the neighbouring parts, before its coats are completely penetrated by the ulcer. You are probably aware that this happened in the body of Napoleon Bonaparte. He died of cancer of the stomach. That organ was strongly adherent to the concave surface of the left lobe of the liver, which formed a part of the wall of the stomach; and this adhesion, no doubt, prolonged his life.

The actual perforation, in cases such as we are now considering, may result merely from the natural progress of the ulcer; but sometimes it would appear that the thin membrane which remains is broken by some accidental force applied to it. Thus the distinctive symptoms occur most frequently after a meal, when the stomach is distended with food, and engaged in the churning movement which attends the process of digestion. They have immediately followed the act of vomiting, brought on by an emetic. Bouillaud relates an instance in which the perforation happened while the patient was straining at stool; and it is conceivable enough, that rough pressure of the abdomen might complete the rupture, when the ulcer had already eaten through all the coats of the bowel except its peritoneal coat. I shall return to this subject in a future lecture.

Occasionally the perforating ulcer has its seat in the *duodenum*. And it is a very curious fact, of which no satisfactory explanation has hitherto been given, that ulcers in the duodenum, at no great distance usually from the pylorus, are of common occurrence in persons who have suffered severe and extensive burns. A number of cases of this kind have been published by Mr. Curling in the 25th volume of the *Medico-Chirurgical Transactions*. The inflammation and ulceration probably commence in one or more of the glands of Brunner. Frequently the ulcer leads to adhesion between the gut and the pancreas. Sometimes it penetrates into the abdominal cavity, and excites fatal peritonitis.

Less commonly than this, in my experience at least, the peritonitis is set up by the escape of urine from the *urinary bladder*, through the extension of an ulcer, or from the forcible rupture of that bag by a blow or a fall, when it was distended with urine. Rupture of the *gall-bladder*, whether by violence or from ulceration, has the same

results; so also has, in general, the rupture of the *uterus*, which sometimes takes place during the efforts of parturition. *Abscess of the liver*, bursting into the peritoneum, is another occasional source of severe and fatal inflammation of that membrane. Acute and general peritonitis sometimes arises, also, in consequence of penetration from without; *i. e.*, it succeeds the puncture made by the trocar in the operation of tapping the belly: and these cases too, are almost all of them mortal; chiefly, I presume, because, in nine instances out of ten, they occur in an unhealthy and debilitated subject.

I formerly offered you some observations respecting a form of hysteria which very closely *mimics* peritonitis, and would most certainly deceive a medical man who was not on his guard against it. We judge by the age and sex of the patient somewhat; by the presence of hysteria in other forms, or of the hysteric diathesis; by the *excessive* tenderness of the abdomen, or rather of its surface; by the coexistence of the same exquisite sensibility in other parts; and by the incongruity and shifting character of the symptoms. The pulse and the tongue will perhaps be natural, while the abdominal irritation is at its height. Forewarned, you will seldom find much difficulty in establishing the diagnosis. Of the signs by which peritonitis may be distinguished from enteritis, I shall speak when I come to the latter disease.

LECTURE LXVI.

Treatment of Acute Peritonitis; Bleeding, Mercury, Opium. Chronic Peritonitis. Granular Peritoneum. Ascites; Ovarian Dropsy; Diagnosis of these diseases. Other forms of Abdominal Dropsy.

ACUTE peritonitis, in its simple form, is always a dangerous, yet frequently a manageable disease. When it is complicated with other and earlier organic mischief, and especially when it has been excited by the entrance of foreign matters into the cavity of the belly, it is all but hopeless under any treatment.

In speaking of the mode of cure, I have again to rehearse, *mutatis mutandis*, the tried remedies for inflammation, and particularly for the adhesive inflammation proper to serous membranes: *blood-letting; mercury.*

It is of the greatest importance in this, as indeed in all cases of inflammation, that the blood-letting should be performed *early*. You must not be deterred from bleeding by the mere smallness of the pulse: a quality which I have frequently shown you to be characteristic of acute inflammation within the abdomen; and which, in the disease now in question, is often present from the very beginning. If the pulse be wiry and hard, we disregard, in these cases, its smallness. Not uncommonly it is rendered more full, as well as softer, by venæsection; and this when it happens, gives assurance of the propriety of that measure.

Topical blood-letting is of much efficacy—of greater efficacy perhaps than in most other forms of abdominal inflammation. Cupping is out of the question, from the tender state of the abdomen. But in adults, after a full bleeding from the arm, such as has produced some sensible impression upon the circulation, or brought the patient to the verge of syncope, the surface of the belly should be *covered* with leeches. From twenty to forty may be applied at once; and sometimes this will make any further loss of blood unnecessary. But in severe cases, you may expect to find that petitions of at least the local bleeding will be requisite.

After the leeches have fallen off, a light poultice may be laid over the abdomen: it may be assiduously fomented with flannels wrung out of hot water. These means will encourage the bleeding from the leech-bites; and are generally found to afford great comfort to the feelings of the patient. *Cold* applications have been commended by some practitioners of high authority. Dr. Sutton injected cold

enemata, and applied cloths, made wet with cold evaporating lotions, to the abdomen, with good effect; and Dr. Abercrombie has since reported favourably of the same kind of treatment. However, I should think this a more precarious plan than the opposite: and I have always observed so much relief to be given by warm epithems that I have never had the inclination, nor the courage, to employ cold.

It is extremely desirable, in these cases, to obtain as speedily as possible the specific influence of mercury upon the system; by calomel and opium, or by inunction. It cannot be necessary that I should again go over in detail the means of following out this indication; but it is an indication which we must diligently pursue.

The treatment of *puerperal* peritonitis is much more uncertain and difficult; for this reason — that it springs out of an antecedent morbid condition more deeply seated, more generally diffused, and less accessible to remedies than itself. Whenever inflammation arises here and there in the body in consequence of a vitiated state of the blood, we have not only the inflammation itself to deal with, but its physical cause also, which may still be in uncontrolled operation.

Dr. Ferguson justly remarks that “inflammation being made up of vascular and of nervous action, of the afflux of blood to a part, and of pain, it is not irrational to act on both the elements of the malady at the same time, or in periods shortly consecutive of each other.” “The abdominal pain that occurs in puerperal fever, is accompanied by two very different states of constitution; one in which little or no depletion is borne, another in which relief is obtained only by very large evacuations of blood.” Between these two there is very conceivable gradation. “In no malady are a cautious boldness, and a sagacious adaptation of remedy to constitutional power, more imperatively demanded.”

“If large bleeding be determined on, it must, to be beneficial, be resorted to within the first twenty-four hours from the attack. In the second stage of the disease it often produces, rapidly, a fatal result.”

In ambiguous cases he gives ten grains of Dover’s powder, and covers the whole of the abdomen with a large linseed-meal poultice, sufficiently thick to retain warmth for four hours. At the end of that time, if the symptoms are alleviated, “ten grains more of the Dover’s powder, and a fresh poultice, should be prescribed. If within four hours from this second medication, the practitioner is not satisfied that the malady is yielding, he must at once resort to depletion.”

Of all the means we possess of arresting this malady, bleeding, general or topical, is, in Dr. Ferguson’s experience, by far the most extensively applicable. “But,” he says, “while I admit this, I am equally certain that *large* bleeding has not been borne in this complaint, generally speaking, during the last twelve years.”

Another most important truth enunciated by our Professor is, that “epidemic puerperal fever has, invariably, the character common to the ordinary fevers raging with it: if the latter require depletion, the presumption is that the former will also.”

Undoubtedly the very same species of febrile disease is variously affected by a given remedy in different places; and during different periods in the same place. There is such a thing as an epidemic state of the human constitution, gradually produced by a gradual fluctuation in the influences whereby communities of men are surrounded and impressed. The fevers that were cured in London twenty or five-and-twenty years ago by copious blood-letting, would now be rendered by that measure, carried to a like extent, irretrievably mortal. There is scarcely a more important object of study to the practical physician than this different capacity, exhibited by the average of constitutions at different times and seasons, of bearing active depletion. “*Nihil mihi prius est,*” says the wary Sydenham, “*quam quando novæ febres grassari incipient, cunctari paulisper, et ad magna præsertim remedia non nisi suspensio pede, ac tardius procedere; diligenter interim illarum ingenium atque morem observare, quibus itidem præsidiorum generibus ægri juventur vel lædantur, ut quam primum his repudiatis, illis utamur.*” The exciting cause of the fever remains the same; the system upon which it operates undergoes from time to time great changes: which are brought to light partly by the altered phenomena of the disease, partly and chiefly by the effects of remedial measures.

Unless you bear these differences in mind, you will be perplexed and disheartened by the discrepant accounts given by competent and faithful observers, respecting the success of different or even opposite plans of treatment, in the same complaint.

Most writers whose works I am acquainted with, recommend *purgatives* as highly serviceable in peritonitis. I do not think the good which they are calculated to do as antiphlogistic remedies can at all be put in competition with the harm that I am persuaded they may produce by increasing the peristaltic action of the intestines, and so causing additional friction and tension of the inflamed membrane. I believe that in all cases of well-marked and pure peritonitis, when the inflammation is limited to the serous membrane, it is far better and safer to restrain than to solicit the internal movements of the alimentary tube. In a pamphlet published several years ago by Mr. Bates, of Sudbury, some striking instances are recorded of recovery from severe peritonitis under large and frequent doses of opium, and a rigid adherence to the horizontal posture, until all pain had subsided. The patients were not allowed to raise themselves, on any account, into a sitting position: and the opium was administered sometimes by the mouth, sometimes by the rectum. These cases, related in an unpretending manner by a practical observer, made a strong impression on my mind when I read them. To simple inflammation of the peritoneum, to those perilous forms of peritonitis which occur in women after delivery, and to those still more terrible cases that follow perforation of the serous membrane, this principle of keeping the intestines at rest, is alike applicable. I stated a little while ago, that the last-mentioned cases are all but hopeless. The *all but* I inserted on the strength of some most interesting facts published by Dr. Wm. Stokes, in the second number of the *Dublin Journal of Medical and Chemical Science*. He truly remarks, that in most of these accidents the powers of life sink so rapidly that bleeding, either local or general, cannot be attempted. Neither can we employ mercury internally, for fear of exciting the peristaltic action of the bowels, which action would tend to tear asunder recent adhesions, to keep the communication between the mucous and serous surfaces open, and to cause a fresh ingress of fecal or other extraneous matter into the sac. Yet in a few instances we find that the patients live for several days, and that a process of organization commences in the effused lymph. It seems that some years before Dr. Stokes wrote this paper, he had witnessed the admirable effects of opium in low forms of peritonitis, as administered by Dr. Graves; who thus saved, without abstracting a drop of blood, two individuals in whom that disease followed paracentesis. I cannot refrain from quoting to you the particulars of one instance, in which the efficacy of the opiate treatment was conspicuous. The well-known symptoms of perforation of the intestines had existed for two days; the patient was apparently sinking, "his countenance was collapsed, anxious, and expressive of dreadful suffering; the extremities were cold, and the pulse hardly perceptible. The exhibition of sixty drops in the twenty-four hours, of the preparation called the *black drop*, was followed by the most signal improvement. The pulse regained fulness and softness, the extremities became warm, and the countenance had lost the Hippocratic expression. The patient could bear pressure on the abdomen, which the day before was exquisitely painful. The same treatment was continued for twenty-four hours longer; and by the end of that time every symptom of abdominal inflammation had completely subsided. The belly felt natural, there was no tenderness, the pulse was good, and the patient declared himself well." At this period of the case, Dr. Stokes omitted the opium, and gave the mildest possible saline laxative, as there had been no stool for forty-eight hours. Four evacuations took place, followed by the immediate return of the symptoms of peritonitis, under which the patient rapidly sank.

"The intestines were everywhere agglutinated together, and adherent to the parietal peritoneum, except in the left iliac fossa, where a quantity of yellow puriform matter was collected. On detaching the caput coli from the peritoneum lining the right iliac fossa, a small perforation of the gut was discovered, by the escape of the contents of the intestines in a jet," &c. &c.

This example puts in a very strong light the *good* effects of *opium*; the *dangerous* effects of *purgatives*; and the *mode* in which recovery from these frightful accidents may sometimes be brought about.

Dr. Stokes gives another instance in which the patient *did* recover; after taking 65 grains of opium, besides what was administered in injections: and he alludes to third case, in which the employment of opium was successful, when peritonitis had supervened upon the bursting of an hepatic abscess into the cavity of the abdomen.

Now I would earnestly recommend you to consider the expediency of applying the

same principle of treatment, as an auxiliary, when the peritonitis does *not* grow out of previous organic disease: in all cases, in short, of *mere peritonitis*. The opium is not to supersede the bleeding, or the mercury; it is not incompatible with either of those remedies; and it may, I believe, be most advantageously adopted in conjunction with them both.

When the time arrives at which it may seem expedient that the bowels should be emptied, it is better, in the first instance, to employ enemata for that purpose, than to give purgatives by the mouth.

I shall relate one example, which has lately occurred to me, of the successful use of opium in simple, but severe peritonitis. Several of you saw this patient. His case was published in the appendix to Dr. Ferguson's volume.

H. Middlehurst, a tailor, seventeen years old, was admitted into the Middlesex Hospital on the 17th of September: looking very ill, and complaining of pain in the epigastrium, with extreme tenderness over the whole abdomen, which was full and tense. He had been ill several days, had shivered in the outset; and had vomited frequently, up to the period of his admission. His bowels were confined; his tongue was dry and white.

Twelve leeches were placed upon his abdomen, and calomel, in five-grain doses, was given two or three times at intervals of four hours. An enema of warm water was injected, and retained. In the evening sixteen more leeches were applied, and a drachm of mercurial ointment was rubbed into his arm.

I first saw him on the 18th. His countenance was then pinched and anxious, and he lay moaning with pain; his knees being drawn up towards his belly, which was tense, and exquisitely sensible to pressure. He complained of nausea and retching, but had not vomited since his admission. His tongue was thickly coated; his pulse small, sharp, 108 in number. No permanent relief had been obtained from the leeches.

I directed immediate venæsection; but not more than four ounces of blood could be got from the arm. Thirty fresh leeches were therefore put upon the abdomen, and afterwards a warm poultice to receive the blood from their bites. Three grains of calomel, and three of blue pill, were ordered to be given every four hours.

The last leeches mitigated the pain; but it returned in the evening with increased severity, and he vomited the pills. He appeared to be in great agony. In this state the apothecary gave him twelve grains of calomel, and five grains of opium, in one dose. Soon after this he fell asleep; and slept during the greater part of the night. Next morning his countenance had lost, in a great degree, its expression of anxiety; his belly was less tender, but still tense; and his tongue cleaner. No stool.

Capiat Pilulæ Saponis cum Opio gr. v. 8vâ. quâq. horâ.

On the 20th the bowels were freely open, the dejections dark and watery; the abdomen was less tender. Pulse 114. He continued to take a grain of opium thrice daily till the 3d of October: the bowels being every day moved; the pulse and tongue gradually improving; and the abdomen being painless even under firm pressure. On the 3d, as the bowels had not acted for the last two days, I discontinued the opium. On the 5th, diarrhœa set in, with some renewed tenderness of the belly; and the pinched and anxious countenance returned. He had then an opiate enema; and resumed the opiate pills as before. Under this treatment he at length got quite well; and left the hospital on the 30th.

Chronic peritonitis is sometimes merely the sequel of that acute form of inflammation of the peritoneum, which I have just been describing. Plastic lymph is effused, and becomes organized; serous fluid is poured out, and is not absorbed again; the products of the original inflammation remain; a low degree of inflammatory action perhaps remains also, or is re-excited by slight causes; the mischief augments; and the patient is slowly conducted to the grave.

There is, however, another, not at all uncommon, and equally formidable source of chronic peritonitis; the presence, I mean, of a multitude of little granules, lying within or immediately beneath the membrane, or occupying, in countless numbers, those folds of the peritoneum which compose the omentum. These granules occur principally, if not exclusively, in serofulous persons. Louis, indeed, who considers them to be *tubercles*, affirms that they are never met with in the peritoneum, without

being met with also—and usually in a more advanced state and greater abundance—in the lungs; but this rule is not universally true. I have seen more than one instance of well-marked granular disease of the serous membrane of the abdomen, without a single tubercle in the pulmonary tissues. Still the observation of Louis holds good in a vast majority of cases: and when we have symptoms of chronic peritonitis, which were not preceded by those of acute inflammation of the membrane, and when we perceive at the same time indications of phthisis, or of any other unequivocal form of scrofula, we shall seldom be wrong in connecting the chronic peritonitis with the presence of these miliary granulations. Whether they are truly scrofulous tubercles, or whether, as some suppose, they are *sui generis*, or again, simply minute spherules of coagulable lymph, I do not undertake to determine. I have been in the habit of regarding them as the cause, and not as the consequence, of the inflammation with which they are found associated.

The *symptoms* of chronic inflammation of the peritoneum are more obscure, in general, than those of the acute disease. And when the disorder is primitive, not the relics I mean of more active inflammation, it often begins, and steals on, in a very insidious manner. The patient complains of abdominal pains: sometimes slight, amounting to scarcely more than uneasiness, but abiding; sometimes occasional only. Usually there is a sensation of fulness and tension of the belly, although its bulk may not be sensibly altered. Sometimes there is a sense of pricking felt. Dr. Pemberton remarks, that you may detect a sort of deep-seated tension; that the skin and muscles lie loosely on the peritoneum, which gives to the hand a sensation as of a tight bandage underneath, over which the integuments appear to slide. The uneasiness, or the pain, is augmented by pressure; or perhaps is felt only when pressure is made. Sometimes the functions of the intestinal canal are disturbed: there are loss of appetite; nausea and vomiting; an irregular state of the bowels, and unnatural evacuations from them. Sometimes, on the contrary, the digestive organs perform their office in a tolerably healthy manner. These differences depend apparently upon the circumstance of the inflammation visiting, or sparing, the peritoneal covering of the stomach and bowels; and of the parts concerned in the secretion of bile. Sooner or later, in most cases, the abdomen enlarges; becomes tight, and tympanitic; and fluctuation is felt. All along there is some fever, more or less distinctly marked; with progressive emaciation and debility. The face is pale and sallow, and wears an expression of languor.

Very much the same set of symptoms are apt to result from scrofulous disease and enlargement of the mesenteric glands; and consecutive slow inflammation of the peritoneal membrane.

Accordingly, after death, we often find those glands swelled, and red, and hard; sometimes forming very large tumours: or we discover the whole surface of the membrane to be thickly bestrewed with innumerable small, round, greyish, or white granules: or it is seen to be covered, here and there, or everywhere, with false membranes. The intestines, full of air, are frequently agglutinated into one mass; or they are adherent to each other, or to the other parts of the peritoneum, in places only. The omentum is generally thick, red, and fleshy, as if its component parts had been matted together; and there is more or less fluid, commonly turbid and flaky, in so much of the cavity as happens to be left.

These are very unpromising forms of disease, and it is seldom that we can do more than mitigate the most distressing of the symptoms; or retard, perhaps, the march of the disorder. Leeches to the abdomen, in moderate numbers, and frequently repeated, and followed by soft warm poultices. Blisters, when the pain is not severe, and the tenderness less. Attention to the state of the bowels, which should be regulated by mild laxatives rather than by drastic purges. A nourishing, but unstimulant diet. These are the measures to which we must look for benefit. It has been thought that frictions upon the belly, with ointments containing iodine, have done good: so that it may be well to make trial of such. But do what we may, in nine cases out of ten, our best directed efforts will be disappointed.

When there is much fluid collected in the abdominal cavity in these cases, they take their character from this predominant symptom, and are called cases of *ascites*. But this is only one form of ascites—that form which results from chronic inflamma

tion of the peritoneal membrane. I shall pass, however, by an easy transition, to the other forms of dropsy of the belly.

There is another species of ascites, not very common, which approaches in its character to inflammation, and which is therefore called *active ascites*. I mean that we sometimes see persons, who were previously in good health, become rapidly ascitic, after exposure to cold and wet, and rapidly recover again under the remedies that are used to subdue inflammation. Perhaps it may be said that these *are* cases of inflammation; and it may be so. But they want many of the ordinary symptoms of peritonitis; and if inflammation *be* present, it has no worse effect than the effusion of serum, which, under depletion or mercury, is speedily taken up again. I should rather conceive, however, that these cases are to be included in that category of dropsical effusions which I spoke of formerly, as resulting from the detention in the blood, or from the absorption into the blood, in the first place, of an undue quantity of watery fluid, and its subsequent discharge, by a kind of secretion, either into shut cavities, or through some one of the natural vents of the body. The balance of the circulation between the skin and the internal surfaces appears to be destroyed on these occasions, by the operation of external cold upon the tegumentary membranes.

But by far the greater number of cases of ascites are cases of passive dropsy which arise slowly from a mechanical obstacle to the free return of the venous blood towards the heart.

Ascites occurs, as you know, in general dropsy, with anasarca of the universal areolar tissue; and this general effusion of fluid depends, in almost every case, either upon a peculiar renal disorder, or upon organic disease of the viscera of the thorax; of the lungs, or of the heart, or of both; and, above all, upon such disorder as is attended with dilatation of the right chambers of the heart.

But I exclude this form of ascites, wherein the dropsy of the belly is only a portion of more general disease of the same kind, and limit myself at present to that kind of passive ascites which is unattended with dropsy elsewhere; or which at any rate precedes the occurrence of serous accumulation in other parts.

The symptom which first leads us to suspect ascites, is the progressive enlargement of the abdomen. But the abdomen may grow gradually large and prominent when there is no disease whatever: in pregnancy, for example; or in mere obesity. It is necessary therefore to search for more definite signs of peritoneal dropsy.

In order to make an accurate diagnosis of ascites, we must know what are the morbid conditions with which it is most liable to be confounded. Solid tumours and simple corpulency are readily enough distinguished. But there are certain kinds of *encysted dropsy* of the abdomen, of which the recognition is not so easy and obvious. Of these what is called *ovarian dropsy* is the chief. In some of its symptoms this complaint closely resembles ascites: in some it differs from it widely. So also the treatment of the two disorders is alike in some respects; dissimilar in others. For these reasons, and because I am more solicitous to be practical than to be methodical, I shall consider these two maladies together; turning first to the one, and then to the other, and marking, as I go on, the various points of similitude, and of contrast, which they mutually offer.

Recollect that ascites signifies the accumulation of serous liquid in the bag of the peritoneum; whereas ovarian dropsy consists in the collection of fluid in one or more cells within the ovary; or in a serous cyst connected with the uterine appendages.

One source of distinction between the two is furnished by the condition of the abdomen during their early stages.

In ascites the enlargement is uniform and symmetrical, in reference to the two sides of the body. When the patient lies on her back the flanks bulge outwards, or swag over, from the weight and lateral pressure of the augmenting fluid. This increased *breadth* of the trunk is not observable in the case of an ovarian tumour; nor, I may add, in pregnancy.

When we are able to trace the early history of ovarian dropsy, we find, in most instances, that the abdominal tumour was first perceived on one side; in one or the other of the iliac fossæ, or somewhere between the ribs and the ilium. But when the enlargement of the abdomen is great, the distinction between ascites and encysted dropsy, drawn from the shape of the swelling, fails. The ovarian tumour distends

the abdomen, if not uniformly, yet nearly or quite as much on one side as on the other.

The next thing that we do, when the *visible* bulk and shape of the abdomen have suggested a suspicion of ascites, is to employ the sense of *touch*.

Examination by *pressure* will sometimes suffice to assure us that there is fluid in the peritoneum. If you press suddenly with the tips of your fingers, in a direction perpendicular to the surface, you will often become aware of a sensation which it is difficult to describe in words, yet which is quite decisive, and not to be mistaken; a sensation of the displacement of liquid, and of the impinging of your fingers upon some solid substance below. So that, by this manœuvre, you frequently detect, not merely the presence of the liquid, but an enlarged liver, or spleen, or (it may be) an ovarian or other tumour; even when simple palpation, or handling in the ordinary way, would not enable you to ascertain these enlargements.

Again, *percussion* of the abdomen is fertile of information in these cases. First, by the sense of fluctuation which it causes when liquid is collected within. The left hand being laid flat against one side of the tumid abdomen, if a slight blow be struck with the fingers of the right upon the opposite side, the impulse is conveyed by a wave of the liquid to the open flat hand, which feels a little shock that is perfectly distinctive. The larger the amount of the accumulated liquid, and the thinner and tighter the walls within which it is confined, the more sensible and decided is this fluctuation. Even when the quantity is small, not exceeding a few ounces, a little practice and management will enable you to detect it. Percuss with one finger the most depending part of the cavity, and apply at the same time a finger of the other hand, very near the part struck; and if liquid be there, you will perceive a limited yet distinct fluctuation. In the same way the presence of liquid in a small cyst may sometimes be ascertained. Much more when the cyst is large. And the cyst, in ovarian dropsy, is often very large: and the liquid it contains is often thin and aqueous; and then the fluctuation may be quite as perfect and perceptible as ever it is in ascites.

Hence mere fluctuation is not a discriminating symptom between ascites and ovarian dropsy.

But, secondly, percussion is full of instruction in the *sounds* it elicits. The sense of *hearing* will generally supply what the sense of touch may leave wanting.

In true ascites the relative place of the liquid and of the intestines is determined by the posture of the patient. The bowels, which always contain some gas, float to the upper part of the liquid, and there give out (when the finger, as a pleximeter, is applied to the corresponding surface, and struck) their peculiar resonance. Mediate percussion will thus follow the gravitating fluid, and discover always a dull sound in the lowermost and a hollow sound in the uppermost part of the abdomen.

But it is not so in ovarian dropsy. The cyst, in a diseased and enlarging ovary, rises in front of the intestines, which, being tied down by the mesentery, cannot embrace the tumour so as to reach its anterior aspect, but are in fact pressed back by it towards the spine. Hence, if there be any resonance produced by percussion, it is in one, or the other, or in both of the flanks; and the umbilical region yields a dull sound whatever the position of the patient may be. The same is true of the enlarging womb in pregnancy.

This simple expedient, then, is quite decisive. In ascites, the patient being supine, the epigastric and umbilical regions are tympanitic on percussion; in ovarian dropsy the latter, at least, is dull. To be quite sure it is well to make the patient assume different postures in succession. If the person affected with ascites turn upon her side, the uppermost flank will become resonant; the umbilical region dull: whereas in ovarian dropsy, the sounds under every change of position remain severally where they were. In ascites, with a little care, you may ascertain the exact level at which the contained liquid stands; and measure its rise or fall from day to day.

This mode of diagnosis is scarcely open, under ordinary circumstances, to fallacy, or exception. Yet there are two or three possible conditions in which it may fail; and these it is right that I should briefly mention.

1. The distension, in true ascites, may be so great, that the mesentery shall not be broad enough to allow the buoyant intestines to reach the surface, when the patient is supine. This impediment to the efficacy of the proposed test I have met with in

practice. A woman came under my charge in the hospital with ascites. Fluctuation of the belly was unequivocal. While she lay on her back, the umbilical and epigastric regions were resonant when percussed; the flanks were dull. When she turned upon either side, the other side, previously dull, gave the hollow sound; the umbilical and epigastric regions, previously resonant, gave the dull flat sound. Under the treatment employed, the accumulated liquid was removed, and she left the hospital.

Some time afterwards, as I was going round the wards, I recognised the same woman among the patients recently admitted by my colleague, Dr. Hawkins. The ascites had returned. The abdomen, enormously distended, projected upwards, as she lay on her back, to an excessive height. I found that fluctuation was very distinct, as before: but every part of the belly yielded a dull sound when struck by the fingers. At length this patient died: and it was seen, after death, that there was nothing to prevent the rising of the intestines. They had floated, at the utmost tether of the mesentery, as high as they could, without reaching the surface of the prominent belly.

2. Another occasional source of fallacy I have just now hinted at. The intestines may be tied down, and so prevented from ascending, by their specific lightness, to the upper part of the surrounding liquid. And this may happen, either in consequence of the adhesion of the various coils of the intestines to each other, and to the parts behind them; which is not an uncommon occurrence:—or the intestines, though unadherent, may be swathed, as it were, and bandaged down, by a thickened and diseased omentum. This also I have myself seen. A man died in the hospital, who had manifest ascites. Yet his whole abdomen, though not so much distended as to hinder the intestines, had they been free to rise, from reaching its walls, sounded dull on percussion. Inspection of the body explained this circumstance. When the peritoneum was opened by an incision carried through the fore-part of the abdomen, a quantity of serous liquid gushed out. The floor of the cavity which it had occupied was smooth and level; and was found, on further examination, to be formed by a thick cake of omentum, strapped tightly over the subjacent intestines. Of course, the same diseased condition may occur in the female.

3. On the other hand, I have once known an ovarian cyst to exist, when the umbilical region was tympanitic under percussion. The case furnished just that kind of exception which serves to prove a rule. This also was a hospital patient. Her *history* was the history of ovarian dropsy. Some time previously she had discovered a small tumour in one of the iliac regions. It increased without much disturbance of her general health, until it became very inconvenient from its bulk. She was then tapped in one of the Borough hospitals: and she stated distinctly that it was not a clear watery fluid that was evacuated; but a glutinous, mixed, and grumous matter: such as belongs to ovarian disease. No doubt could be entertained that the enlargement of the abdomen resulted from disease of that kind. Yet the umbilical region, when percussed, always rendered a hollow sound. Upon the death of the patient the mystery was solved. Air hissed forth from the opening made by the scalpel through the abdominal parietes: and the source of it being traced, an ovarian cyst, of considerable magnitude, was found adhering to the peritoneum in front of the belly, and containing no liquid, but some yellowish shreds only; the remains, apparently, of some smaller included cysts. This ovarian bag had been filled with air, and had given occasion to the equivocal sounds.

Another way in which air may sometimes get into an ovarian cyst, and perplex the diagnosis, is through an opening of communication between the cyst and a portion of adherent bowel.

These sources of possible mistake or obscurity very seldom occur; and the physical diagnosis, as I have now pointed it out, is very sure and valuable. So completely physical, indeed, are these tests, that we recognise ascites by them as readily and certainly in the dead, as in the living body.

Other points of distinction may frequently be derived from the history and progress of the two disorders.

The equable enlargement of the abdomen, on both sides, in ascites, and its unequal prominence on one side in the early stages of ovarian disease, I have already mentioned.

Again, it is observable that in true ascites, there are almost always manifest indications of constitutional suffering and disturbance: a sallow complexion; debility; ema-

ciation. The morbid accumulation results (as we shall see) from disease in some organ, of which the functions cannot be impaired without injury to the whole system.

Ovarian dropsy, on the other hand, may last long, and be extreme in degree, while the general health is scarcely affected. The very bulk and weight of the swelling produce, indeed, much inconvenience and discomfort; but, in other respects, the patient often remains in good health. This appears to be owing to the circumstance that the ovary is not directly necessary to the life or well-being of the individual, but is merely subservient, for a limited time, to the purpose of reproduction.

Among the symptoms that are common to ascites and ovarian dropsy in their advanced stages, are all those which are occasioned by weight and pressure: viz. shortness of breath, from the resistance opposed to the descent of the diaphragm; anasarca of the legs and thighs, from pressure upon the inferior cava and its branches; and a peculiarity of carriage and gait, like those of a woman big with child, and depending upon the same cause, the necessity of throwing the head and shoulders backwards, to balance the weight of the distended abdomen in front.

It is necessary to caution you against mistaking a distended bladder for dropsy of the abdomen. An old Frenchman was brought into the Middlesex Hospital, afflicted (so his friends said) with dropsy. He had been treated for that complaint. The abdomen was large, and dull under percussion from the pubes to above the umbilicus. In the hypogastric region I could detect an obscure sense of fluctuation. I noticed a strong smell of urine about this patient. Being interrogated, he said that he had formerly had some "stoppage," but that he now passed plenty of water; that it even ran from him. It was obvious that his bladder was enormously distended, unable to contract upon its contents, and overflowing. With some difficulty a catheter was introduced, and some quarts, I forget the exact quantity, of turbid and stinking urine were drawn off. The patient sank at length, and the bladder was found to be much diseased. I have known several similar mistakes to occur in private practice. You will not think the caution I am now giving you superfluous, when I tell you, on the authority of Sir Everard Home, that no less a surgeon than John Hunter once actually tapped a distended bladder, in the belief that the disorder was ascites.

Encysted dropsy, in the abdomen, is not always *ovarian dropsy*. *Omental dropsy* is described: the omental cavity alone being unfolded, and full of liquid. This I have never seen. Cysts containing a considerable quantity of a clear thin liquid, and connected with the *liver*, are common. Probably these are in all cases (they certainly are in many) the effects of the growth of hydatids. Dropsy of the *fallopian tubes*: dropsy of the *uterus*: large serous cysts in the *kidney*: constitute other forms of abdominal encysted dropsy. Such states must be discovered by their own particular circumstances. None of them are very common.

LECTURE LXVII.

Pathology of Chronic Ascites; of Ovarian Dropsy. Treatment of these two disorders. Internal remedies: Extirpation of the ovarian sac: Paracentesis Abdominis.

IN my last lecture I pointed out the means we possess of distinguishing ascites from ovarian dropsy. Continuing the parallel between these two disorders, I have till to consider their pathology; and to prescribe their treatment.

I mentioned that chronic ascites is sometimes the sequel of acute inflammation of the peritoneum. In such cases, the abdomen is usually uneasy, and tender under pressure; or, at any rate, more than commonly sensitive: and I believe more than commonly *hot* also. Whereas when ascites is passive as well as chronic, you may make the requisite examination without causing any distress to your patient. There is no pain produced by palpation, by percussion, or by pressure. Even when the

dropsy has resulted from bygone inflammation, it does occasionally happen, though rarely, that no other trace of such inflammation is discoverable in the *living* patient. The absorbing function of the membrane having, however, been spoiled, the collected liquid remains. Such a condition, I believe, I have witnessed. The history of sudden and sharp pain, and tenderness of the abdomen, with fever, immediately before the dropsical swelling took place, made it probable that it was the consequence of inflammatory effusion. But the fever had entirely subsided; no tenderness was left; no large veins were visible on the surface of the belly, denoting internal obstruction; and the general health was good. The patient had no other dropsy.

The main exciting cause, however, of true and uncombined ascites, when no inflammation is, or has been, at work, is some impediment to the venous circulation in the abdomen. Whereabouts, and of what kind, is this impediment? That is the question which, in each particular instance, we ask ourselves.

The old doctrine respecting the causes of ascites, vaguely referred the collection of liquid to *obstruction*; and to *organic diseases* of the abdominal viscera; and, above all, to *hepatic* disease. But as we are now better instructed, and know that organic diseases produce the dropsy, ultimately, by retarding the flow of blood through the system of the vena portæ, we see that the truth was only half perceived by the ancient pathologists. We can now understand why some organic diseases of the abdomen lead to dropsy of the peritoneum, and others (even of the same viscus) do not. And we have no difficulty in comprehending why, of all the abdominal viscera, the *liver* is the one of which the diseases are the most frequently connected with ascites; that gland being traversed by the converging branches of the venous trunk, through which passes by far the greatest part of the serosity absorbed from the surface of the vast membrane that inwraps most of the abdominal organs, and lines the cavity containing them. It is plain that an accumulation of serum in the peritoneal sac may arise from a mechanical obstruction in the trunk of the vena portæ, or in some of the principal branches that unite to form that vein; or from certain diseases of the liver itself. But we know that disease of the liver is of very common occurrence, and oftentimes very obvious, while there is no ascites. And a further question arises—With what kind of disease of the liver is hepatic ascites most apt to be associated?

In truth, there is one special form of liver-disease which, though not the sole, is the grand cause, of passive and simple ascites. It has long been noticed that mere enlargement is not the most common condition of the liver met with in hepatic dropsy; but rather the small, hard, contracted viscus. Mere increase in the size of the organ may interfere but little with the portal circulation; whereas a shrinking and diminution of its bulk must needs do so. In point of fact, that particular state of the liver which the French have termed *cirrhose*, and which is familiar to morbid anatomists in this country as the *hobnail* liver, is the great source of passive ascites.

The true character of this remarkable condition of the liver is of modern discovery. The credit of correcting the erroneous opinions which had been entertained respecting it is due, I believe, to Mr. Kiernan. The change undergone by the organ has also been clearly described by Sir R. Carswell; whose delineations of its physical appearance are now before you. The change results from chronic inflammation, and chronic thickening (miscalled hypertrophy), of Glisson's capsule. Since Mr. Kiernan's admirable exposition of the minute anatomy of the liver has been given to the world, few can be ignorant that the areolar tissue, termed the capsule of Glisson, accompanies the portal vein, the hepatic artery, and the biliary ducts; and forms a sheath around these vessels in their course through the liver; while the hepatic vein and its branches are lodged in the proper substance of the gland without any such investing membrane. It follows that a general thickening of this tissue must produce a general pressure upon the portal veins, large and small, and hinder the return of the venous blood from the intestines. Hence, as in analogous cases, congestion of the capillaries, arrested absorption, mechanical transudation of serous liquid. The pressure affects also the nutrient vessel, the artery of the liver; so that, in most instances, there is atrophy and shrinking of the organ; and occasionally, though rarely, jaundice also ensues, from pressure upon the biliary vessels. By degrees, the areolar tissue itself begins to shrink; and the spaces in which it ramifies on the surface of the liver are pulled inwards; the lobules appear to be prominent; and the sur-

face becomes irregular and knobby, and studded with little roundish eminences like the heads of nails. The constricted lobules are likewise very conspicuous in the cut surface of the liver: appearing like a congeries of peas, of a pale yellowish colour.

In the living body the presence of this hepatic disease is, for the most part, a matter of inference only. It is rendered probable by its ascertained frequency in connexion with ascites, and by the absence of any other obvious cause for the dropsy. But sometimes the irregular surface can be felt through the walls of the abdomen.

The nature of this morbid change affords a reason for the intractable and unpromising character of ascites in general. The obstructed blood seeks indeed new channels; but the compensation they furnish is rarely sufficient. The portal blood is diverted towards the vena cava and its tributaries. The superficial veins become obvious, numerous, large; and wander with many inosculations over the surface of the belly. Large veins, significant of the same compensating effort, have been met with also in the adhesions which previous inflammation had left between the liver and the diaphragm.

Among the causes to which the thickening of the capsule of Glisson may be ascribed, habitual intemperance holds the chief place. The diagnosis will be assisted therefore by our knowing that the patient has been a spirit drinker. The liver in question is the true gin-drinker's liver. It must arise from other causes also, for we sometimes meet with it in the bodies of children, and of adults who had always lived temperately: but in ninety-nine cases out of a hundred, it is traceable to the repeated operation of the poison of alcohol. In dogs that had been destroyed by this poison, Dr. Percy recovered alcohol from the blood, the brain, and other parts, but most of all from the liver; and Dr. Budd, who has exhausted the subject of diseases of that organ, concludes that the alcohol, absorbed into the veins, is conveyed at once to the liver, and exercises a direct action upon its tissues.

But this condition of Glisson's capsule, though it is the principal, and by far the most frequent, is not the only cause of obstruction to the current of the blood in the portal vessels, and of consequent ascites. In those specific forms of liver disease in which separate tumours are scattered through its substance, one of these tumours may be so placed as to press upon the trunk of the vein. So, obviously, may abdominal tumours of any kind; enlarged mesenteric glands; cancer of the pylorus; cancer of the head of the pancreas; and the like.

Ascites is found to be not unfrequently associated with disease and enlargement of the *spleen* also; but in most instances of this kind, the enlargement of the spleen and the peritoneal dropsy are not connected as cause and effect; but are both consequences of portal obstruction.

When, after death preceded by ascites, the cavity of the abdomen is laid open, its contents present a bleached and sodden appearance. It has been made a question whether this be the result of the long-continued immersion of the living tissues in the accumulated water; or of their short maceration after death. The question has no practical importance.



FIG. 101.

Incipient cyst-formation. The ovary is represented of the normal size.

Dropsy of the ovary in its ordinary form, consists (I believe) in disease and enlargement of one, or more, of the Graafian vesicles; or of the ova which they inclose.

The actual condition of the dropsical ovary is subject to much variety. Sometimes there is but one cyst; and this may be no bigger than a pea; or it may be large enough to contain many gallons. Its walls may be as thin and flexible as those of the healthy urinary bladder; or they may be firm, and half an inch or more in thickness. It may spring from a small pedicle, and lie free and otherwise unattached in

the cavity of the peritoneum; or it may adhere, partially or at all points, to the contiguous surfaces; or it may be tied and tethered by bands of coagulable lymph. Its inner surface may be smooth and even, or knobby and irregular. Lastly, the fluid contained in the cyst may be thin, or consistent; limpid, or glutinous; opaque, or transparent; and of various tints; so that, in different cases, it may be colourless, green, purple, red; and more or less resemble in appearance, pure water, white of egg, jelly, glue, birdlime, or treacle. Most commonly, however, when the cyst is single, its contents are thin and aqueous.

Again, the dropsical ovary may be multilocular, composed of many cysts, which are usually distinct, but which sometimes communicate together; and these cysts, in the same ovary, while they vary much in size, may differ also from each other in any or in all the particulars just enumerated as being incidental to a solitary cyst.

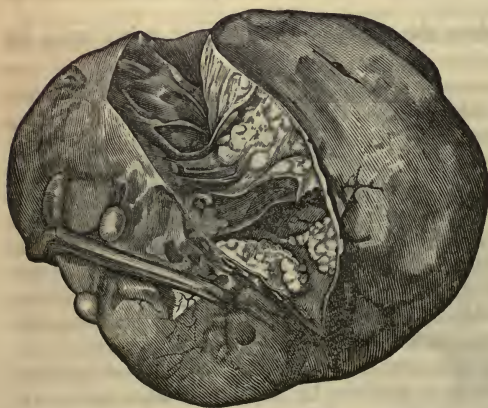
Commonly one of the cysts is much more capacious than the rest; and some part of its inner surface is frequently embossed, as it were, by the projecting outline of a group of small nodules, which seem to lie within the parietes of the larger cyst, but which, in truth, are cysts of similar origin with it, but of more stunted growth.

The external surface also of the multilocular ovary is generally lobulated; and its inequalities may often be discovered by a careful examination of the abdomen in the living subject.

Sometimes the tumour is solid throughout; in which case the term dropsy is altogether misapplied.

These differences are not without occasional importance, in reference to some points in the treatment of the disease.

FIG. 103.



A multilocular ovarian cyst, removed from a female, æt. 29, during life, by Mr. J. B. Brown. Septa form larger compartments, in which there is a secondary and tertiary growth of cysts. The tumour weighed 11 lbs. 3 oz.

The *progress* of ovarian dropsy is no less wanting in uniformity. Sometimes it is very rapid; sometimes it is very slow. It may destroy life in a few months; it may continue, a mere burden, with scarcely any fatal tendency, for many years. Not unfrequently, after a period of active increase in the tumour, the morbid process, without any obvious cause, suddenly stops: and the pause may be final; or, after an uncertain interval, the disease may resume its former activity.

Under all circumstances the malady is a serious one; for its possible grievances are many; and its issue is precarious and unpromising. Although, in some cases, the general health for a long time is but slightly, or not at all impaired, in others the disease runs a short course; the tumour increasing rapidly and proving ultimately fatal by its bulk and pressure; or embittering and abridging the unhappy patient's existence by some accident of growth or position. Even when of no vast magnitude, it may be so

situated as to impede or prevent the expulsion of the fæces from the bowel, of the urine from the bladder, or of the fœtus from the gravid uterus.

The single cysts, having thin parietes, and containing a serous liquid, are not always produced by disease and distension of a Graafian vesicle; for they sometimes have no connexion with the ovary, but spring from some other part of the uterine appendages.

Neither, perhaps, can it be *demonstrated* that the complaint originated within the Graafian vesicles, when it exists in its more complicated form; when the cysts are many, and their contents various. But the shape of the cysts, which is more or less spherical, their number, their isolation in most cases, and the diversity in the matters by which they are filled, render this view of their origin at least a probable one.

This form of the disorder has been considered as belonging to the category of *malignant* diseases; but, in my judgment, without sufficient reason. It is true that the tumour does sometimes involve one or more of those morbid conditions, which have been denominated scirrhus, fungus hæmatodes, cerebriiform disease, or melanosis, and which all, or nearly all, appear to be varying results of the same morbid process, and to be referable to the genus *carcinoma*; but whenever this is observed to be the case, other structures also are found to be infested with analogous changes. The so-called malignant disease occupies the ovary in common with other parts; and this is one of its most constant characters, namely, that proceeding from some vice in the constitution, or disseminated from some local germ, it pervades different organs of the body at the same time, or in succession; whereas in by far the majority of instances of ovarian dropsy, these peculiar products are met with neither in the diseased gland, nor in any other place. It has already been remarked that many women, labouring under ovarian dropsy, enjoy nevertheless in all other respects very good health, even for many years. The victims of malignant disease are not so fortunate. *They* either are soon cut off, or, if they linger, they seldom fail to exhibit, in their complexion and general condition, notable indications of the mischief which is in progress, and gradually undermining the powers of life.

If it be admitted, as a reasonable conjecture, that the Graafian vesicles, or the ova they contain, are the seat of the primary changes, we may push our speculations a little further. These ova are destined, under the peculiar stimulus of impregnation, to build up the fabric of the body in all its parts and qualities. And we may suppose that, in consequence of some unnatural and morbid stimulus, perverted and erring action may be set up, and strange products result. It is not uncommon to find fat, hair, cholesterine, teeth, and other bones, in the diseased ovaria, even of virgins.

This view of the matter is strengthened by the fact, that dropsy of the *ovary*, of the ordinary kind, has not been known to *commence* before the age of puberty; nor often after the capability of child-bearing had ceased; but only, or chiefly, during that period in which the organ, if healthy, is susceptible of its proper and temporary function. Virgins, and barren and fruitful wives, are alike subject to the disease; but in what relative proportions, statistical inquiry has not yet (so far as I know) determined. Where it accompanies, it may also account for, sterility.

The catamenia during the progress of the malady sometimes appear with more or less of punctuality and quantity; sometimes are entirely suspended. This function is so often interrupted under other circumstances, that its derangements shed but little light upon cases that are otherwise obscure. When the discharge continues to recur, we may presume that one, at least, of the ovaries is in a tolerably healthy state: when both are sensibly diseased, the catamenia may be expected to be wanting.

The *treatment* of these two forms of abdominal dropsy must, up to a certain point, at which the operation of tapping becomes expedient, be considered separately. Of both it may be said, that their cure is seldom accomplished; yet, for reasons already assigned, ascites has, upon the whole, a more certain progress towards the destruction of life than ovarian disease: while, perhaps, it is oftener cured.

In passive ascites, when the distension of the peritoneum has crept on without pain, fever, or other marks of inflammatory action, our first and best hope of evacuating the collected fluid will rest upon diuretics. Hepatic ascites and renal disease may be sometimes found in conjunction, but according to my experience, they seldom are so: and except that both may probably owe their occasional origin to habits of

intemperance, there appears no reason why they should be. Diuretics may be administered, therefore, without scruple. The hydragogue purgatives are to be employed, also, when diuretics fail to act, or to reduce the swelling; and when the disease is not already complicated with diarrhœa. And inferring with more or less certainty the existence of hepatic disease—sometimes from palpation of the enlarged or altered liver, sometimes from the coincidence of jaundice, and from the history and habits of the patient, but most of all from the result of accumulated experience respecting the morbid anatomy of such cases—we give the patient the chance of the remedial influence of mercury. The disorder being chronic, the introduction of that drug should be gradual. The iodide of potassium is held by some physicians to be especially serviceable in hepatic ascites. Compounds of mercury and iodine may be applied, by inunction, to the surface of the abdomen, and to the right hypochondrium in particular. In Germany, the muriate of ammonia is in much repute as a therapeutic agent. This diuretic salt, though seldom administered internally in this country, is believed by some practical men who have employed it, to exercise the same beneficial influence upon the functions of the liver, as is commonly attributed to preparations of mercury; while it is less productive of distress or inconvenience. My own experience upon this point is too limited to justify me in expressing any confident opinion about it; but in some recent instances I certainly have noticed a remarkable improvement in the character of the biliary excretion, after the daily exhibition of sal-ammoniac combined with the extract of taraxacum.

You will generally be obliged to try, in their turn, all the diuretics within your reach, and frequently to no purpose. Our efforts to remove by medicine the accumulated liquid, or to cure the morbid condition on which the accumulation depends, are too often made in vain. The distension of the peritoneum continues to augment; the distress therefrom arising becomes urgent and extreme; and at length, to afford temporary ease to the patient, and in the faint hope also of giving him permanent relief, we resort to the mechanical expedient of *paracentesis*.

When we have the opportunity of treating *ovarian dropsy* from its commencement, we sometimes find that the enlarging ovary is painful, or tender. This is an indication for *antiphlogistic* measures. But from such remedies, or from any remedies, little more than temporary relief is to be expected. My position as physician to a hospital, has brought under my notice many cases of ovarian swelling, at a very early period of its development; when all that could be detected by careful examination of the abdomen was a small tumour, not larger, perhaps, than an egg, and occupying the situation of the ovary; to which tumour the attention of the patient had been drawn by some pain or uneasy feeling in that part. I have treated such cases assiduously, with the remedies of chronic inflammation, frequent topical bleedings, and the use of mercury till the gums were affected: with the remedies of ordinary dropsy, diuretics and drastic purgatives: and with remedies accounted specific; the liquor potassæ, the various preparations of iodine: and I must honestly confess to you that I am unable to reckon one single instance of success. Yet these are the measures that we are bound to try. They have succeeded—as we are assured by competent and credible witnesses: they may therefore succeed again. The amount of my own experience, however, tends to the persuasion that medicine has, in general, very small influence over the progress of this disorder. The cases that do well, do well we scarcely know how or why; the cases that prove fatal run their course in spite of us.

Sometimes, as has been stated, these ovarian tumours reach a certain magnitude, and then (wherefore we cannot tell,) enlarge no more; but remain, a mere inconvenience and deformity, for many years. Occasionally, either spontaneously, or in consequence of some accidental violence, they burst into the cavity of the peritoneum, whence the effused fluid may be absorbed; but more commonly it causes fatal inflammation. Or the bursting tumour may empty itself harmlessly (adhesion having previously taken place) through some channel of communication with the bowels, with the vagina, or with the bladder; or externally through the parietes of the abdomen.

Tumours, supposed to be ovarian, do sometimes disappear entirely. It may, however, be doubted whether all, or even many, of the enlargements which have had this fortunate issue, were really connected with the ovary. One source of mistake I have myself more than once encountered, and I believe it to be not uncommon. A

brief statement of the circumstances under which I first observed the fallacious symptom, will show you at once what I mean. Some years ago I was sent for by a lady, who for many days had been labouring under an ordinary attack of continued fever. While examining the abdomen by pressure, I discovered, on the right side, between the ilium and the umbilicus, a round, hard, painless tumour, as big as a swan's egg. The patient was aware of it; and thought it had existed for some time. At the next visit it was gone. In the interim, very abundant discharges from the bowels had followed the administration of purgative medicine. The tumour had obviously been formed by the accumulation of faecal matters in the cæcum.

Similar collections take place, less frequently, on the left side, just above the sigmoid flexure.

The parts concerned in this disorder are not essential to life, or to the enjoyment of health. On some of the lower animals, the operation of spaying is as customary in the one sex, and is performed with as little risk, as that of castration in the other. The ovaries have in several instances been extracted from the living human body without any ill consequences. These facts, and the intractable character of the disease, have naturally suggested the expediency of extirpating the tumour in cases of ovarian dropsy.

But although the ovary, when healthy, or when not greatly enlarged, may be removed without much difficulty or hazard, the operation becomes always perilous, and often impracticable, when the altered gland has attained any considerable magnitude. Yet these are the very cases for which the remedy is needed. A large ovarian tumour is usually multilocular, with firm parietes, and thick internal septa; and is therefore incapable of collapsing much when punctured. To extirpate such a tumour, the abdomen must be (as it frequently has been) laid open from the sternum to the pubes. Most commonly, also, a large ovarian swelling is adherent to the contiguous parts; a circumstance which either makes the proposed removal of the tumour impossible, or, if the connexions admit of being broken down, augments in a fearful manner the jeopardy of the patient. It is not surprising, therefore, that the results of experience have been so discouraging as well nigh, in most minds, to prohibit such attempts in future. The operation has, indeed, in many authentic cases, been quite successful. In other instances, the surgeon, after exposing the adherent mass, has been fain to replace the effused bowels, and to sew up the abdomen, as speedily as he could; and the patient having suffered all this in vain, has sometimes been fortunate enough to escape with life; but not always. In others, the adhering tumour has been separated, and the object achieved; and the woman has lived thereafter for some hours. Three times, at least (one of the cases is recorded by Mr. Lizars, another by Mr. King, the third fell under the cognizance of Dr. Richard Bright), the abdominal muscles and the peritoneum have been slit open, for the purpose of extracting a diseased ovary, when no disease existed.

But of late, a modification of the process of excision—whereby it is adapted to certain forms or conditions of the disease—has been proposed and practised; and holds out somewhat more of promise.

It consists in making, not a long, but a small incision through the walls of the abdomen, and through the peritoneum, so as to bring the surface of the diseased ovary into view. The cyst is then secured, by means of a tenaculum, or of a ligature, from receding inwards, and punctured, and its contents are suffered to escape. When the sac has emptied itself, it is withdrawn through the external orifice by gentle traction, until its stalk or place of attachment to the broad ligament, comes near the wound. A thread is tied round this stalk, the cyst is cut off, the uterine appendages are put back into the cavity of the abdomen, and the lips of the wound are brought together.

Although this method does not appear to have been actually *performed* till recently, it had been *suggested* as long ago, at least, as the time of Dr. William Hunter, who, in a paper on the disease, has these remarks:—"If it be proposed, indeed, to make such a wound in the belly as will admit only two fingers or so, and then to tap the bag, and draw it out, so as to bring its root or peduncle close to the wound of the belly, that the surgeon may cut it without introducing his hand, surely in a case otherwise so desperate, it might be advisable to do it, could we beforehand know that the circumstances would admit of that treatment."

In these few words, Dr. William Hunter not only describes the mode of excision, but alludes to circumstances that may render it inadmissible, as well as to the uncertainty that may arise about the existence of those circumstances. What the circumstances are is sufficiently obvious.

However, this suggestion has been carried into successful practice by Mr. West, Mr. King, and others. To an interesting paper on the subject, published by Mr. Gorham, in the *Lancet*, is appended a summary account of ten cases; by which account it appears, that five of the ten patients were cured by the operation; two recovered from the attempt to extract, which was unsuccessful; two died very soon after the operation, and evidently in consequence of it. The remaining patient, whose case has already been adverted to, recovered also from the incision; but there was no diseased ovary to amputate: so fallacious, sometimes, is the diagnosis. The tumour which she had presented to the notice of the surgeon was what has been called a *phantom* tumour.

This mode of operating merits careful consideration, and further trial. Its recommendations are—

1. That the first steps—the incision and the puncture—are the same in kind as the first steps in ordinary paracentesis, and not much more severe or dangerous; the only difference being that the incision requires to be somewhat larger in the one case than in the other.

2. That, when successful, it affords a complete and permanent cure, which can hardly be hoped for from any other plan.

The objections to its general use are—

1. That the single sacs, with thin and flexible walls, do not, commonly, reach a very large size. When small, or of moderate dimensions, they produce so little distress or inconvenience, that an operation, which must always be uncertain and tentative, ought not to be recommended.

2. That the multilocular tumours, with solid walls and partitions, can scarcely be so drawn through the opening made into the abdomen.

3. And, above all, that adhesion of the tumour to the neighbouring parts would interfere with and frequently prevent the success of the operation.

In most, however, of the five cases of cure referred to by Mr. Gorham, the sacs thus removed were large. From one of them 12 pints of fluid were let out; from another, 27 pints; from a third, upwards of 20; and from a fourth, 24 pints.

It is difficult to guess beforehand whether the tumour be attached to the parts surrounding it or no. If it be readily moveable by the fingers applied to the surface of the belly, so as to admit of being pushed hither and thither without pain or distress to the patient, it is probably unadherent. In one of the same five cases, the patient being in labour, Mr. West found that the tumour lay between the uterus and the brim of the pelvis. By gentle pressure, *per vaginam*, it was made to recede into the cavity of the abdomen. This afforded a presumption that it was free from adhesion; accordingly the sac was afterwards easily excised. The tumour is attached anteriorly to the peritoneum lining the front of the abdomen more frequently than to any other part. This Mr. Gorham attributes to partial inflammation produced by the puncture in the operation of tapping; hence the presumption that the cyst is loose is, *cæteris paribus*, the stronger, when paracentesis has never been performed.

If this mode of excision should be thought advisable, it ought to be attempted while the tumour is still of moderate bulk, yet enlarging, before the peritoneum has been much stretched, and while the likelihood of adhesion is the least. When any distinct history of bygone peritonitis can be traced, the existence of adhesions may be confidently reckoned upon.¹

¹ Since this lecture was first printed, an interesting paper by Mr. Phillips upon the question of extirpating ovarian cysts has been published in the 27th volume of the *Medico-Chirurgical Transactions*.

Mr. Phillips exhibits, in a tabular form, “the results of 81 operations, performed for the purpose of extracting ovarian tumours. In 61 cases the tumour was extracted; in 15 cases adhesions, or other circumstances, prevented its removal; in five instances no tumour existed. Of the cases in which the operation was completed, the tumour being extracted, 35 terminated favourably; the patient recovered. In 26 instances the termination was unfavourable; the patient died. Of the five cases in which no tumour was found, all recovered.”

To the simpler operation of tapping, the contrary precept applies. Neither in ascites, nor in ovarian dropsy, should paracentesis be resorted to, until it seems absolutely indispensable. To this rule there are, in my opinion, very few exceptions.

The operation itself, though commonly esteemed a trivial one, is not without its dangers. The instances are not few in which it has been followed by fatal peritonitis, excited either by the mere passage of the lancet or trocar through a previously unhealthy membrane, or (in the case of ovarian dropsy) by the escape of some portion of the contents of the cyst into the cavity of the abdomen. To obviate, or to remedy, this source of danger, it is expedient that the bowels should be thoroughly emptied by some mild purgative the day before the operation, and kept quiet for several days afterwards, by moderate doses of opium. Formerly, the rapid evacuation of a large quantity of liquid from the belly was often attended by terrifying effects; fainting, convulsions, almost instant death. This made the ancient physicians afraid of the operation; and when they could no longer avoid it, they let the accumulated fluid out by little and little, and at short intervals.

The cause of these alarming symptoms is now well understood, and easily averted. They were owing, doubtless, to the sudden removal of the pressure to which the viscera and large blood-vessels had been for some time submitted and accustomed. For this explanation of the fact we are indebted to the sagacity of our celebrated countryman, Dr. Mead, who was the first to suggest that external compression should be substituted, in lieu of the tension taken off by the operation. The complete success of that expedient fully justified his ingenious opinion. We now drain the cavity of its liquid contents without scruple or delay. Round the body of the patient, who sits on the edge of a chair or of the bed, a sheet, or broad roller is thrown, and tightened as the fluid escapes, so as to maintain an equable pressure, which is continued for a while, and at length gradually withdrawn. The risk, however, of exhaustion or syncope, and therefore the necessity for this artificial compression, may in most cases be avoided or diminished by keeping the patient, during the performance of the operation, in the horizontal posture, upon his or her side. We owe this practical improvement, I believe, to Dr. Simpson of Edinburgh.

Other casualties occasionally happen; the trocar has sometimes pierced the intestine. In one instance, which I myself witnessed, clear serum issued for some time through the canula, but at length pure blood; not less than a pint. The patient sank; and no opportunity was given of investigating the cause of the bleeding. In another strange but well-authenticated case, the almost incredible quantity, twenty-six pints, of blood flowed out at the orifice made by the trocar, and afterwards separated into clot and serum. To the wonder of those who saw the incident, this patient recovered from the tapping; and the source of the hæmorrhage is still a matter of conjecture.

And apart from these mischances—which, after all, are not of frequent occurrence—you must bear in mind that paracentesis can seldom be contemplated as a mode of cure, but simply of temporary relief from distress. A few instances have happened where the liquid has been drawn off, and has not again collected: but such cases are very rare. So, also, according to my experience, are those, much talked of by authors, in which the kidneys resume their activity upon the removal of the dropsical fluid. Ordinarily, the liquid re-accumulates, often with more rapidity than before; and again, and again, the pain and the hazard of the operation must be repeated; where-

“Of the 15 cases in which adhesions or other circumstances prevented the extraction of the tumour, 9 recovered, 6 died.”

In two other tables Mr. Phillips collects together:—

I. Cases in which a large incision was made, 55 in all: and among these cases there were 26 deaths, 23 cures, and 6 recoveries which were not cures.

II. Cases in which the incision was small (usually under six inches) amounting in all to 27. Among these there were 7 deaths, 13 cures, and 7 recoveries from the unsuccessful operation.

In the 34th volume of the Transactions of the same society, 1851, Dr. Lee has published an analytical table of 162 cases in which the operation has been attempted or performed in Great Britain. “In 60 the ovarian disease could not be removed; 19 of these proved fatal. Of the remaining 102 cases in which the operation was completed, 42 terminated fatally. The present condition of the 60 patients who recovered is very imperfectly known.”

Dr. Clay, of Manchester, states, in 1856, that he has performed the operation in 71 cases, and that 49 of his patients had recovered.

fore, in my judgment, paracentesis in abdominal dropsy ought seldom to be performed, unless the quantity of liquid is so great as to occasion painful distension; or causes great distress of breathing by its upward pressure against the diaphragm; or gives rise to some positive suffering or urgent inconvenience, which the evacuation of the water may be expected to remedy.

Acupuncture of the dropsical belly has of late been recommended; the passage of a grooved *needle*, instead of a *trocar*, through the abdominal parietes. I believe this to be sometimes an eligible and a useful piece of practice. By ascertaining the character of the inclosed liquid, it may settle the diagnosis of a case otherwise ambiguous; but it may do much more. Dr. Robert Lee informs me that he has done this minor operation many times; never with any bad result, generally with relief and benefit to the patient. In one case, ten gallons of liquid escaped from the little puncture. In another, where ordinary tapping was thought unsafe, acupuncture was performed, and fluid oozed freely away for two days and two nights. Great comfort was obtained from this process, and the woman's life was probably prolonged for two years by several repetitions of it. In a third instance, four ounces only of liquid followed the puncture, but a larger portion, which was left behind, gradually disappeared.

It is seldom that tapping is many times performed upon the same person, when the complaint is mere passive ascites. The dropsy returns indeed, and again the operation is required; meanwhile, in most cases, the health and strength rapidly deteriorate and the patient sinks.

The same speedy declension and early death too frequently occur in ovarian dropsy also; yet the operation commonly bears to be repeated more often than in ascites, without serious detriment to the general health. Sometimes the liquid re-accumulates in the cyst very quickly; sometimes slowly; in a very few instances not at all. I have had under my own care a patient who had been tapped for this disease thirty-eight or thirty-nine times. Extraordinary examples of a similar kind are on record; one or two I may mention as specimens.

Dr. Mead narrates the case of a lady, who "for the information of posterity, ordered by her will that the following English inscription should be engraved on her monument in Bunhill Fields:—

‘Here lies Dame Mary Page,
Relict of Sir Gregory Page, Bart.
She departed this life, March 4, 1728,
In the 56th year of her age.
In 67 months she was tapped 66 times:
Had taken away 240 gallons of water,
without ever repining at her case,
or ever fearing the operation.’”

Among authenticated instances, the most remarkable that I have met with is detailed in the *Philosophical Transactions* for 1784, by Mr. Martineau, who was at that time surgeon to the Norfolk and Norwich Hospital. An abstract of the case is given in the printed catalogue of the Hunterian Museum, where the cyst is preserved: it belonged to the left ovary of Sarah Kippus, a widow, fifty-five years old. "The complaint began after a miscarriage, at the age of twenty-seven. From the year 1757, to August, 1783, when she died, she had been tapped eighty times, and had, in all, had taken from her 6631 pints of fluid, or upwards of thirteen hogsheads. 108 pints was the largest quantity ever taken away at any one time. But after death, Mr. Martineau could not make the sac contain more than fifty pints."

Upon the whole, it may be stated of this operation, as applied to ovarian dropsy,—

1. That when it is essential to the comfort and continued existence of the patient, it brings sensible relief to her distress, and often materially prolongs her life;

But, 2. That when it is performed under less pressing circumstances, it tends to shorten the patient's days. Dr. Bright is of opinion that the number is small of those who survive the first tapping more than four years. I question whether even that brief limit might not justly be abridged by one-half. A respectable woman having very large ovarian dropsy, entered the Middlesex Hospital, under my care, for the express purpose of being tapped. The tumour incommoded her by its bulk and weight, but in no other way; and she had carried it for thirteen years. I felt that I

should not be justified in sanctioning the operation in such a case. The patient was made to understand that the performance of it would not be altogether free from immediate danger; and that if she went through it safely, the swelling would return, and the same kind of remedy again become equally necessary. She was instructed how to suspend the heavy overhanging abdomen by a sling passing over her shoulders. There appeared no reason why she should not continue in good health for another period of thirteen years.

I am aware of another instance, in which a woman, similarly burdened, but otherwise in comfortable health, has lived, not without enjoying life, between twenty and thirty years. Had she been tapped when the mere enlargement might have seemed to warrant the operation, she would probably have been for twenty years in her grave.

In connexion with the subject of paracentesis for ovarian dropsy, I must here notice two important points of practice, which, since the foregoing observations were delivered, have been advocated, respectively, by Mr. Isaac Brown of this place, and by Mr. Bambridge, of Liverpool.

Mr. Brown's principle of treatment is the same, in the main, with that propounded many years ago by the late Professor Hamilton, of Edinburgh, but differing in detail; the principle, I mean, of systematic *pressure*. He administers mercury till the gums are slightly tender, and keeps them so for some weeks; giving at the same time diuretics and tonics. Meanwhile the tumour is steadily compressed by a tight flannel bandage. When, under this management, the swelling has for some time decreased, or ceased to increase, the cyst is emptied by tapping. After this operation the cyst and body are again subjected to firm and constant pressure by means of accurately-adapted pads and tight bandages: and the medicines are continued for at least six weeks.

I mention this plan, because of the prosperous results which are said to have followed its adoption. Nevertheless I am constrained to express very serious doubts whether, if tried upon a more extensive scale, it will be found generally successful, or expedient, or even endurable.

Of the other procedure I am more inclined to think favourably. It is simply that of keeping open the orifice made by the trocar, without further meddling by instruments or injections—and allowing the fluid secreted by the cyst to escape as fast as it forms. Mr. Bambridge, who has used this method, adduces (in the 38th volume of the *Medical Gazette*) so many examples of its favourable issue, as to challenge the attentive consideration of the profession. In some of these the opening was artificial, and the experiment intended: in others it was the effect of some fortunate accident, or rather the spontaneous work of nature—whose safe and simple guidings are too often neglected. The theoretic objection to this course seems to be the apprehension that destructive inflammation may ensue in the interior of the cyst, and compromise the immediate safety of the patient. But this fear has not, that I am aware of, been practically realized, under sufficient trial. What most commonly happened, in the cases collected by Mr. Bambridge, was that the discharge gradually became puriform, then grew less, and finally ceased; the aperture closing in some instances, and the recovery being complete—while in others a fistulous opening remained, and the cure was, to that extent, imperfect. Even the great evils of a permanent discharge in this way, would probably be less than those which belong to the ordinary practice. The advantages therefore are, that the empty cyst is permitted to contract, until at last (perhaps) its cavity is obliterated; and that the mental suspense and anxiety, as well as the bodily wear and tear, of repeated fillings and repeated tappings, are avoided. When the orifice is healed immediately, unless the morbid secretion ceases at once, which it scarcely ever does, the cyst, distended afresh, is prevented from undergoing what would appear to be its natural and radical cure: and its contents are stored up, a useless and hurtful burden, only to be at length evacuated as before. Knowing how well the open drain has answered in some forms of empyema, I should augur hopefully of the same principle when applied to the sad and perplexing category of ovarian dropsies.

Neither indeed of these two expedients is at all new. Each has been occasionally employed for centuries past. But the merits of the last of the two have never, I think, been ascertained with sufficient exactness. Without absolutely counselling its

adoption, therefore, I go so far as to propose it to more competent judges for reconsideration. A further question might arise respecting the most eligible spot for the puncture. Many reasons which I need not specify, as well as a certain amount of experience, would seem to indicate the vagina, as affording a convenient place of outlet.

Another piece of practice in such cases is new; that, namely, of injecting the emptied cyst with a solution of iodine. What the object of these injections may be I do not clearly apprehend. A cure, immediate and radical, like that which is attainable by similar injections in cases of hydrocele, can hardly be contemplated. The arrest of all further secretion from the interior of the cyst seems scarcely more probable. That the danger incidental to the tapping must be greatly enhanced by this expedient I cannot but believe, notwithstanding strong assertions to the contrary. It is an expedient, however, which is at present upon its trial. You will be guided in forming your own judgment of it, not by my misgivings, but by the results of that trial.

LECTURE LXVIII.

Acute Gastritis: symptoms; anatomical characters; treatment. Chronic Inflammation of the Stomach; thickening of the Mucous Membrane; Ulceration; symptoms and treatment of the disorder; Softening and perforation by the Gastric Juice. Cancer of the Stomach.

ACUTE inflammation, when it affects the peritoneum, usually spreads with rapidity over the whole surface of the membrane. This is characteristic of inflammation of the serous membranes generally. But it is not so with the other tissues that compose the alimentary canal. Inflammation of the mucous membrane may be, and often is, very limited in extent: and the different portions of the intestinal tube, as they differ in function, so also they differ somewhat in their diseases, and still more in the symptoms by which those diseases are revealed. Not being fettered by any artificial system of arrangement, I shall take the course which promises to be practically most useful, and consider separately the maladies of the several parts of the alimentary canal in the abdomen, extending my remarks occasionally to the whole of the tube, when speaking of disorders that are common to all portions of it.

Let me, then, in the first place, draw your attention to the organic diseases, and the morbid conditions, of the *stomach*.

It is remarkable, all things considered, how seldom the stomach is affected with *acute inflammation*. Scarcely ever do we find either the organ as a whole, or any one of its tissues separately, the subject of *spontaneous* acute inflammation. What is described in books as *gastritis*, means inflammation of the *mucous membrane* of the stomach: and almost all that we know, for certain, of this disease, we derive from observation of the effects of strongly irritant substances upon that membrane. Idiopathic gastritis, in an acute form, I never saw. Acute gastritis, from the contact of corrosive or acrid poisons, I have frequently seen: and a highly interesting affection it then becomes. This is a subject that cannot be thoroughly discussed in this course of lectures: neither may it be altogether omitted.

When an irritant poison has been received into the stomach and excites inflammation there—or when acute inflammation arises from any cause—the symptoms which mark that inflammation are pain, usually of a burning character, in the epigastrium; with frequent vomiting, especially upon the entrance of any thing into the stomach; and often with hiccup, and with tenderness and tension of the upper part of the abdomen. To these local symptoms are added fever of a low type; and a small weak pulse. At first, indeed, the pulse, although small, is generally sharp and hard; but it soon becomes thready and feeble. The muscular power undergoes a corresponding

depression; the patient is pale and faint, with collapsed features, cold extremities, and a damp skin.

In all this we see a strong tendency to death by *asthenia*. It is clear that the subdued state of the circulation is dependent upon the inflammation, for it is often relieved by the remedies of inflammation. In acute gastritis, as well as in peritonitis, you will find that the pulse expands, becomes more distinct and full, under early bleeding: sometimes even while the blood is flowing.

Upon this remarkable sympathy between the heart and the stomach I have had frequent occasion to insist. You are aware that a smart blow upon the epigastrium may put a sudden stop to the movements of the heart, and induce mortal syncope; without leaving any local trace of its operation. On the other hand, a person in a state of extreme exhaustion and faintness, will sometimes revive at once, upon swallowing into the stomach an ounce or two of brandy, and recover his pulse and colour much too speedily, to allow of our ascribing these effects to the absorption of the alcohol into the blood. Dr. Alison suggests that the depression of the circulation may be attributable to the peculiar sickening pain which accompanies inflammation or sudden injury of the stomach. It appears, however, more probable that the remarkable sympathy in question is governed by the nerves of organic life. The great solar plexus of the ganglionic system lies upon the spinal column immediately behind the stomach. The heart is largely supplied with nervous filaments from the same system. Hence we might almost expect that any sudden stimulus applied to this important plexus would excite, and that any sudden depressing influence would subdue, the natural action of the heart. Upon the same principle may be explained the facts that deadly faintness and nausea are apt to result from injury to the testes, which are also abundantly endowed with influence from the nerves of organic life. Be this as it may, it is important for you to know that the mode of dying in these cases is precisely what Bichat describes as *death beginning at the heart*.

The pain that accompanies gastritis is augmented by pressure upon the epigastrium. It is increased also by the full descent of the diaphragm, and the *breathing* is consequently short and constrained. In the most exquisite cases of gastritis, produced by chemical or mechanical irritants applied to the interior of the stomach, the inflammation probably reaches and involves, more or less, the peritoneum. The patients speak of the pain as a pricking and burning sensation; it is attended with great anxiety and restlessness. The sufferer is tormented with extreme thirst, while all that he drinks, even cold water, is almost instantly rejected by vomiting.

Hiccup does not always accompany acute gastritis. It sometimes occurs early; but more generally it comes on late in the disease, when the patient is sunk and much debilitated.

The bowels, in this complaint, are sometimes bound: sometimes, on the contrary—especially when the inflammation has been caused by corrosive poison—dysenteric diarrhoea ensues, with much griping and tenesmus.

Such, then, are the symptoms that indicate the existence of acute gastritis; but you ought to be aware that they occur in varying combinations, and with different degrees of severity; and consequently that the course of the disease is not uniformly the same in all cases. When the symptoms are the most violent, and the progress of the complaint is the most rapid, the peritoneal coat of the stomach is usually, I believe, more or less implicated.

Intense inflammation of the stomach may be expected to be rapid in its progress. It may destroy life within twenty-four, or even twelve hours. When it is fatal, it generally is so within a few days; and death takes place by fainting; with a remission of the pain, sometimes very sudden, and sometimes occurring only just before dissolution. But as idiopathic gastritis is rare, *fatal* idiopathic gastritis is, of course, still more so. Louis states, that during six years' experience at La Charité, in which period he noted the details of 6000 cases of disease, and of 500 dissections, he did not meet with a single instance of fatal idiopathic gastritis. The subject derives almost all its importance, therefore, from its connexion with poisoning; and the many interesting points of inquiry which arise out of that connexion will be brought before you by the Professor of Forensic Medicine. This consideration is a great satisfaction and relief to me; because I find that the limits of my own course will not permit me to go into any detail in this matter.

The morbid appearances to be looked for after death by acute gastritis, are redness of the mucous membrane, softening, sloughing, and even (after the action of strongly corrosive poisons), perforation of all the coats of the stomach.

I wish particularly to caution you against being misled by mere *redness* of the interior of the stomach; or of the inner surface of the alimentary canal in general; or of *any* mucous membrane; and indeed I may add, of any *serous* membrane also. Redness and inflammation have been made, too often, convertible terms. Persons finding the inner surface of the stomach red, have thence too hastily concluded, that suspicions of poisoning which had arisen, were well founded. We are indebted to Dr. Yelloly, in the first instance, and to M. Billard and some other Frenchmen, in the second, for correcting this error—an error which not only was of importance in questions of imputed poisoning, but has run through and vitiated almost the whole of pathology, both lately and heretofore. Mistaking mere redness for evidence of inflammation, Cullen divided gastritis into two species—one of which he called gastritis *erythematica*; and he inferred from the observation of cases in which redness of the membrane had been met with after death, that this peculiar kind of inflammation of the mucous coat of the stomach might take place, without fever, pain, vomiting, or any other symptom indicative of gastritis: whereas it is almost certain that, in the cases to which he refers, there really was no inflammation at all. So also Morgagni, puzzled by intestinal vascularity, was disposed to attribute the absence of pain, in what he believed to have been inflammation of the bowels, to a paralytic affection which blunted the sensibility of the parts; and Haller conceived, from so constantly meeting with this vascularity in *his* inspections of the body, that inflammation of the bowels was almost always present in fevers of all kinds; and was frequent in every other complaint. And the same doctrine has been strenuously inculcated of late years, as I dare say you know, by Broussais, in France, and adopted by a vast host of his disciples. Finding the lining membrane of the stomach and intestines red and vascular in most of the bodies of patients who had died of fever, Broussais concluded that fever depends, in all cases, upon inflammation of the gastro-enteric mucous membrane. You will perceive that this doctrine is likely to exercise a vast influence upon the *practice* of those who entertain it. If inflammation constitute an essential part of any disorder, it follows that the *remedies* of inflammation will be adapted to that disorder; and thus, even so slight a mistake as that may appear to be, against which I am now cautioning you, of regarding every surface which is red as being inflamed also, may lead to very mischievous views in respect of treatment.

The redness that is *independent* of inflammation may be of various kinds; but the principal cause of it is venous congestion. “The appearances of vascular fulness (says Dr. Yelloly) in the villous coat of the stomach, whether florid or dark-coloured, in distinct vessels or in extravasations of different sizes, are not to be regarded as unequivocal marks of disease; inasmuch as they occur in every variety of degree and character, under every circumstance of previous indisposition, and in situations where the most healthy aspect of the organ may be expected.” To the truth of this statement I can bear witness, having at one time of my life carefully examined, with a view to this matter, a great number of stomachs in succession, in the dead-house of a large hospital. “The vascularity (according to Dr. Yelloly) is entirely *venous*, and depends on a power capable of being exercised on the artery itself at the close of life, which carries on the blood to the veins, after the further supply of fresh blood from the heart is stopped. The branched or stellated form of vessels, under which the vascularity usually appears, is capable of being *imitated*, either by injecting the veins with fine injection, or by forcing back with the finger, or the back of a scalpel, the blood from the larger branches of veins into the smaller.” “And this vascularity soon becomes diffused redness, by transudation of the blood through the coats of the containing vessels, just as happens with the bile in the gall-bladder.”

Redness, from mere repletion of the smaller veins, is usually extensive and undefined; except that, being influenced by the force of gravity, it settles into the most depending parts of the organ, which are either its exclusive seat, or at any rate are of a deeper colour than the parts more elevated. It is attended with an empty state of the arteries, and with a full state of the larger veins. Hence the condition of the venous and arterial trunks, and especially of the vena portæ, should, in doubtful cases,

be ascertained before the main blood-vessels are laid open, and drained of their contents.

The redness that belongs to inflammation is generally circumscribed, and of limited extent; it occupies indiscriminately the upper or the lower side of the tube (for these remarks apply alike to the stomach and to the intestines); it is attended with some fulness of the corresponding arterial trunks; and it may or may not be coincident with comparative emptiness of the venous system within the abdomen. Much will depend, in this respect, upon the *mode of dying*, as I have fully explained to you on a former occasion.

You will please to remember then, in all your future investigations into morbid anatomy, that it is generally difficult, and often impossible, to determine, from the aspect of the vessels of a dead part, from its redness, that inflammation had been present in that part during life, unless the unequivocal products or effects of inflammatory action are present also.

A much more certain evidence of inflammation of the mucous membrane of the stomach and intestines, is its *softening*. This can seldom be attributed to anything else, unless it be to decomposition, or to the solvent action of the gastric juice. Neither of these last causes can come into operation until life is extinct. It is well known that the membrane is slow and late in passing into the state of putrefaction after death. To the effect of the gastric juice in softening and dissolving the coats of the stomach, and to certain important questions respecting their perforation, I shall by and by return.

I say that gastritis is most commonly the effect of poisons applied to the mucous surface of the stomach; but I must include under that head certain substances, which, to most people, are not poisonous or injurious at all, and which only become so to some persons under particular circumstances. Thus, large draughts of cold drink, taken when the body is hot, and rapidly parting with its heat, and especially large draughts of cold *sour* liquors, as cider or stale beer, are apt to give rise to acute gastritis. Another occasional cause of gastritis is the ingestion of very large quantities of food at one time, especially during convalescence from any serious disorder. It is an exceedingly curious fact, too, but one which I merely mention without dwelling upon it, that certain poisons introduced into the body through some other channel, will cause inflammation of the mucous membrane of the stomach, with which they have *not* been in contact. Corrosive sublimate, and arsenic, excite inflammation, with ulceration or sloughing of the gastric mucous membrane, even when they are merely rubbed, in a certain quantity, upon the skin; or when they are applied to the surface of a wound, or inserted into the rectum.

The treatment of acute gastritis is simple. The chief nicety respects the employment of blood-letting. Early in the disease, if the pain be severe, you must *try* the effect of venæsection, notwithstanding the smallness and feebleness of the pulse. How much blood you are to abstract cannot be told beforehand. Take away a small teacupful, keeping, meanwhile, your finger on the wrist. If the beat of the artery do not grow weaker, and still more if it become fuller and stronger, go on with the bleeding, and take another cupful, and another, according to the circumstances of the case and to the effects produced. Apply leeches to the epigastrium, and cover the bleeding bites with a soft, light poultice. Keep the patient as strictly as you can in the horizontal posture: in other words, see that the depressing influence of the disease upon the action of the heart is not aided and augmented by the position of the body. If cold water be retained, that is the best medicine which you can give by the mouth; purgatives so administered would be almost sure to be rejected; and if not rejected, they would be likely to increase the existing inflammation of the organ. Enemata are, however, extremely useful: of warm water, if the bowels are not much confined; of purgative materials if they are. After the intestines have been thus cleared — or when they are loose and irritable — opiate injections (thirty or forty drops of laudanum, with three or four ounces of starch or gruel) do much good. They often have a very tranquillizing effect upon the irritable stomach, and check the vomiting. These measures are to be pursued until the inflammation has subsided. If the stomach be capable of retaining any nutriment at all, it must be given in small quantities, at distant intervals, in a liquid form, and of the blandest kind: barley-water, milk diluted with water, arrow-root, smooth gruel, and the like.

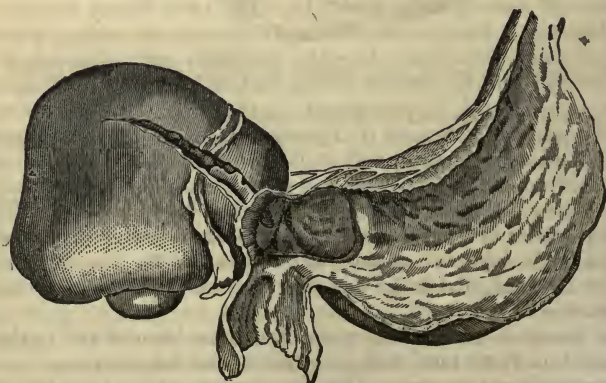
When any *corrosive* substance has been swallowed, I scarcely need say that pains should be taken to remove it as speedily as possible from the stomach: or to administer such remedies as are known to be capable of decomposing the poison, or of affording a specific antidote to it. Not that the stomach-pump should be employed in such cases, as too often it is. These, however, are points that must be fully treated of in the lectures on forensic medicine, and therefore I shall dwell upon them no longer here.

Chronic inflammation of the stomach is probably a very common disorder. Except when it results in ulceration, it does not put life in imminent jeopardy: and it is often recovered from. Deranging, however, the functions, and perverting the feelings of the stomach, it gives rise to the manifold and multiform symptoms of *dyspepsia*. But *dyspepsia*, with its manifold and multiform symptoms may be, and often is, entirely independent of inflammation. You see, then, why the effects of chronic gastritis are various; and why the symptoms that are supposed to denote its presence are apt to be obscure, uncertain, and equivocal. I intend, before I quit the subject of the stomach, to investigate the principal circumstances that mark its functional disorders, and to describe the means which we sometimes find conducive to their relief. I shall therefore restrict myself at present to a few points which seem to have been fairly ascertained respecting chronic gastritis.

We know that chronic inflammation had been going on in the stomach when, after death, we see that its coats are thickened; or when we perceive that a portion of one or more of them has been removed by ulceration.

It is not at all uncommon to find the mucous membrane of the stomach, over a larger or smaller space, thick, granular, uneven, and of an unnatural colour. Gray, or slate-coloured, it often is. This slate colour is much dwelt upon by the French writers, as being a sure and unequivocal impress of chronic inflammation. The colour proceeds, I believe, from the operation of the gastric acids upon the blood, which, under habitual congestion or slow inflammation, is detained in the vessels of the altered part. The ulcers that result from chronic inflammatory action are usually small, varying from the size of a split pea to that of a shilling; sometimes with no surrounding vascularity or thickening at all, but looking exactly as though a piece of the mucous membrane had been struck out by a stamp; sometimes with rounded and elevated edges only; and sometimes they occupy *patches* of thickening and induration

FIG. 104.



Stomach presenting a chronic ulcer; at its upper margin the pneumogastric nerve is shown extending into dense fibrous tissue. The pancreas and the left lobe of the liver formed the base of the ulcer: the latter presents fibroid degeneration of its structure.—From *Habershon*.

of the parietes of the stomach. Generally there is but one solitary ulcer. Its most usual situation is the posterior part of the stomach, in or near its smaller curvature,

and nearer the pyloric opening than the cardiac. More rarely it occupies the anterior part. Now and then an ulcer is found on both the back and front surfaces, at exactly opposite spots. Sometimes two, or three, or more ulcers, are met with in the same stomach. It is very seldom, however, that they are numerous

FIG. 105.



FIG. 106.



Stomach exceedingly contracted from chronic ulceration, with villous growth. Simulating cancer:—

Fig. 105.—External view, resembling colon in appearance.

Fig. 106.—Internal surface, showing ulceration near the pylorus, and villous growth near the centre of the stomach.—From *Habershon*.

Ulcerative disease of the stomach may prove fatal in various ways. The ulcer may penetrate as far as the peritoneum, and excite inflammation of that membrane, whereby the stomach becomes adherent to the neighbouring parts. In these cases, prior or subsequently to adhesion, death may at length ensue, from gradual exhaustion and protracted suffering.

If an ulcer happen to lie over the track of a large blood-vessel in the stomach, it may eat its way into that vessel, and give rise to fatal hæmorrhage.

Or the ulcer may perforate the walls of the stomach, without any previous adhesion, and suffer the food, or the secretions of the stomach, to pass into the peritoneal cavity, where intense inflammation is lighted up, and the patient soon perishes.

Or the ulcers may at length *heal*. Of this we are certain, because we often find cicatrices denoting the spots which the ulcers had occupied.

Our stock of knowledge respecting this dangerous disease, ulcer of the stomach, has been much extended, and rendered more exact, by the researches of living physicians. There are three English volumes which, if you read them carefully, will

furnish you with all that has hitherto been learned on this interesting subject: Dr. George Budd's on *Diseases of the Stomach*; Dr. Thomas Chambers' on *Digestion and its Derangements*; and Dr. Brinton's comprehensive monograph on *Ulcer of the Stomach*. In amplifying somewhat the sketch which I have just given you, I borrow chiefly from these writers.

FIG. 107.



Perforating ulcer of the stomach; the mucous membrane is puckered into folds around it.

Ulcer of the stomach is not an unfrequent disease. Dr. Brinton met with it in about one per cent. of his out-patients at the Free Hospital. It is much more common in women than in men; and it is mainly, though by no means solely, a disease of middle and of advancing life. That it is capable of cure is manifest, as I have said, from the puckered scars which mark the sites of former ulcers. Reckoning from a large number of recorded cases, Dr. Brinton concludes that, in dead bodies, the cicatrix is not less often seen than the open ulcer. There are obvious reasons why a spontaneous cure is not more frequent even than

this. The healing of such ulcers must be hindered by the alternate stretchings and contractions to which they are subject in the sudden and repeated changes of volume of the stomach, now full and distended with food or with gas, now empty and flaccid: it must be hindered by the vermicular movements of the stomach during the work of digestion; by the contact of food and drink of various kind and quality; and probably, as Dr. Budd suggests, by the action of the gastric juice upon the soft and recent lymph, which must needs form the material of repair in the healing process.

Of the open ulcers of the stomach a certain proportion only — about one in four — go through; become perforating ulcers. And if it be admitted that of the whole number of ulcers there are as many healed as open, then the ratio of the perforating ulcer to the whole number becomes one in seven or eight. This accident of the ulcer is more than twice as common in females as in males: and it is a curious fact, which I am not able to explain, that it occurs more often in maid-servants, between the ages of fifteen and twenty-five, than in any other class of persons. As life goes on, after the thirtieth year, the liability to the formation of a gastric ulcer increases, while the risk of its perforating the walls of the stomach decreases. According to Dr. Brinton, perforating ulcers of the pyloric extremity of the stomach are more common in men than in women.

This risk of going through has some relation to the position of the ulcer; and it is a relation which is quite intelligible. Perforation is much more frequent in the anterior than in the posterior wall of the stomach. The posterior wall is at once the most subject to ulcers, and the least subject to perforation: it is more closely and more constantly applied to the solid abdominal viscera; its movements over them are fewer and less extensive; and it therefore more readily contracts adhesions with them, which adhesions prevents its perforation, in both senses of the word prevent. The threatened aperture is more often stopped by an adherent pancreas than by any other viscus; but adhesion may take place with the liver, with the colon, or with any part that happens to lie in contact with the stomach. When the colon is the attacked part the ulcer may indeed penetrate into that intestine, but I use the word perforation to express the formation of an opening that communicates with the general cavity of the peritoneum. Without any such communication the ulcer may eat its way beyond the stomach, and produce limited abscess in adherent organs or tissues.

Perforation, when it does occur, may result from sloughing or rupture of the peritoneal coat of the stomach, in the sometimes slow, sometimes rapid progress of the deepening ulcer: but it is more often caused, at last, by pressure of some sort, which suddenly breaks the thinned and fragile membrane. The instant of the rupture is marked by definite and terrible symptoms. It has frequently happened just after a hearty meal; and during the acts of vomiting, and of straining at stool. It has been known to take place in the effort of sneezing; under the sudden compression of the

waist by a tight belt; from a rough jolt in a dog-cart. These facts suggest a caution to ourselves—how we handle in such cases the epigastric region, or explore the abdomen by pressure.

A certain number of the ulcers (from 4 to 5 per cent. it is calculated), prove fatal by erosion of a large blood-vessel, and consequent hæmorrhage. As the ulcers most commonly occupy the lesser curvature or the posterior wall of the stomach, it follows that the coronary artery which runs along its lesser curvature, and the splenic which crosses its posterior surface, taking its course along the upper border of the pancreas, are the arteries most obnoxious to this erosion. Here is a drawing after Sir R. Carswell, representing an ulcer which laid open the coronary artery of the stomach and caused fatal hæmorrhage: you may observe that it shows also three scars of healed ulcers. The hæmorrhage is usually abundant, and the vomiting of the blood is preceded by faintness, or actual syncope. It is not however always followed by immediate death. Indeed Dr. Budd states that it was fatal *at the time* in one instance only, among “a considerable number of cases of the kind” that had fallen under his own observation. The bleeding is capable of being somehow staunched, and the injury repaired; and the danger, if not permanently averted, yet postponed. An example of death from this cause is detailed in the *Journal Hebdomadaire*, for May, 1830. The patient had vomited considerable quantities of blood for eight days in succession, five years previously to the attack which terminated his life. So that hæmatemesis from a ruptured blood-vessel in the stomach is not absolutely hopeless.

As this accident of the ulcer is, after all, rare, I will briefly relate an instance of it, which occurred in the year 1831, at St. Bartholomew's Hospital. Dr. Latham, who had charge of the case, was good enough, some time ago, to give me the following history of it. The subject of the disorder was a man thirty-eight years old. He was admitted on the 19th January. His countenance was dusky, but exsanguine; his pulse 100, and weak; his tongue pale, and slightly furred. He made no complaint of pain anywhere.

He had been ailing for two years; had suffered much pain across the epigastric region; and had frequently vomited his meals. Two days before, he had been suddenly attacked with faintness and giddiness, and then vomited about two quarts of blood. He was a habitual spirit-drinker.

In the afternoon of the day on which he entered the hospital, he was again seized with giddiness; and fell into a state of syncope, in which he remained for several minutes. Upon recovering, he vomited a large quantity of blood, not less than three pints. The next morning, early, he brought up a like quantity, under similar circumstances; and he passed three evacuations from the bowels, all of them *black*. He was gradually sinking during the whole of that day, the 20th. Towards the evening, he vomited about half a pint more blood. He died quietly the next morning.

When the abdomen was laid open, the stomach was seen to be distended. The intestines had, in several places, a black appearance; from the colour of their contents. The stomach contained about two pints of coagula, and of a dirty red liquid. At the upper part of its lesser arch was a small excavated ulcer, with hardened edges. In the centre of this ulcer there were visible the orifices of three or four arteries, filled with minute clots of blood.

Blood to a less amount, and by a slower drain, and less arterial in character, darker and more tar-like, may be poured out from many ruptured capillary vessels, in the stomach itself, or in the adherent and eroded pancreas, liver, or spleen.

The symptoms that indicate the existence of ulcer of the stomach are, in kind, the symptoms that accompany chronic gastritis; pain or uneasiness in the epigastrium increased by pressure, increased also on the introduction of food, or perhaps felt only while digestion is in progress; flatulence and eructation; vomiting of mucus, and of the meals; loss of sleep; languor and debility.

By closely observing the course and succession of symptoms of this sort we may often arrive at a tolerably sure diagnosis of the presence of an ulcer.

Some of the most fearful cases of perforation of the stomach, those I mean which are apt to occur in young unmarried women, run apparently a brief course, and are attended with few or but slightly marked symptoms. These patients are, however, mostly anæmic; and when questioned, generally confess to previous dyspeptic feelings. It has been fancied that some derangement of the uterine functions may be

influential in causing this mysterious and terrible form of ulceration: but it has happened prior to the period of puberty, and when the menstrual flux has been complete and regular, as well as when it has been scanty and suspended. The ulcer has always the punched-out character.

In slower cases (and they sometimes go on for years), the symptoms, equivocal at first, become more and more significant as the disease proceeds. One leading symptom is *pain*—felt in a circumscribed space in the epigastrium, and often at the same time, or alternately, in the back, just below the shoulders. The pain begins immediately upon, or very soon after, the entrance of food into the stomach; especially of food or drink which is hot or stimulating. It usually continues until the digested aliment has passed the pylorus; or until vomiting puts an end to it. The pain is produced or aggravated by pressure, by exercise, curiously too, by mental anxiety,—mitigated by recumbency—and accompanied frequently by sour eructations.

Vomiting is another of the principal symptoms,—later commonly in its arrival than the pain; occasional at first; afterwards very frequent. Supposing an ulcer present, this is a very dangerous symptom. It tends to starve and weaken the patient, and so to promote the progress of the ulcer: it augments also the hazard of its suddenly breaking through.

The persistence of these symptoms,—which are symptoms of mere dyspepsia also may justly engender the suspicion of a gastric ulcer: and if, after they have existed for some time, copious hæmatemesis should supervene, the suspicion passes into something like certainty. Dr. Budd holds that if profuse vomiting of blood occurs in a person between the ages of eighteen and thirty, after a long continuance of pain in the stomach, extending into the back, with tenderness of the epigastrium, the pain and soreness being always brought on or increased by meals, with occasional sour eructations and occasional vomiting, with no great wasting or constitutional disturbance, no evidence that the orifices of the stomach are obstructed, and no tumour to be felt—hardly a doubt can remain that the disease is simple ulcer of the stomach.

Some of the last qualifications, and the limitation as to age, are introduced to exclude the only possible alternative of *cancerous* ulceration—of which I shall by and by have to speak.

Having achieved this point in the diagnosis, the ambition of still greater precision is natural and laudable. Dr. Budd thinks that when there is much tenderness of the epigastrium, and no pain in the back, the ulcer is most probably on the anterior face of the stomach. According to Dr. Brinton the decubitus may sometimes be a guide: the ulcer is most likely to occupy that part of the stomach which is uppermost when the patient is lying in his habitually easiest position.

The practical management of this perilous condition is delicate, but simple and obvious. When the symptoms are urgent, the patient should remain at rest; and even keep himself in the recumbent posture. All food which is likely to create pain by its quality or by its temperature, or which has been found upon trial to give pain, should of course be forbidden. Tepid milk, alone, or thickened with biscuit-powder—containing as it does all the elements of nutrition—is probably the very best *kind* of food. The stomach must never be *distended* by a meal; yet the strength and nourishment of the body require to be cherished and sustained. The food must therefore be taken in small quantities, and often; a table-spoonful say, or two table-spoonfuls, every two hours. If the pain be severe, it may sometimes be appeased by counter-irritation; the mustard poultice for example, or a stimulating liniment containing opium, applied to the epigastrium, or to the back. Bismuth, in doses of eight or ten grains, is often found serviceable; it may sometimes be advantageously combined with five grains of the compound kino-powder: or from three to five grains of the compound soap-pill may be given from time to time. When there is hæmorrhage, ice swallowed in small quantities is both beneficial and grateful to the patient: and nutritive enemata may spare the stomach some of its work, and help to maintain his strength. If the bowels are sluggish, their action may be regulated by an aloetic or a colocynth pill.

These are the points to be kept in view. The detail must be left to the common-sense of the practitioner. And when I say this, I am forcibly reminded of a most striking and instructive case, beautifully told by the celebrated Dr. William Hunter, in the sixth volume of the *Medical Observations and Inquiries*. The perusal of that

history has afforded me hints upon which I have often acted with great advantage to my patients, and with some credit to myself, in treating chronic diseases of the stomach. As I doubt whether many of you would find immediate opportunity or leisure for referring to the narrative, and as I should spoil it by attempting to give you an abstract of it, I am tempted to read it here in Dr. Hunter's own words.

"Many years ago (he says) a gentleman came to me from the eastern part of the city, with his son, about eight or nine years old, to ask my advice for him. The complaint was great pain in the stomach, frequent and violent vomitings, great weakness, and wasting of flesh. I think I hardly ever saw a human creature more emaciated, or with a look more expressive of being near the end of all the miseries of life. The disorder was of some months' standing, and from the beginning to that time had been daily growing more desperate. He was at school when first taken ill, and concealed his disorder for some time; but growing much worse he was compelled to complain, and was brought home to be more carefully attended. From his sickly look, his total loss of appetite, besides what he said of the pain which he suffered, but especially from his vomiting up almost everything which he swallowed, it was evident that his disorder was very serious.

"Three of the most eminent physicians of that time attended him in succession: and tried a variety of medicines without the least good effect. They had all, as the father told me, after sufficient trial, given the patient up, having nothing further to propose. The last prescription was a pill of solid opium; for in the fluid state, though at first the opiate had staid some time upon his stomach, and brought a temporary relief, it failed at length, and like food, drink, and every medicine which had been given, was presently brought up again by vomiting. The opiate *pill* was therefore given in the hopes that it would elude the expulsive efforts of the stomach. It did so for a time; but after a little use, *that* likewise brought on vomiting. Then it was that his physician was consulted for the last time, who said that he had nothing further to propose.

"Though at first the boy professed that he could assign no cause for his complaint, being strictly interrogated by his father, if he had ever swallowed anything that could hurt his stomach, or receive any injury by a blow, or otherwise, he confessed that the usher in the school had grasped him by the waistcoat at the pit of the stomach, in a peevish fit, and shaken him rudely, for not having come up to the usher's expectation in a school-exercise. That though it was not very painful at the time, the disorder came on soon after. This account disposed the father to suspect that the rude grasp and shake had hurt the stomach. With that idea he brought him to me, as an anatomist, that an accurate examination might if possible discover the cause or nature of the disorder.

"He was stripped before the fire, and examined with attention in various situations and postures; but no fulness, hardness, or tumour whatever could be discovered; on the contrary, he appeared everywhere like a skeleton covered with a mere skin; and the abdomen was as flat, or rather as much drawn inwards, as if it had not contained half the usual quantity of bowels.

"Having received all the information that I could expect, and reflected some little time upon the case, I wished to speak with the father alone, in another room; and to give my patient some employment as well as refreshment, asked him to take a little milk in the mean time. But his father begged that taking anything into his stomach might be put off till he got home, because he was certain that it would make him sick; 'just before we set out (said he) I gave him a little milk; but he was sick, and brought it all up in the coach, before we had got many paces from the house.'

"In the adjacent room I said to the father, This case, sir, appears to me so desperate, that I could not tell you my thoughts before your son. I think it most probable, no doubt, that he will sink under it; I believe that no human sagacity or experience could pretend to ascertain the cause of his complaint: and without supposing a particular or specific cause, there is hardly anything to be *aimed at* in the way of a cure. Yet, dreadful as this language must be to your ear, I think you are not to be without hope. As we do not know the cause, it may happen to be of a temporary nature, and may of itself take a favourable turn; we see such wonderful changes every day, in cases that appear the most desperate, and especially in young people. In them the resources of nature are astonishing

"Then he asked me if I could communicate any rules or directions, for giving him a better chance of getting that cure from nature, which he saw he must despair of from art.

"I told him that there were two things which I would recommend. The first was not so important indeed, yet I thought it might be useful, and certainly could do no harm. It was to have his son well rubbed, for half an hour together, with warm oil and a warm hand, before a fire, over and all around his stomach, every morning and evening. The oil, perhaps, would do little more than make the friction harmless, as well as easy; and the friction would both sooth pain, and be a healthful exercise to a weak body.

"The second thing that I had to propose, I imagined to be of the utmost consequence. It was something which I had particularly attended to in the disorders of the stomach, especially vomitings. It was, carefully to avoid offending a very weak stomach, either with the quantity, or quality, of what is taken down; and yet to get enough retained for supporting life. I need not tell you, sir, said I, that your son cannot live long, without taking *some* nourishment; he must be supported to allow of any chance in his favour. You think that for some time he has kept nothing of what he swallowed; but a small part must have remained, else he could not have lived till now. Do you not think, then, that it would have been better for him if he had only taken the very small quantity which remained with him, and was converted to nourishment? It would have answered the end of supporting life as well, and perhaps have saved him such constant distress of being sick, and of vomiting. The nourishment which he takes should not only be in very small quantity at a time, but in quality the most inoffensive to a weak stomach that can be found. Milk is that kind of nourishment. It is what Providence has contrived for supporting animals in the most tender stage of life. Take your son home, and as soon as he has rested a little, give him *one* spoonful of milk. If he keeps it some time without sickness or vomiting, repeat the meal, and so on. If he vomits it, after a little rest, try him with a smaller quantity, viz., with a dessert, or even a teaspoonful. If he can but bear the smallest quantity, you will be sure of being able to give him nourishment. Let it be the sole business of one person to feed him. If you succeed in the beginning, persevere with great caution, and proceed very gradually to a greater quantity, and to *other* fluid food, especially to what his own fancy may invite him: such as smooth gruel, or panada, milk boiled with a little flour of wheat or rice; thin chocolate and milk; any broth without fat, or with a little jelly or rice or barley in it, &c., &c.

"We then went in to our patient again; and that he might be encouraged with hope, and act his part with resolution, I repeated the directions with an air of being confident of success. The plan was simple, and perfectly understood. They left me.

"I heard nothing of the case till, I believe, between two and three months after. His father came to me with a most joyful countenance, and with kind expressions of gratitude told me, that the plan had been pursued with scrupulous exactness, and with astonishing success; that his son had never vomited since I had seen him; that he was daily gaining flesh, and strength, and colour, and spirits, and now grown very importunate to have more substantial food. I recommended a change to be made by degrees. He recovered completely; and many years ago he was a healthy and a very strong young man."

In fulfilment of my promise I revert to the subject of *perforation* of the stomach.

There are three ways, exclusive of mechanical violence, in which such perforation may be effected. In each of the three the perforation proceeds from within outwards. All the coats of the stomach, as we have seen, may be penetrated in succession by a chronic ulcer. The direct contact of corrosive poisons may rapidly eat them through. They may be partially digested and destroyed by their own proper secretion, the gastric juice. Questions of much nicety and of grave importance present themselves, from time to time, respecting holes that are discovered in the stomach after death: medical questions, bearing upon pathology and therapeutics; legal questions, involving life and character in their solution. Perforation by disease, perforation by the gastric juice, are both liable to be mistaken, (and often have been mis-

taken) for evidence of murder or of suicide by poisoning. It is fit that you should have considered these points.

Now of ulcers of the stomach I have already told you all that I know: and it would be impossible for me, as well as out of place, to enter at large upon the topic of corrosive poisoning: that you will hear fully discussed by the Professor of Forensic Medicine. Perforation by the gastric juice demands a somewhat closer attention.

John Hunter, as you probably know, was the first to recognize and announce the remarkable fact that the stomach is capable, through the peculiar fluid furnished by itself, of digesting its own tissues. The dissolving power of the gastric juice survives for a while the vitality of the body, and acts as readily after death upon its parent flesh, as upon the food submitted to its influence during life. This discovery naturally excited great curiosity and interest. Hunter's observations were verified by several of his contemporaries or immediate successors; and hypotheses were soon framed to account for the supposed infrequency of the phenomenon. Dr. Adams thought that the stomach was soluble by the gastric juice, only when the death was general and complete as well as sudden; only, *i. e.*, when the stomach itself became instantly dead, and no organic vitality lingered in its tissues: and he took the continued fluidity of the blood, and the absence of the *rigor mortis*, as tests of such universal sudden death. He correctly supposed also that temperature was concerned in the matter. Mr. Allan Burns remarked that softening and perforation sometimes occurred in persons dead of chronic diseases; and he conceived that the gastric juice could exercise its solvent power, not only after being poured forth into the stomach, but also while still contained in its proper vessels. To this opinion he was led by finding perforation on the anterior face of the stomach. By degrees the simplicity of Hunter's doctrine was obscured and frittered away by hypothetical notions, propounded chiefly by continental writers, respecting the accessory operation of disease in producing these softenings. It was held that either the gastric juice, through some vice of the nervous system, was secreted of an unusually acrid and corrosive quality;—or that the mucous membrane of the stomach was rendered, by some previous morbid condition, more than commonly soluble in its own secretion after death. At length, the effect of the gastric juice was lost sight of altogether, and softening of the mucous membrane was ascribed to the sole agency of a kind of inflammation, or to faulty nutrition.

More than fifty years after the publication of Mr. Hunter's first paper on the subject in the *Philosophical Transactions*, Sir R. Carswell endeavoured to bring pathologists back to the truth, in a French essay, read before the Royal Academy of Medicine in Paris, of which a version is to be found in the thirty-fourth volume of the *Edinburgh Medical and Surgical Journal*. He therein shows that the action of the healthy gastric juice is sufficient to account for changes which—by Chaussier, Broussais, Louis, and others, abroad, and by Dr. John Gairdner in particular in this country, since the time of Hunter, as well as by Morgagni, and still earlier pathologists before that time,—had been attributed to the operation of disease. The whole subject has finally been surveyed and simplified by Dr. Budd, who has cleared up several of the difficulties that surrounded it, and explained some apparent anomalies, in the Croonian Lectures delivered before the College of Physicians in 1847. The substance of these lectures has since been published in his excellent book, already referred to, on diseases of the stomach.

For perforation of the stomach, or for softening of its tissues, by the gastric juice, three conditions must concur. In the first place the stomach must (of course) contain gastric juice; which appears to be secreted directly into its cavity, and never to be retained, as Allan Burns supposed, within its coats. Secondly, that fluid must possess its natural quality of acidity. Thirdly, a certain degree of heat is requisite for its solvent operation.

Now it has been proved by Spallanzani, and more clearly and fully by the interesting observations of Dr. Beaumont, to which I shall hereafter more particularly refer, that during the state of health, no gastric juice is secreted into the stomach, except under the stimulus of food, or of some mechanical irritation, applied to its interior.

Hence we perceive why it is that perforations of the stomach, of the kind in question, are most of all to be expected when a healthy person is suddenly killed by violence, soon after a meal, and while the process of digestion is in progress.

But instances do occasionally happen (Dr. Budd relates a very remarkable one) in which the same kind of perforation is met with, although no food had for some time before death been admitted into the stomach.

Dr. Budd believes the secretion of the gastric juice to be a reflex process; which he assimilates to, and illustrates by, the secretion of tears. Tears may be presently made to flow by direct mechanical irritation of the conjunctiva, or, indirectly, by pungent vapours acting upon the nostrils, or by certain feelings of the mind. In like manner the secretion of the gastric juice may, he conceives, be excited, not only by some stimulus applied immediately to the mucous surface of the stomach, but also under certain diseased conditions or injuries of distant organs, (as the brain, and lungs) and even by mental emotion. In this way he would explain the occurrence of perforation or of softening, after death by blows on the head, when no food had been recently introduced into the stomach; and after death by pulmonary consumption.

That more or less digestion of the tissues of the stomach after death is exceedingly common, is a fact which was well known to John Hunter, but which has been lost sight of by the majority of more recent observers. "There are few dead bodies" (he writes) "in which the stomach is not at its great end in some degree digested; and one who is acquainted with dissections can easily trace the gradations from the smallest to the greatest."

Dr. Budd points out circumstances which frequently interfere to prevent this effect of the gastric juice, by annulling one or both of the other conditions, just now mentioned.

The solvent property of the fluid is arrested whenever its acidity is neutralized by the admixture of an *alkali*. This has been fully proved by Spallanzani and others. The same is true of *alcohol*. But in the last moments of slowly ebbing life, medicines containing ammonia, and alcohol in some form or other, are very commonly indeed poured into the stomach. Moreover, if the gastric acid happen to be present in small quantity, "it may be neutralized, and thus rendered inert after death, by transudation of the alkaline serum of the blood."

Cruveilhier found softening of the fore part only of the stomach, in a person who had died of fever, with marked disorder of the brain. Allan Burns also records a case of perforation of the anterior of the stomach; the patient was anasarcaous. In both instances the stomach was empty; *i. e.*, "its surface was merely moistened by the gastric juice." Dr. Budd supposes that in the first of these cases, the blood, remaining fluid after death, gravitated to the lowest part of the organ, and there gave out its alkaline serum, whereby the small quantity of gastric acid collected in its fundus was rendered neutral and inert; and that, in the second case, the alkaline dropsical fluid, oozing through the coats of the stomach at its lowermost part, had the same effect.

Again, the gastric juice is solvent of those things whereof it is the natural menstruum, at a certain temperature only. Probably it is most active at or about the standard temperature of the body. Below 60° its digestive action is found to be feeble, or extinct. Softening therefore—and *à fortiori* perforation—are more likely to take place in summer than in winter; in warm weather than in cold; in a heated room than in the cool open air; and after some modes of dying, which imply a long retention of the vital warmth, than after others.

I have mentioned, for the sake of explaining them, certain exceptional cases, in which the front of the stomach was digested; but the rule is that the softening, which usually comprehends a considerable space, happens almost always at its largest end, and in its lowest part, where whatever fluid it may contain collects under the influence of gravity. If the surface be wrinkled into folds or ridges, the summits of those ridges may alone be dissolved. Sometimes the stomach, lying across the vertebral column, is partially supported by it, and two little pools, and two spots of softening, are formed, one of them to the right of the spine, towards the pylorus. Now and then the gastric juice passes out of the stomach into the œsophagus, or into the duodenum, and these parts exhibit traces of its action. And when actual perforation occurs, the chemical solution of the animal tissues sometimes extends further; and the organs immediately opposite to the aperture undergo the digestive process:—the

spleen, the intestines, the liver, the diaphragm, nay, after penetration of the diaphragm, even a portion of the lung.

These apertures produced by the gastric juice have soft and ragged edges, and are irregular in their size and outline. When the softening has stopped short of perforation, the mucous membrane looks and feels pulpy, like paste, or is completely dissolved and gone. The pulp varies in colour from brown to gray, according to the quantity of blood contained in the part. The blood-vessels that ramify over the softened portion are rendered black, or brown, and therefore conspicuous, by the effect of the acid on their contained blood; or, if they are empty, the surface is pale, and presents that uniform, semi-transparent, jelly-like aspect which, under the name of "gelatinous softening" has been erroneously spoken of as the result of disease.

The stomachs in which this agency of the gastric juice is discernible show no marks of putrefaction: there is no extrication of gas, nothing of the fœtor of gangrene; but their interior always exhales a peculiar acid odour, and litmus applied to the softened spots turns red.

Chronic *ulcers*, on the contrary, affect chiefly the lesser curvature of the stomach, and are commonly situate nearer the pyloric orifice than the cardiac: their margins are thickened by inflammation; or, if not thickened, the edge of the hole in the mucous membrane is smooth and regular. They are not necessarily associated with a sour smell, nor with acid reaction upon litmus paper.

Perforations of the stomach by corrosive poisons are discriminated from other perforations, by the specific chemical tests of the presence of those poisons; by the amount of disorganization which they have produced; by traces of their corrosive action upon other parts—in the mouth, in the fauces, in the œsophagus; by the violent symptoms which precede the fatal result of their operation; and (often) by the history and moral features of the case.

If you bear in mind the particulars that I have thus hastily brought together, they will, I trust, enable you to avoid wrong inferences, which you might otherwise be led to form concerning the morbid appearances and real conditions of the tissues of the stomach, laid open to your inspection after death.

[Acute gastritis is a disease of by no means unfrequent occurrence during infancy, and then often causes a softening of the mucous membrane of the stomach, to which much attention has of late years been directed by the observations of Cruveilhier, Louis, Laisné, Billard, and others; by several of whom it is described as a specific disease. It is to this particular lesion that we wish to direct especial attention on the present occasion, referring the reader for information on the subject of the inflammatory affections of the stomach generally, that occur during the early period of life, to the Editor's Treatise on the Diseases of Children.

The symptoms of the particular form of acute gastritis, to which we have reference, are very accurately described by Billard. The disease commences with the phenomena of a violent inflammation of the stomach; as tension of the epigastrium, which is painful to the touch; frequent vomiting, not only of the milk and drinks taken, but also of a green or yellow fluid; the vomiting occurring every moment, without any reference to the period when the child has taken food or drink. There is, sometimes, diarrhoea, varying in different subjects, and returning after having ceased for a day or two; the matters discharged from the bowels being often green, and similar to those brought up from the stomach by vomiting. The extremities are cold; the pulse is generally irregular; the countenance has a permanent expression of suffering, the face remaining furrowed, as if the infant were crying; the cry is expressive of pain; the respiration is interrupted, and the general agitation so great that the existence of a cerebral affection might be suspected. At the end of six, eight, or fifteen days, the patient sinks exhausted from the want of sleep, the constant vomiting, and pain. In very young infants the disease is attended with little or no fever.

Dr. Iselin, of Mühlheim, who had an opportunity of studying the disease during its extensive prevalence as an epidemic at Göttingen, gives the following description of its phenomena:

Previous to the attack, the child is often, for several days, unusually restless and fretful—finally, a decided febrile attack occurs, attended with a quick pulse, hot and dry skin, intense thirst, sleeplessness, and constant vomiting, increased upon food or

drink being taken into the stomach. To these symptoms is speedily added a copious diarrhoea—the dejections being, at first, of a greyish colour, and of some degree of consistence, but subsequently of a yellowish or greenish serum, decidedly acid. The evacuations are usually preceded by contortions of the countenance, indicative of pain, which are likewise induced by pressure on the abdomen. The latter is always much, often enormously distended, and its temperature, especially at the epigastrium, is often considerably elevated, while the extremities are cool, or even decidedly cold. The patient exhibits great prostration of strength, and a profuse partial perspiration soon breaks out, particularly about the head.

The disease varies in its duration—proving fatal, in some cases, within twenty-four hours, and in others, not until after a continuance of several days, or even weeks.

In the more violent cases, its onset is very sudden—and it is attended with frequent fainting, and an irregular and very quick pulse—the eyes and features are sunken—the body is bathed in a profuse perspiration. Convulsions, more or less severe, often attended with strabismus, sooner or later, occur; the face becomes of a bluish tint; the diarrhoea or vomiting, or both, soon become suspended; the accessions of fainting and convulsions become more frequent, and during one of these, death usually takes place.

The intumescence and augmented heat of the abdomen, particularly of the epigastric region, and the coldness of the extremities, are, according to Dr. Iselin, to be considered as among the most constant and characteristic symptoms of the disease. The agonized expression of countenance, and sunken appearance of the eyes, are likewise almost invariably present, even from its earliest stages.

The age at which this form of gastritis usually occurs is within the first year; it is not, however, necessarily restricted to this period of life.

The most constant lesion discovered after death is a softening of the mucous membrane of the stomach, and often of the small and great intestines; the membrane being reduced to a disorganized and gelatinous pulp; or, the softening may extend to all the tissues of the stomach, rendering them liable to be perforated by the slightest force. In the stomach these softenings occur, according to Baron, Billard, and Iselin, especially at the great curvature, seldom extending beyond the most depending parts. Distinct traces of inflammation are occasionally observed, surrounding the softened parts, and patches of inflammation often exist, also, in different portions of the intestinal canal, especially in the small intestines. The parietes of the stomach frequently present a serous infiltration.

Carswell, Burns, Gairdner, Hope, and several other pathologists, deny the connection in these cases, between the softening of the gastro-intestinal mucous membrane and inflammation, and maintain that, in general, it is to be viewed as a cadaveric phenomenon, resulting from the action of the gastric juice upon the tissues of the stomach; others, with Jæger, Zeller, and Camerer, refer it to a paralysis of the nerves of the stomach, with increased acidity of the gastric juices, by which the tissues of the organ are dissolved during the lifetime of the patient. A somewhat similar opinion is entertained by Laisné, Chaussier, Desbarreaux, Bernard, and others. Without denying that the softening observed after death in the stomachs of children may be, in many cases, strictly a cadaveric phenomenon, and that, in other cases, it may result, during the lifetime of the patient, from other causes than inflammation, we are, nevertheless, well convinced from the result of repeated and cautious observations, that the gelatinous softening so frequently met with in the stomach of those infants who die after exhibiting all the symptoms of acute gastritis, is invariably the effect of an intense inflammation, commencing in the mucous tissue of the organ.

Softening of the parietes of the stomach, as well as of the intestines, unquestionably often takes place previously to the death of the patient, in cases where it cannot be supposed to be the result of inflammation. Thus, we not unfrequently observe in infants brought up by the hand, or improperly fed subsequently to weaning, a loss of appetite, peevishness, great restlessness, and want of sleep; the tongue becomes coated with a layer of white or yellowish mucus; and in some instances aphthæ appear upon the parietes of the mouth, and the breath has usually a decidedly acid odour. There are a constant diarrhoea, and intense thirst. The diarrhoea, after a time, frequently diminishes, or entirely ceases—but soon returns again with increased violence—the discharges being a thin serous fluid of a yellow or greenish hue, and having a very

strong acid smell; great emaciation and exhaustion soon ensue; the face and extremities become cold; the pulse small and irregular; the respiration quick and short. The child utters continually a low piteous moan, or lies upon his back with the eyes fixed, glassy, and half closed. No pain or tenderness is indicated upon gentle pressure of the abdomen; the latter is, however, often greatly swollen and tympanitic. The child becomes more and more exhausted, and, finally, expires quietly, and without convulsions.

This train of symptoms, with slight variations, marks that form of disease described by Camerer, Pommer, Hergt, Romberg, Droste and others, as gastro-malacia, and in which the stomach, and often the intestines likewise, present a gelatinous softening of their parietes, to a greater or less extent, but without the slightest indication of inflammation. The softening appearing to depend upon a diminished cohesion of the tissues—the result probably of disordered or suspended nutrition.

Acute gastritis as it occurs in the infant, is under all circumstances, and throughout all its stages, difficult to manage, and but little under the control of remedies. At its very onset leeches to the epigastrium, followed by warm fomentations or soft emollient poultices, assiduously applied, will be found advantageous. Internally, minute doses of calomel will often remain upon the stomach and allay the excessive irritability of that organ. We have occasionally combined the calomel with the acetate of lead: one-fourth of a grain of the first with half a grain to a grain of the second, will, in a large number of instances, very speedily control the vomiting and purging, and afford us time to reduce the local inflammation by leeches to the epigastrium, followed by blisters, kept on for one or two hours, and then replaced by an emollient poultice. The thirst of the child should be allayed by small quantities of some bland mucilaginous fluid, given cold, and repeated at short intervals. Slightly astringent and bitter infusions are recommended by some practitioners during the period of convalescence, and we think that we have seen very decided benefit result at this period from the proto-carbonate of iron in moderate doses.—C.]

The stomach is very frequently the seat of specific malignant disease; of cancer in its various forms and denominations. The fatal nature of this complaint; the obscurity in which it is sometimes wrapped; the possibility of overlooking it altogether, or of confounding it with disease of a more innocent character, combine to invest it with peculiar interest.

Carcinoma of the stomach has sometimes no symptoms at all, or none which the most sagacious practitioner would refer to the organ affected. Not long since I saw, in consultation, an elderly clergyman, who complained of pains in his back, which were brought on or aggravated by certain movements of the body. His bowels were costive: and purgatives always relieved his pains. He was passing lithic acid gravel. The pains were felt in or near the renal region. Several years before he had suffered in a similar manner; and had then been cured by being cupped on the loins. What was the matter here? Was it lumbago? Was there a calculus in one of his kidneys? These were the best guesses that I could make. The eminent physician whom I met, and a surgeon of no less eminence, who had seen the patient previously, had not been able to attain any more exact diagnosis. Upon this gentleman's death, which occurred not long afterwards, his disorder was discovered to have been cancer of the stomach. Excepting slight sickness a day or two before he died, there had been no symptom to direct attention to that part.

A young woman came into the Middlesex Hospital, under one of my colleagues, with a pulsating tumour in her epigastrium. It was thought, at first, to be an aneurism, and the case attracted, on that account, a good deal of notice. But the tumour subsided very much after free purgation. This led some to suppose that it was formed by accumulated fæces in the transverse colon. There was no sickness; nor indeed any one symptom referable to the stomach. She died. The tumour was cancerous; and in the stomach. Lying in front of the abdominal aorta, it had been lifted by its pulsations.

I was summoned to one of the hotels in Albemarle-street, to see a gentleman between forty and fifty years of age, who was on his way home from the Scottish Highlands, where he had been deer-stalking, and shooting grouse. He had been seized in the night with deadly faintness, very rapid breathing, and severe pain which

he referred to the sternum. I could detect no fault in his heart, or in his lungs. His epigastrium felt full and pulpy. The next night he had a similar paroxysm, in which he died. His body was examined by Mr. Paget. The lungs and heart were sound in structure. The large curvature of the stomach presented, throughout its whole extent, a mass of scirrhus lying beneath and among thick ridges of mucous membrane, with two or three deep patches of ulceration. Both the cardiac and the pyloric orifice were free from change.

This gentleman had gradually lost flesh and strength; but the only definite symptoms of which he had complained were sour eructations, and a total loss of appetite, and repugnance to take food.

Cases to the same effect are related by Dr. Seymour, in the *Medico-Chirurgical Transactions*; and by M. Andral, in his *Clinique Médicale*.

But even when the stomach is the organ pointed out, by the symptoms, as the probable seat of the malady, those symptoms fail, often, to indicate with any certainty its nature. The effects of the carcinomatous disease exhibit no uniformity. The ingestion of food is apt to produce great distress; but differently in different cases: sometimes as soon as the food is swallowed; sometimes not for an hour or two afterwards. Some cases are attended with much pain; some with none at all. One patient vomits continually; another has little or no vomiting from first to last.

Can these differences be in any way accounted for? Partly they may. By analyzing case after case, we approximate to a knowledge of their causes. But this knowledge is yet far from being complete.

One circumstance that has a considerable influence upon the symptoms, is the *situation* of the disease. In respect to this point there are certain general rules which are for the most part true. Still we can speak of them only as applicable *on the average*; they are not absolute or infallible.

The rules I mean are these:—

1st. That there is more suffering, *ceteris paribus*, when the cancerous disease is situate at, or very near, either extremity or orifice of the stomach, than when it occupies the intermediate parts: whether in the greater, or in the lesser curvature.

2d. That when the cardia, and its immediate neighbourhood, is the part solely or principally diseased, the food and drink find a hinderance in passing the stomach; but being once there, the distress is over. The symptoms are very like those of strictures of the œsophagus. The morsel reaches the bottom of that tube, and there causes uneasiness, till at length it is brought up again through the mouth, or passes gradually in the natural direction.

3d. That when, on the other hand, the disease is limited to the pyloric end of the stomach, the food enters that bag readily enough, and remains there for a certain time; then uneasy sensations arise, and the imperfectly digested meal is apt to be rejected by vomiting.

It is the difficulty of passing the *doorway* in these cases, that gives rise to the principal suffering: the difficulty of getting into, or the difficulty of getting out of, the stomach. But when the disease is confined to the intermediate space, no such difficulty occurs: and therefore little or no pain.

You must expect, I say, to meet with individual variations from these rules. A remarkable example of such variation was presented by one of my hospital patients, in the year 1837. I have the notes of that case before me, which I will read *short*.

Simon Ailes, aged thirty-six, admitted March 14. His main complaint was of pain in the epigastrium, always present, but augmented, in frequent paroxysms, to an extreme degree of severity. At first, pressure gave him some relief. The pain was most violent an hour or two after he ate. He was troubled also with flatulence, and with sour eructations. Occasionally a clear tasteless fluid, looking like water, rose into his mouth. His bowels were costive.

At this time his countenance was natural and placid; but it gradually assumed that pinched and anxious expression, and that peculiar yellowish hue, which are so significant of organic visceral disease. He wasted fast. At length the epigastrium became tender as well as painful: but no tumour, except the left edge of the liver, could be felt there. He died on the 11th of May, about eight months from the commencement of the pain. A week before his death he vomited some dark, grumous, offensive fluid, evidently containing blood. With this exception he had no vomiting.

Many remedies were tried, which I do not specify, for none of them gave him any sensible or continued relief.

In the smaller curvature of the stomach we found a ragged, sloughy surface, as big as the palm of one's hand, and extending to within half an inch from the pylorus. A section of this diseased portion exhibited the characters of true scirrhus: a white and hard mass, nearly half an inch across at its thickest part. The mucous membrane of the duodenum was congested, and dark coloured. The rest of the intestines were healthy except the rectum, which was surrounded, towards the anus, by scirrhus and thickened areolar tissue, intermixed in laminae. The gut itself was not affected.

The diseased stomach was removed, and examined by Mr. Kiernan, who found, upon careful dissection, that the trunk of the gastric branch of the par vagum ran directly into, and was lost in, the scirrhus mass. This sufficiently accounted for the dreadful sufferings of the patient.

And I am here reminded that, with regard to the structural alteration itself, there are some circumstances well worth attending to.

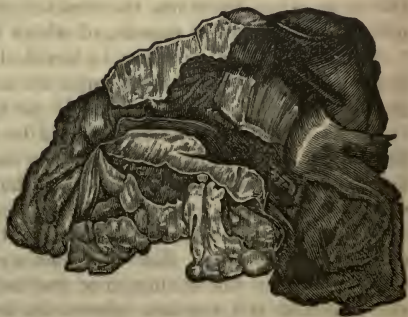
Andral places all these organic affections of the stomach in the class of chronic gastritis. But it is clear that he is wrong: and you will perceive at once that it is of immense importance to recognize the specific disease from the mere result of common inflammation.

But though cancer is not, in any case, a mere product of common inflammation; neither is all that is called cancer really such. Specimens of morbid texture, misnamed scirrhus of the pylorus, are not uncommon in anatomical museums. I show you some from our own; not so denominated, however. The correct labelling would be *hypertrophy*. You may perceive that the areolar and the muscular tissues near the pylorus, are very much thickened. A section of the thickened parts presents an appearance somewhat like horn; and is crossed by whitish lines that run nearly parallel to each other. The morbid structure is quite definite and uniform; and very dissimilar, in that respect, to the irregular masses of scirrhus, and to the amorphous deposits of encephaloid cancer. Neither does it at all resemble that of the colloid variety of carcinoma. Changes of this kind are liable to occur in the muscular tissue of this, as of other organs, whenever a permanent obstacle is opposed to the onward progress of the contents of the hollow viscus. The impediment may have been originally produced by inflammatory thickening of the textures composing the pylorus; and then the hypertrophy may, in a certain sense, be accounted a consequence of inflammation. To that extent alone is M. Andral right. He has unquestionably pushed his theory on this subject too far. Whatever narrows the pyloric orifice leads to increased effort of the propelling muscle, and to augmentation of its bulk and power. Now cancer itself, situated at, or close upon, the pylorus, may impede the exit of the digested aliment; and then it causes a gradual hypertrophy of the muscular coat. In these cases there is a mixture of the two changes; of the cancerous growth, with the muscular hypertrophy: and this is one reason why they have been confounded together. Here are several preparations, exhibiting true cancer of the stomach. At the bottom of each bottle may be seen a sort of whitish powder or sediment, consisting of some of the matters peculiar to cancer. This fact has been pointed out to me by Mr. Kiernan in the numerous specimens contained in his private collection. But there is no such deposit when the hypertrophy is not combined with malignant disease.

To those who are conversant with its revelations, the microscope, in equivocal cases, becomes a valuable diagnostic test.

It has long been thought and asserted, that the cancer of the stomach is not so apt to be attended, as cancer of other parts, with a disposition to present itself in various organs of the body at the same time, or

FIG. 108.



Scirrhus Pylori. At the diseased part, the walls of the stomach are extremely thickened, and of a whitish colour.

in succession. Now I believe—and I am glad to add the weight of Mr. Kiernan's authority to my statement—that this is not really so. Cancer is a constitutional affection: or, if local and solitary in the outset, is prone to disseminate itself. The error has arisen out of that confounding of one morbid condition with another, against which I have been warning you. Instances are not at all uncommon of thickening of the areolar and mucous tissues about the pylorus, producing first a mechanical impediment to the passage of the food, then more energetic muscular efforts towards its expulsion from the stomach, and at last hypertrophy of the muscular tunic. In these cases, you do not find cancer in other organs: because, in fact, there is no cancer in the stomach.

You may say that as both forms of disease are alike fatal, it signifies nothing whether there be really cancer or not. But it is always satisfactory to clear away an apparent anomaly, and to show that it has no real existence. Besides, you know with how much anxiety the relatives of the dead inquire concerning these matters. That cancer “runs in families” is well understood even by the public. An example of this hereditary disposition has just occurred to me in practice (1853). A patient of mine, a barrister, forty-eight years old, has sunk under scirrhus disease involving the omentum, and extending into all the folds of the peritoneum. This gentleman's mother died of malignant disease of the leg, where it commenced as a small wart. Her brother died of cancer of the lungs which penetrated the sternum, and sprouted out upon the chest. And this brother's wife, who was his cousin also, fell a victim to some form of cancer. The distinctions which I have been pointing out are surely worth learning, if they do no more than enable us to comfort the minds of survivors, and to relieve them from the apprehension that they also may be doomed or likely to become the subjects of this horrible disorder.

Notwithstanding what I have now been saying, it must be allowed that genuine cancer of the stomach is accompanied less frequently than some other modes of carcinomatous disease, by cancer elsewhere. In fact cancer of the stomach is most often of the scirrhus variety, which until it softens is not so readily disseminated as the others, and which is apt to prove fatal before it softens.

When primary cancer of the stomach is of the encephaloid kind, — or when, being scirrhus, it begins to grow soft,—its secondary manifestation occurs chiefly, as I told you on a former occasion, in the *liver*: the reason being that the veins of the stomach communicate with the general system not directly, but through the hepatic capillary system. Cancer of the colloid or gelatiniform species, with which also the stomach is liable to be affected, spreads mainly, when it spreads at all, by inoculation of parts of the body that happen to be in contact with it.

But to return to the *symptoms* of carcinoma of the stomach. In some cases, I say, the food is rejected by vomiting; in some cases it is not. Now it has been argued that this difference depends upon the condition of the pyloric outlet; whether it be free and open, or contracted and shut. The explanation is more plausible than sound.

It is not strictly consistent with facts. Vomiting of the food has been an urgent symptom, when there was no mechanical bar to its passage into the duodenum. The pylorus is a sphincter muscle, of which the natural and habitual state is that of contraction. It yields, however, in health, to the pressure of the digested aliment, which is driven forwards by the muscular fibres that surround, and compress by their action, the *pyloric end of the stomach*. If there be a mechanical impediment, that affords a sufficient reason why the food should be thrown up again. But sometimes, I repeat, the orifice is wide open, and yet the food is rejected: and it is rejected because the disease so involves the pyloric end of the stomach, that the propelling force cannot be exercised.

When there is a mechanical obstacle, the disposition to hypertrophy of the muscular coat is conservative. But in feeble and delicate persons, the baffled muscles may never acquire strength enough to overcome the impediment; and then the very opposite condition is apt to take place: the coats all become very thin; meal after meal is retained; the stomach is enormously distended, and relieves itself now and then, at distant intervals, by copious vomiting; until at last it is unequal to that effort, and the patient dies.

Sometimes the sickness and vomiting are urgent even when the stomach contains no food: and the matters rejected are of various character and appearance. They

often resemble coffee-grounds, and consist, no doubt, of altered blood. Vomiting of this kind is a very pregnant sign of *organic* mischief in the stomach.

Emaciation is another ugly circumstance in these cases; and forms a strong ground of presumption that the symptoms depend upon structural disease. Yet it is not a uniform consequence, even of malignant disorganization of the stomach. Napoleon Bonaparte was very fat when he died. His omentum is described as having been "remarkably fat:" and "the fat was upwards of an inch thick upon his sternum, and one inch and a half upon his abdomen."

The existence of a palpable tumour strengthens the unfavourable diagnosis. But this is far from being a constant phenomenon. It is not even pathognomonic when it does occur. The diseased head of the pancreas has been mistaken for a thickened pylorus. The stomach is liable also to be dragged much out of its place; and then a thickened pylorus may be mistaken for something else. Sometimes the form of the stomach may be distinctly traced. In the person of a medical practitioner who died lately in this neighbourhood, the shape of the organ, its occasional peristaltic motions, and the irregular and hardened pylorus, were plainly to be felt. They might indeed almost be *seen*, in the hollow and attenuated abdomen. When a tumour is ascertained to belong to the stomach, it indicates disease of the pylorus rather than of the cardia.

In equivocal cases the diagnosis may sometimes be aided by remembering the facts that cancer of the stomach rarely happens before the age of 35; that it is steadily progressive, and generally kills the patient within 12 or 15 months; and that it is attended with a peculiar sallowness of complexion, and gradual wasting of the flesh: whereas chronic ulceration of the stomach may arise at any period of life, and may continue to exist for 5, 10, or even 20 years together, without much constitutional disturbance, or marked diminution of the general bulk and strength.

It is a curious feature in these malignant diseases of the stomach, that the symptoms sometimes remit, in a remarkable manner; so as to excite a hope in the mind of the patient, and in that of his medical attendant, that the nature of the malady had been mistaken, and that recovery is about to take place. But the truce is not for long. Frightful disorganization is at length produced, ragged ulceration, perforation of the coats of the stomach, adhesion to the parts adjacent, which thus are constituted adventitious walls; — and inevitable death at last.

The treatment of this dreadful complaint can only be palliative. If there be pain, we are driven, sooner or later, to opium. Anodyne enemata have often as good an effect in relieving the pain as opium given by the mouth; and they have this advantage, that their constipating properties are more easily obviated than when that drug is put into the stomach. Nutritive injections are proper when food taken through the natural channel is not retained.

Other palliative measures may be aimed at particular symptoms: of these I propose to speak when I come to the symptoms and remedies of *dyspepsia*.

LECTURE LXIX.

Hæmorrhage from the Stomach: sometimes from a large vessel, usually capillary. Idiopathic Hæmatemesis. Vicarious Hæmatemesis; Hæmatemesis from Gastric disease or injury; from disease in other organs. Melæna. Hæmatemesis from a morbid state of the blood. General phenomena of Hæmatemesis. Diagnosis. Treatment.

HÆMORRHAGE from the stomach, to which I wish next to direct your attention, is much more frequent occurrence than acute gastritis. It is a complaint, or a symptom, that presents several points of interest and importance. I use the phrase "hæmorrhage from the stomach," rather than the single term "hæmatemesis,"

because that term, signifying strictly a *vomiting* of blood, does not necessarily imply hæmorrhage *from the stomach*; nor, indeed, does it always accompany such hæmorrhage, although it is one of its most common and most striking symptoms.

What I have so frequently mentioned in respect of hæmorrhages from the mucous membranes generally, viz., that the efflux of the blood is seldom owing to the rupture of a large blood-vessel, holds true in this. It can rarely happen that any vein or artery belonging to the stomach is divided or laid open by accidental injury, so as to pour forth its blood. When hæmorrhage does proceed from one or more of the larger blood-vessels, the opening by which the blood escapes is commonly the result of chronic ulceration; such as I spoke of yesterday. Sometimes hæmatemesis is a consequence of the erosion of a blood-vessel by an ulcer in the *duodenum*; such as I mentioned before as being apt to follow severe and extensive burns. But hæmorrhage from the stomach, and from the alimentary canal generally, is far more commonly what we have agreed to call *capillary hæmorrhage*.

Now this kind of hæmorrhage happens under various circumstances; and is attended with different degrees of danger. 1. The bleeding may be idiopathic. 2. It may be vicarious of some other habitual hæmorrhage. 3. It may depend upon disease or injury of the stomach itself. 4. It may be the consequence of disease situate elsewhere, and producing, mechanically, a plethora of the veins of the stomach. 5. It may result from a morbid condition of the blood, and form one symptom of a more general disease; as in the passive hæmorrhages of purpura and sea-scurvy. Each of these varieties requires a short notice.

1. Hæmorrhage strictly *idiopathic*—*i. e.* independent of any apparent change of texture, whether in the surface itself, or in any part obviously capable of influencing its blood-vessels—is as rare, I believe, from the mucous membrane of the *stomach*, as from that of the *lungs*. I have never seen, nor do I recollect to have read of, any instance of hæmatemesis analogous to the *epistaxis* which is so common in children and young persons; and which affords the most familiar example of idiopathic hæmorrhage.

2. But hæmorrhage from the stomach, occurring in connection with other constitutional hæmorrhages, or in their stead—and above all, occurring *vicariously* of menstruation—is *abundantly* common. It is the most common indeed of all the species of hæmorrhage *by deviation*. I told you, in a former lecture, that patients will sometimes menstruate for years together through the lungs; without any apparent injury to their general health. More commonly still do they menstruate through the stomach. I will mention one concise but singular example of this which I had from Dr. Latham, and which came within his own knowledge. A young woman became the subject of hæmatemesis, recurring at monthly periods, about the age of fourteen. She had never menstruated. This continued until she married and, in due time, fell with child. Thereupon, the hæmatemesis ceased. She brought forth and suckled her infant. During lactation the hæmorrhage did not recur. It came on again soon after she ceased to nurse the child; no regular menstruation by the uterus having ever happened. This was the woman's own account, and there appeared no reason to question its accuracy.

Gastric hæmorrhage of this kind, vicarious of regular menstruation, is not generally thought to have any tendency to shorten the existence of those who are afflicted with it. Cullen states broadly that this species of hæmatemesis is hardly ever a dangerous disorder: and this is true. Yet it is not so *entirely* free from peril as to preclude the necessity of *some* caution and qualification in stating the prognosis. The exhaustion from the mere loss of blood is sometimes so great as to create serious alarm for the patient's safety. And Mr. North has recorded (in the *London Medical and Physical Journal*) two instances in which suppressed menstruation was followed by repeated and at length *fatal* hæmatemesis. In neither of these women was the health seriously deranged; nor, previously to the hæmorrhage, did there exist debility, or any other symptom calculated to excite the apprehension of danger. In fact, in both of these cases, a strongly favourable prognosis was given by experienced physicians a very short time only before the fatal event.

3. Gastric capillary hæmorrhage is often a consequence of *disease or injury of the stomach itself*. It is sometimes one of the earliest declaratory symptoms of scirrhus or cancer of that organ—occurring long prior to ulceration. Hæmatemesis attends

also, very commonly, the *ultimate* stages of that fatal disease: and then it may be owing to the erosion of some vessel of notable magnitude, in the course of the process of disorganization, as in the examples already spoken of: or (what I believe is far more common) it may result from a kind of general oozing from the ulcerating surface. Blood is often vomited soon after the reception of strongly irritant poisons into the stomach. I show you again Dr. Roupell's plate, representing the crimson surface of a portion of the stomach of a dog which had been killed shortly after the administration of a dose of alcohol. The intense congestion thus produced is doubtless *active* congestion; congestion belonging to inordinate *arterial* action. Pushed a degree further, such congestion passes into hæmorrhage.

4. On the other hand, intense *passive* congestion—congestion arising from the detention of blood in the *veins* by some mechanical obstacle to its progress—is a very common source of gastric hæmorrhage. Hæmatemesis is therefore an occasional symptom of obstructive disease of the heart. Much more frequently, however, it depends upon abdominal changes. The hæmorrhage is symptomatic of disease situate, not in the stomach itself, but elsewhere. And the viscera, with the diseases or morbid conditions of which, bleeding from the stomach is most often connected, are the liver and the spleen.

All this is well known: and it is easy to see, from the peculiar construction of the venous apparatus in the abdomen, how disease of one or both of these viscera may produce mechanical congestion of the submucous capillary tissue; and how that congestion may be relieved, under certain circumstances, by the effusion of serous fluid on the one or the other surface, constituting ascites or diarrhœa, as the case may be; or under *other* circumstances, not perhaps easily discriminated or well understood, by the extravasation of the collected blood itself. It would be superfluous to describe the peculiar distribution and functions of the vessels which return the main portion of the venous blood from the stomach and intestines towards the heart. It seems to me highly probable that one at least of the offices of the *spleen* is to provide a receptacle or reservoir for this blood, when its free passage through the portal vessels is temporarily obstructed. It then becomes a sort of safety valve (if such an illustration be allowable), which obviates the danger that might otherwise arise to more vital parts from any great or sudden disturbance of the venous circulation. The stress of the congestion is continually felt in the submucous capillary system; and the hæmorrhage which is apt in such cases to occur from the loaded membrane, receives a simple solution upon principles almost purely mechanical. Nay, the very circumstances which lead to the effusion of the blood from the *mucous* surface on the one side, rather than from the *serous* on the other, may perhaps be themselves susceptible of mechanical explanation.

Gastric hæmorrhage, symptomatic of hepatic disease, is chiefly to be looked for in those morbid conditions of the liver which imply obstruction of the portal vein and of its ramifications. We are not surprised, therefore, to find it coincident, often, with a contracted and shrunken state of that organ. The state of the spleen, on the contrary, for reasons that must be obvious to you, is uniformly, in the cases that we are now considering, a state of *enlargement*. And the augmentation of bulk is not so much to be ascribed to disease inherent in its proper texture, as to distension by the mere quantity of blood which it holds. The internal structure of the spleen furnishes a credible presumption in favour of that view of one of its uses to which I just now alluded; and this structure, and this presumed function, when considered together, throw a strong light upon some of the pathological relations of the spleen, which well deserve attention.

Numerous instances are on record of hæmatemesis going along with evident enlargement of the spleen; and in some of them that organ has been observed to diminish in bulk, in proportion as blood was poured out by the stomach. If I am not greatly mistaken, I have more than once seen this myself. In such cases the tumid condition of the spleen may be regarded as an evidence of venous obstruction *elsewhere*; and as depending, sometimes at least, upon disease of a less striking and prominent character in the liver, impeding the progress of the blood through the vena portæ. Of this kind would seem to have been a case related by Morgagni, wherein, after repeated attacks of hæmatemesis, under which the patient sank at last, the spleen was found to weigh four pounds, and to be gorged with dark blood: while the liver was pale

and exsanguine. Frank gives the history of a patient, who had vomitings of blood, and whose spleen, taken from the body after death, weighed sixteen pounds: the ordinary weight of the spleen in a healthy adult being from eight to ten ounces. In Latour's work on *Hæmorrhage*, which is remarkable for the number of examples it contains, collected from various sources, and amounting to nearly a thousand, several instances are detailed of this combination of splenic enlargement with hæmatemesis. One of these occurred in the person of a friend of his, who had been living in a malarious district, and who had laboured for nearly two years under obstinate intermittent fever. This was followed by an immense enlargement of the spleen—a great *ague-cake*—which came to occupy almost the whole of the abdomen. Latour's experience enabled him to predict that hæmatemesis would probably supervene upon this condition of the spleen; and, accordingly, one night he was called in a hurry to his friend, and found that he had vomited an enormous quantity of clotted blood. A great deal passed away through the bowels also. The hæmorrhage recurred from time to time, till in the course of a month the spleen was so far reduced in bulk, that it could no longer be felt in the belly: and the patient lived, and enjoyed good health, for twenty-five years afterwards.

It is necessary, therefore, in marking the connexion which frequently subsists between hæmatemesis and enlargement of the spleen, to guard ourselves against concluding that these two circumstances hold always the relation of cause and effect. In many such cases, probably in most of them, they are simply concurrent effects of one common cause; and that cause is chiefly to be sought in such morbid conditions of the liver—or of other parts within the abdomen—as are competent to produce a considerable impediment to the free transmission of blood through the system of the *vena portæ*.

When gastric hæmorrhage results from hepatic obstruction, there is almost always *intestinal* hæmorrhage also. At any rate there are almost always black alvine evacuations, like tar or dark paint. This form of disease has therefore been called *melæna*. The ancients supposed that the unnatural stools consisted of black *bile*.

Hæmorrhage from the stomach, independent of *disease* in that or in any other part, sometimes happens in the advanced periods of utero-gestation. Yet, though it does not result in these cases from disease, it is difficult to class it among idiopathic hæmorrhages. The want of periodical recurrence, and the absence of the hæmorrhage during the *earlier* months of pregnancy, are circumstances which sufficiently refute the old notion, that this form of hæmatemesis depends also upon the suspension of the catamenia. It is caused, no doubt, by the pressure of the gravid uterus, which impedes mechanically the venous circulation in the abdomen.

5. Gastric hæmorrhage, resulting from *changes in the blood* itself, occurs in scurvy, in purpura hæmorrhagica, and in the yellow fever. Being merely a symptom in these cases, it requires no separate consideration here.

When a *large* quantity of blood is poured into the stomach, whatever may have been its source, it appears to have a nauseating and emetic effect. At least the blood ejected in hæmatemesis is almost always considerable in amount. The vomiting may, for aught I know, be dependent on the mere distension of the stomach, which appears to be tolerant of the presence of the blood, up to a certain point, but no further. A small quantity may, doubtless, pass all of it onwards through the pylorus, after undergoing, more or less completely, the process of digestion in the stomach; and a *portion* of the blood pursues that course in most instances. But when it is vomited, it comes up in large quantities, usually of a dark colour, and more or less coagulated. Sometimes the coagula have evidently been moulded in the stomach; and sometimes clots are thrown up, partially deprived of the colouring matter of the blood, and resembling the fibrinous polypi so often met with in the cavities of the heart. Of course the degree of the coagulation of the blood, and of its separation into serum and crassamentum, will depend upon the time that it remains in the stomach; and this again would seem to bear a proportion to the rate of its effusion.

The blood that is vomited is almost always of a dark colour; while that which is coughed up is most frequently florid and bright. Why is this? We are told that the blood which comes from the lungs is rendered florid by the admixture of atmospheric air. But this is not the whole of the matter. Neither can we say that the

dark hue of the blood ejected in hæmatemesis is always, or solely, due to some morbid alteration effected in that fluid while yet circulating in its proper vessels. There is another cause, which, till of late years, was much overlooked, but which frequently changes the colour and appearance of the blood *after* it has been extravasated into the stomach; and that in so great a degree as sometimes to render doubtful, or to disguise altogether, the real nature of the fluid vomited. I mean the chemical agency of the gastric acid. The effect of acids in blackening the blood out of the body is well known; and it is somewhat singular that the ascertained existence of an acid secretion in the stomach, varying in quantity at different times and under different circumstances, was not sooner applied in explanation of the dark colour of the blood, and its occasional blackness, when vomited. The degree of blackness will be in proportion to the relative quantity of acid which it meets with in the stomach, and the intimacy of the admixture. Sometimes the blood is clotted and not very much altered in colour; sometimes it is grumous, brown, of a chocolate tint, or like coffee-grounds. This generally denotes the existence of *organic disease*; and the appearance of the blood is probably modified in some degree by the morbid process that leads to its effusion. There is good reason for believing that in the *black vomit* of the yellow fever, the colour of the blood undergoes alteration, even while it is yet circulating through the blood-vessels; but that the black appearance of the matter vomited is in great part owing to the chemical action of the gastric acid, may be inferred from the fact, that the fluid so discharged is always (so I am informed) intensely acid. Andral has described an effusion of black liquid into the stomach, as an example of *melanosis*. He states at the same time that an accurate analysis of the liquid showed its composition to be very nearly the same with that of the blood. May we not suspect that this inky fluid really consisted of blood that had been blackened, subsequently to its extravasation, by the acid with which it mixed in the stomach? Upon the same principle may be explained the dark brown, or almost black, colour of the spots which are sometimes seen (I presume when there has been a great superabundance of acid) in the substance of the mucous membrane of the stomach, or even beneath it; and which also have been set down as melanotic. They are so like, in all circumstances, except in the single particular of colour, to the crimson spots which are obviously formed by minute extravasations of blood in the same parts, that we can scarcely refer them to any other source. The slate-coloured patches, which I spoke of yesterday as being vestiges of chronic gastritis, depend likewise upon the blackening effect of the gastric acid upon the congested surface. We have the same dark colour of the effused blood, in many cases, when it is poured out in the intestines. Here, of course, its colour is not referable to the gastric juice; but it is blackened by some of the intestinal gases; probably by the sulphuretted hydrogen for example, or by the carbonic acid that enters into their composition.

There can be no doubt that this gastric acid, when intense in strength, or copious in quantity, is capable of changing the colour of the blood, after death, even while it is contained in the sub-mucous blood-vessels. In these cases it must be conveyed to the blood by imbibition. And the very same thing takes place when strong acids are introduced into the stomach from without. When, for instance, the sulphuric acid, or what is perhaps more to our present purpose, the vegetable oxalic acid, has been taken as a poison, it has the effect of blackening, and, as it were charring the blood, with which the membrane becomes loaded in consequence of the irritation produced by the poison. It does this when no destruction of the mucous membrane has been produced.

It is but justice to observe, that the credit of having been the first to perceive, and to explain, this cause of the blackened state of the blood, while yet remaining in its proper vessels, is due to Sir Robert Carswell.

When blood is ejected through the œsophagus and mouth, we have demonstrative evidence of the existence of *hæmorrhage*; and the *diagnosis* of *hæmatemesis* may appear to be so simple as to admit of neither mistake nor doubt. The diagnosis of *hæmorrhage from the stomach*, however, is really oftentimes difficult and obscure, and to be established by presumptive evidence alone.

In the first place, bleeding may take place from the mucous membrane of the stomach, and no hæmatemesis ensue, especially when the blood is poured forth in

small quantities, and slowly. In these cases the blood becomes visible only in the stools, where it may not be looked for, and where, if seen, it may not always be recognized, in consequence of the changes which it has undergone during its passage through the intestinal canal. And even supposing that its presence is detected in the alvine evacuations, it will remain uncertain in what part of that long canal it was effused. The hæmorrhage may even be profuse, and the patient may die, without any escape of the blood externally. There is a case related by Frank, in which death took place from hæmorrhage of the stomach without hæmatemesis; and both the stomach and the intestines were found distended by an enormous coagulum of blood which had assumed their form.

Even when the blood is ejected by the mouth, the exercise of some care and sagacity is occasionally, though not always, required, in order to determine the part from which it was originally poured out.

Thus blood may be swallowed, and afterwards vomited: and so we may have hæmatemesis without hæmorrhage from the stomach; just as we may have hæmorrhage from the stomach without hæmatemesis. There are cases of slow bleeding from the lungs, the fauces, the mouth, or the nasal cavities, where the blood, collecting in the pharynx, provokes, from time to time, an instinctive and involuntary act of deglutition; and thus is gradually accumulated in the stomach up to that point at which the organ becomes impatient of its contents, and ejects them by vomiting. This is very apt to happen during sleep, and especially to young children: and as the blood, *when vomited*, is coagulated, and in considerable quantity, it is scarcely possible to conclude, from its mere appearance, that it has proceeded from any other source than the stomach itself. If, however, we mistake such cases, our error is likely to produce much needless alarm, and to lead us to unnecessary activity in treating them. We are assisted towards forming a right judgment (when our attention happens to be directed to this source of fallacy), partly by the general history and symptoms, and partly by an examination of the mouth, fauces, and nostrils, to ascertain whether any coagula, or other marks of hæmorrhage, are visible on the mucous membrane belonging to those parts.

But blood may be swallowed knowingly and purposely, by impostors, and afterwards vomited. Hæmatemesis is one of the complaints which have frequently been feigned; either for the sake of avoiding some imminent punishment, or distasteful service; or with the view of exciting compassion, and of profiting by the contributions of the charitable and the credulous; or sometimes from a kind of wilful perversity, akin to insanity. In treatises on forensic medicine, you will generally find reference made to an instance of this kind recorded by Sauvages, in his *Nosology*. A young girl, who was anxious at all hazards to escape the constraints of a convent, pretended that she was suffering from violent hæmatemesis. In fact she did, for several days in succession, vomit large quantities of blood in the presence of the physician who had been summoned to her assistance. It was afterwards discovered that on each of those days she had swallowed blood which had been secretly conveyed to her from the neighbouring shambles. A case of precisely the same kind occurred (as I was informed by a gentleman who witnessed it) in the Bristol Infirmary some years ago. A girl had been long a patient there, labouring (as was supposed) under hæmatemesis; but it was at length discovered that she was a malingerer. She was in the habit of assisting the nurses in their work; and this afforded her opportunities — of which she availed herself — of drinking the blood which had been drawn from the veins of other patients: and this blood she afterwards vomited.

And even where no fraud is attempted, nor any blood swallowed, it occasionally becomes a nice matter to determine the origin of the hæmorrhage, when blood is ejected in large quantities from the mouth: to decide, namely, whether the blood has come originally from the *lungs* or from the *stomach*. In copious hæmoptysis, the blood issues from the mouth in gushes, as it does in hæmatemesis; and the reflux of the blood into the pharynx, the tickling sensation it there produces, and the cough (which we know, even when the expectation is not of blood, frequently excites retching); these causes, acting singly, or together, occasion sometimes a convulsive contraction of the muscles of the thorax, which *looks like* the effort of vomiting: they often, indeed, give rise to actual vomiting. On the other hand, in sudden and profuse hæmatemesis, the irritation caused by the blood

as it passes over the upper part of the larynx, is apt to provoke a paroxysm of choking cough.

Now, when I was speaking, some lectures back, of hæmoptysis, I promised that I would point out the means of distinguishing it from hæmatemesis, when I came to the consideration of the latter complaint. I have now therefore to redeem my promise.

However equivocal certain cases may be at first sight, we may generally guide ourselves to a correct decision by a careful investigation of the circumstances that *precede, accompany, and follow* the hæmorrhage. *Vomiting* of blood is commonly preceded by a sensation of weight and uneasiness in the epigastrium; and by nausea. Hæmatemesis is also, more frequently than hæmoptysis, ushered in by paleness of the face, dimness of vision, and an approach to syncope, or even actual fainting. These symptoms are not to be regarded (I apprehend) as premonitory of the hæmorrhage, although they have been so considered by some; they are rather a sign that it has already taken place; and yet they are preliminary of the hæmatemesis. Occurring before the blood comes up, they cannot be ascribed to alarm at the sight of it. On the other hand, hæmoptysis is wont to be announced by dyspnœa, cough, tickling in the throat, and a sensation as if of *bubbling* within the thorax. Most commonly too, before the expulsion of much blood from the lungs, some sputa are *coughed up*, composed more or less of that fluid. The symptoms that usually *succeed* the hæmorrhage in either case, afford equally valuable assistance to our judgment, in cases that might otherwise be doubtful. Generally copious hæmoptysis goes on, in a succession of mouthfuls, for some time; whereas there is, mostly, only one access of full vomiting. At any rate, at the close of abundant pulmonary hæmorrhage, the patient manifestly *coughs up*, and expectorates, smaller quantities of blood; while we usually may observe that, a few hours after hæmatemesis has occurred, slight griping pains come on in the abdomen, and a portion of blood is got rid of from the bowels.

Other questions, often of much importance in regard to the ultimate diagnosis, when the blood is traceable with certainty to the stomach, are, whether it be idiopathic, if, indeed, it *ever* be so: whether it be supplemental of some other discharge: whether it depend on disease of the stomach itself; of one, or more, of the contiguous viscera; or of the system at large. Certainly, in a very great majority of cases, gastric hæmorrhage is symptomatic; and the nature and seat of the disease of which the bleeding is a symptom, may, in many instances, be determined without much difficulty. That which depends upon *incipient* cancer of the stomach, while it is by no means of rare occurrence, is also, (I think) more frequently than other forms of hæmorrhage from that organ, obscure. It must be obvious to you, and therefore I need not dwell upon this part of the subject, that a little attention to the symptoms and past history of the patient will usually suffice to elucidate the nature of the case, where hæmatemesis supervenes immediately upon the introduction of corrosive poisons, or within a certain interval after they have been swallowed: where it depends upon the bursting of a large aneurism: where it breaks forth among other symptoms of scurvy or purpura: where it is the result of an *advanced* stage of cancer of the stomach: where it accompanies organic disease of the liver, spleen, or heart: where it occurs as a symptom of yellow fever: where it takes the place of suppressed or imperfect menstruation: or where it is occasioned by the pressure of the gravid uterus. In all these cases, there is, ordinarily, no room for mistaking the one disease for the other; or for regarding the hæmorrhage as idiopathic.

With respect to the *treatment* that should be adopted in cases of hæmorrhage from the stomach, it must be apparent, from what has just been said of the many different morbid conditions upon which it may depend, or with which it may be essentially connected, that remedies are, in most cases, rather to be directed against the disease of which the hæmatemesis is a symptom, than against that symptom itself. But sometimes we are obliged to treat the symptom: either because we are not certain of the exact nature of its cause; or because the condition out of which it springs is not within our reach.

Cases of *melæna* (I have told you what is meant by that term) require hard purging: and many patients recover thoroughly under that rough mode of treatment. You may prescribe five grains of calomel every night, and a black dose every morning, till the stools lose their pitchy colour. Do not be afraid of purging your patients

in such cases. If they are curable at all, that is the way to cure them. I have pursued that plan with perfect success, even with patients whom the previous hæmorrhages had blanched, and whose pulse was feeble and irregular. You may sustain them, at the same time, by a full allowance of nourishing broths. The portal system is drained and unburdened by this active depletion. And if there be no irremediable change of texture in the liver, the recurrence of the hæmorrhage may often, by a proper regulation of the habits and diet, be averted. The ancients had learned by observation the efficacy of treatment of this kind; but they used a different form of medicine, and purged away the *atra bilis* with hellebore.

It is plain that for melæna, dependent on mechanical congestion, *styptic* substances would be worse than useless. They are more adapted to those cases (could we but surely distinguish them) in which the hæmorrhage proceeds from a bleeding vessel. This is indeed the mode whereby we often succeed in stanching external hæmorrhages; namely, by applying astringents to the very part. Dr. Budd very properly lays great stress upon *prolonged fasting* in such cases. Similar means may be employed when hæmatemesis, of a purely passive character, depends upon some modification of the circulating blood. There is one remedy which is thought to have a sort of specific effect upon hæmorrhages of the gastro-intestinal canal: I mean the oil of turpentine, given in small doses; from twenty minims to half a drachm every four or six hours. I cannot say that I have had much experience of it. Of course the patient must be kept cool and quiet; whatever he drinks he should drink cold: even ice is often both grateful and effectual. If ordinary measures fail, recourse may be had to the acetate of lead; or even to the quack medicine, Ruspini's styptic. Not that I think you will often find the latter expedient successful, when more rational treatment has failed; but in obstinate and dangerous cases it ought to be tried. The Gallic acid, in solution, would however be more legitimate. If, with the hæmatemesis, there be any fever, it may be proper and necessary to abstract blood from a vein, and to employ refrigerant substances as remedies; and if, with or without much fever, there be tenderness at the epigastrium, leeches, or a blister, should be applied. In cases where the catamenia desert their natural channel, and seek an outlet through the mucous membrane of the stomach, it will be well, while means are taken to discourage the hæmatemesis, as iced drinks and so forth, to endeavour to solicit the discharge towards its right direction. And we often succeed in this object, by placing leeches upon the groins of these patients immediately before the period when the vicarious menstruation is expected; and by putting their feet at the same time into hot water; or even by laying them in a warm hip bath.

LECTURE LXX.

Dyspepsia. Physiology of digestion. Symptoms of Dyspepsia. Treatment and Prevention, Dietetic and Medicinal.

It is my intention to appropriate this evening's lecture to a cursory account of *dyspepsia*; by which I mean some evident derangement in the natural process of digesting and assimilating our food; and more especially, a faulty performance of the functions of the *stomach*. Indigestion is the prevailing malady of civilized life. We are more often consulted about the disorders that belong to eating and drinking, than perhaps about any others: and I know of no medical topic concerning which there is as much, both within and beyond the profession, so much ignorant dogmatism and quackery.

Cullen, in his definition of dyspepsia, enumerates the various symptoms, by the occurrence of more or fewer of which, that complaint is most commonly manifested. "Anorexia, nausea, vomitus, inflatio, ructus, ruminatio, cardialgia, gastrodynia:—

pauciora saltem vel plura horum simul concurrentia, plerumque cum alvo adstrictâ, et sine alio vel ventriculi ipsius, vel aliarum partium, morbo."

The variety in the actual presence and combination of these symptoms is very great: and any attempt to give a perfect or complete history of dyspepsia in these lectures is quite out of the question. But I will endeavour to draw such a general outline of the disorder as may assist and direct your observation of it hereafter.

I shall first take a brief view of the *pathology* of indigestion, so far as it is understood: and to make this intelligible, it will be necessary to interweave something of the *physiology* of the subject. To these preliminary considerations, I shall add a short comment upon the several symptoms of dyspepsia, enumerated in Cullen's definition: and lastly, I shall state what I know respecting the means of curing, and of preventing, this familiar disorder.

The conditions of healthy digestion are these: that the food should be masticated, mixed with saliva, and swallowed into the stomach: that in the stomach it should be reduced into a semi-fluid consistence and converted into a uniform pulp, called chyme: that the chyme should be transmitted through the pylorus into the duodenum, and there mixed with the bile, the pancreatic secretion, and the intestinal mucus; in consequence, as it would seem, of which admixture, the whole is separated into two parts, viz. the chyle or the nutritive portion of the food, now in a fit state to be taken up by the veins, or by the lacteals which open upon the mucous surface of the intestines, and to be carried by them into the blood; and the excrementitious portion, which at length is conveyed out of the body.

The food is dissolved and transformed, in the stomach, by the chemical agency of the *gastric juice*. This is a secretion peculiar to the stomach. All that need be stated of it here—almost all indeed that is known—is, first, that it oozes forth in minute drops from the mucous surface, but only when food (or some solid substance) is present in the stomach; secondly, that it is always *acid*; and thirdly, that under various disturbing influences and conditions, it is liable to be excessive, or else deficient, in quantity. To its acid, together with a peculiar albuminous principle which has been named *pepsin*, it appears to owe its solvent power.

The food, having arrived in the stomach, is moved about, by a sort of churning or revolving movement, and mixed with the gastric juice, and gradually changed into chyme: which also is acid. Finally, the chyme is propelled by degrees into the duodenum by the pressure of the transverse band of muscular fibres which embraces the pyloric extremity of the stomach. The time in which the whole operation is completed varies from two to four or five hours.

Liquids introduced into the stomach disappear much more speedily; either by direct absorption, or through the pylorus.

All this we know, not from mere speculation on the anatomy and usages of the organ, but from actual observation. An American physician had, for several months in succession, the singular privilege of looking, whenever he pleased, into a healthy human stomach, and of watching its condition, its movements, and its contents, during the process of digestion. A young Canadian had a portion of the skin, muscles, and ribs, of the left side of the body blown away in a gun-shot wound, which laid open the stomach also. He recovered from this frightful injury with a permanent aperture in the side, communicating directly with the stomach. Through this loop-hole Dr. Beaumont was allowed to introduce various articles of food: and to withdraw from time to time the gastric secretions; and the aliment, in the different stages of its digestion. He has published a very interesting account of these experiments, which have set at rest some points in the physiology of the stomach that were previously uncertain. I shall embody his deductions in what I have further to say on the subject.

In order that digestion may be perfect and easy, it is requisite that the food be in a state of minute division. This object is attained by *mastication*. In like manner the chemist first *tritulates* a solid, when he desires to facilitate its solution in the proper menstruum. A weak dyspeptic stomach acts slowly, or not at all, on solid lumps and tough masses of food. The delayed morsels undergo spontaneous changes, promoted by the mere warmth and moisture of the stomach: gases are extricated: acids are formed: perhaps the half-digested mass is at length expelled by vomiting; or it passes undissolved into the duodenum, and becomes a source of irritation and disturbance during the whole of its journey through the intestines. Here then we have one com-

mon cause of dyspepsia; and an easy and obvious preventive. Dyspeptic persons should not eat in a hurry, as busy men, and studious and solitary men, are apt to eat. They are to be cautioned against *bolting* their food: it must be well ground in the mill that nature has provided for that purpose. I am not at all sure that the increased longevity of modern generations is not, in some degree, attributable to the capability of chewing their food which the skill of the dentist prolongs to persons far advanced in life.

There are certain things upon which the gastric juice has no power. The green colouring matter of certain vegetables; the husks of seeds; the rinds of many fruits. You may perhaps have observed that dried currants and the pips of apples, swallowed entire, reappear, unchanged, among the egesta. Whatever passes the stomach untouched by the gastric liquor, passes undissolved through the whole of the alimentary canal; provoking disorder sometimes in its transit; forming sometimes a nucleus for intestinal concretions. These substances are therefore unfit for a weak stomach. When the digestive powers are active, and the bowels slow, they may perhaps occasionally be even useful. Thus brown bread—*i. e.* the indigestible bran, or tegument of the kernel of wheat—stimulates the peristaltic motions of the intestines, and averts, in certain persons, the necessity of more direct purgatives. Unbruised mustard-seed, once so much in vogue, owed much of whatever virtue it possessed to this principle. But if these intractable substances fail to excite the proper action of the bowels, they are apt to accumulate, and to lay the foundation of serious disease.

Indigestible matters, to which the pylorus refuses a passage, may remain in the stomach, and disturb its functions, for days, or even sometimes for weeks, together. If we could ascertain their presence, an emetic would be the remedy. And sooner or later vomiting is set up, and the offending substance is expelled. I lately saw a mass of hard curd—a small cream cheese in respect of consistence—which was thrown up after several days of severe gastric pain and disorder. The relief was immediate and complete. The patient had been taking large quantities of cream with his tea and coffee. In another person, a similar fit of indigestion terminated in the ejection of a mass of snuff. This is no unusual source of derangement of the stomach among those who use lavishly that nasty luxury.

The essential change which the chyme undergoes after leaving the stomach, appears to consist in its separation into two parts: namely, into chyle, which is taken up by the lacteals; and into excrement, which is discharged from the body. Any undissolved portions of the food become attached to this last part. We do not know exactly what is the function or agency of the pancreatic liquor; it has probably something to do with the absorption of fat: but with regard to the bile our knowledge is somewhat more definite. The acid developed in the stomach combines in the duodenum with the alkali of the bile, and is more or less neutralized. Dr. Prout conjectures that in a healthy state of the organs it is entirely neutralized. Bile is, moreover, the natural stimulus of the intestines: when its secretion is stopped, or its passage into the duodenum prevented, digestion and assimilation may go on, but the bowels are usually sluggish. This hepatic secretion has doubtless other important uses; but with these we are not at present concerned. It is pretty evident that the state of the biliary functions can have no direct influence in the production of mere indigestion. When the constituents of the bile are imperfectly eliminated from the blood, various parts of the body may suffer detriment. And when the functions of the stomach and the functions of the liver are both disordered, it *may be* that the former organ sympathizes indirectly with the morbid state of the latter: or it *may be* that one and the same cause operates in producing the derangement of both organs.

Let us now review the symptoms of dyspepsia which are mentioned in Cullen's definition. The first of these is *anorexia*: want of the natural appetite. Sometimes this is almost the only symptom observable. The patient is warned, by loss of appetite, not to take too much food; he refrains instinctively from certain kinds of food; or he feels perhaps absolute repugnance and disgust at the very thought of eating. Various have been the speculations respecting the immediate cause of hunger. It has been ascribed to the action of the gastric juice upon the surface of the empty stomach. But during health the gastric juice is never present in an otherwise empty stomach. Neither can the appetite depend upon contraction of the muscular fibres of the stomach; for the empty stomach, during health, is always contracted upon

itself. No doubt the sensation of hunger, like all other sensations, arises from some particular condition of the *nerves* of the part. It returns periodically, acknowledging in this respect the influence of habit. It is sensibly affected by agencies which operate upon and through the nervous system. The receipt of a piece of bad news will destroy, in a moment, the keenest appetite.

Sometimes there is no anorexia. The appetite may even be morbidly craving and ravenous; or capricious and uncertain.

When defect of appetite is the only symptom, it may be remedied, often, by the employment of bitters, or of the mineral acids, taken twice or thrice daily, for some time together. It would be out of place for me to speak in detail of particular medicines of this kind: it is enough if I indicate quina, columbo, gentian, quassia; the dilute sulphuric and nitric acids; or a mixture of the nitric and muriatic.

Nausea—vomitus. These are, in some instances, the most distressing results and signs of the dyspepsia. Sometimes nausea comes on soon after the food is swallowed. Sometimes there is no nausea; but after the lapse of a certain period, an hour or two generally, the food is rejected by vomiting. The matters thus thrown up are most frequently sour. Not seldom they are mixed also with bile, especially if the retching have been violent, or long continued; and then the patient is apt to ascribe the whole of his complaint to "an overflow of bile," although in fact the secretions of the liver have nothing whatever to do with it; the appearance of bile, in the fluids ejected from the stomach, proceeding from an inverted action of the duodenum. The effort of vomiting, however induced, will, if often repeated, be attended with the expulsion of yellow bile. I have more than once referred you, for an illustration of this fact, to the phenomena of sea-sickness. The fallacy I now point out has been one cause of the notion that is prevalent among patients, and the public—and not unfrequently perhaps among practitioners—that indigestion very commonly depends upon a disordered state of the biliary organs.

The vomiting which occurs in dyspepsia is often connected with a morbid irritability of the stomach; and it is sometimes a very troublesome symptom to treat. The carbonic acid has certainly a marked effect in allaying it, in many cases. We give it, as you know, in the effervescing saline draught, made with the carbonate of potash, or of soda, and lemon-juice. Sometimes the mineral acids answer better. Sometimes, on the other hand, alkalies—the liquor potassæ for example, or lime-water—are more effectual. In these latter cases we may presume that there is a morbid acidity of the stomach. A few drops of chloroform swallowed in water sometimes answer well. Small doses of opium are occasionally successful when other means fail. Opiates thrown into the rectum—opium plasters to the epigastrium—blisters to the same part: these are measures which you will sometimes have to try one after another. There are two special remedies which have been greatly extolled for their virtue in abating sickness: the hydrocyanic acid is one of them; creasote is the other. The hydrocyanic acid I have found exceedingly useful in obstinate cases. It may be given alone—or mixed with the effervescing draught—or combined with a few grains of the sesquicarbonate of soda. The creasote has disappointed me oftener than it has answered my hopes from it. Yet it has a decided influence in checking some forms of nausea; and it is the more likely to succeed, in proportion as the condition of the stomach is remote from inflammation.

But after all, the grand principle on which to treat chronic vomiting—not dependent upon disease in other parts, as the head, the kidney, or the uterus—is that laid down by Dr. William Hunter; of reducing the *quantity* of food to that amount, whatever it may be, which the stomach is able and willing to retain, and making its *quality* as bland and nutritious as possible. The most satisfactory case which I have had to treat upon this principle occurred some years ago, in the person of one of my hospital patients. She was brought out of Kent by her father. She had been under the care of several medical men, one of whom had been a pupil at the hospital, and recommended her as a proper patient for admission there. Her age was sixteen. She and her father both agreed in the same story; viz., that she constantly vomited her meals; the food generally coming up again immediately after it was swallowed, and never remaining longer in her stomach than ten minutes. The vomiting was described as being easy; and was neither preceded nor accompanied by nausea.

She had been ill for four years: ever since a severe attack of scarlet fever. At

first she vomited her meals now and then — three or four times a week — but the vomiting gradually became more and more frequent; and at the time of her admission she had vomited after *every* meal, for three months in succession. She had grown considerably in the four years; and was tolerably plump; and looked healthy; and the catamenia had begun to appear, though scantily, in the same period: but they had been altogether suspended for a year.

It was clear that a good deal of her food must have remained: and, bearing William Hunter's case in mind, I directed that she should have a very small quantity of roast meat for dinner, and a coffee cup of milk occasionally during the day; and no other food. I prescribed also some pills, consisting of aloes and soap, to act moderately on the bowels. I expected to have been obliged still further to limit her food: but she never vomited again, from that time. This distressful and protracted disorder, after long and fruitless treatment previously, yielded thus at once and easily to very simple means.

Inflatio — ructus. Flatulence, and belching. The gas that produces these symptoms is sometimes extricated from undigested food detained in the stomach, and in a state of fermentation, or of simple putrefactive change; sometimes secreted, apparently, by the stomach itself; for the flatulence comes on when the stomach is empty of food. It is apt to arise, in dyspeptic persons, if a meal happen to be delayed beyond the accustomed hour. Patients complain grievously of these symptoms, and accuse the “wind in their stomachs” as being at once the essence and the cause of all their complaints. They ask for medicines to get rid of the wind; and its escape may indeed be promoted by warm aromatics, and carminatives, as they are called; the relief thus afforded to the distended stomach being so sudden, and for the time so complete, that the sufferer ascribes to the medicine, *vim carminis*, the power of a charm. One of the most effectual and popular of these carminatives is peppermint-water. A due regulation of the periods for taking food will often suffice to obviate the flatulence that belongs to emptiness. That which follows eating may, in many cases, be prevented, by swallowing, immediately before the meal, five or six grains of the extract of rhubarb, with or without a grain of cayenne pepper: or still more certainly, according to my experience, by the nitro-muriatic acid, taken in small doses, half an hour, or an hour, before the food. When the belching is attended with the odour and flavour of rotten eggs—in other words when the gas evolved is sulphuretted hydrogen—it results from decomposition of the contents of the stomach. Flatulence from this cause may sometimes be prevented or subdued by charcoal, or by creasote, which, like the gastric juice itself, are powerful antiseptics. But this condition is commonly an accidental and transitory condition, and its best cure is an emetic. If the ascending wind brings into the throat and mouth a portion of the solid contents of the stomach, the patient is said to *ruminate*. The regurgitated matters are often intensely acid; and then an alkali may remedy the existing flatulence; a tea-spoonful of *sal volatile*, for example; or ten grains of the carbonate of soda.

Indigestion is, in many instances, attended with scarcely any *pain*; while in others the pain is very tormenting. Cullen speaks of it under the terms *cardialgia*: and *gastrodynia*. Cardialgia is that less violent and more permanent uneasiness which in popular language is called *heart-burn*. Gastrodynia is that more severe, and usually more transient pain, which is commonly denominated *spasm* or *cramp* of the stomach.

Dr. Abercrombie has some useful practical observations in respect to pain of the stomach. He speaks of it as occurring under four different forms; and I am able to bear witness to the reality of the distinctions that he has drawn. A still nicer discrimination of the varieties of stomach distress has been made by my friend Dr. Budd. In the first place, some persons suffer pain, occasionally, when the stomach is empty, even when there is no flatulence; and they are comforted and relieved by taking food. A clergyman of my acquaintance, who used to be much harassed by gastric pain of this kind recurring several times daily, and who had tried a round of drugs in vain, found by accident that it was appeased at once upon his eating a small biscuit. He therefore carries about with him always a supply of this easy remedy. It is reasonable to suppose that the pain in such cases depends upon some degree of acrimony of the fluids of the stomach itself. It often yields readily to alkalies, or to absorbent medicines. A tea-spoonful of the aromatic spirit of ammonia in a wine-glass of camphor

julep, or half a drachm of magnesia, will still the whole uneasiness sometimes in a moment, as if by magic.

Dr. Budd notes these further characteristics of this sort of pain; that it is accompanied by slowness of the pulse, and coldness of the surface of the body; that the recumbent posture helps it away; and that the hydrocyanic acid is a very successful remedy for it.

A second form of pain in the stomach is when it occurs *immediately* after taking food, and continues during the whole process of digestion, or until vomiting ensues, which gives instant ease. In such cases we have reason to suspect the existence of chronic inflammation, or of ulceration, or of some undue sensibility of the mucous membrane of the stomach. The suitable remedies are such as I spoke of in the last lecture. I might have mentioned a form of medicine which Dr. James Johnson found especially serviceable against this morbid sensibility; I mean the nitrate of silver, in small doses.

When uneasiness rather than pain occurs presently after a meal, with a sensation of weight at the pit of the stomach, and indisposition to bodily or mental exertion, we may infer that the work of digestion is slow and difficult, in consequence of a too scanty secretion of the gastric juice. For this form of dyspepsia those remedies are the most proper which are believed to promote the secretions of the stomach; ipecacuanha in small doses, or rhubarb, before the meal; or condiments taken with it, such as salt, mustard, cayenne pepper.

In the third species of painful disorder of the stomach the pain does not begin till from two to four hours after a meal, but continues for several hours. This is a very common form of complaint. Dr. Abercrombie is of opinion that the pain is seated in the duodenum, and connected with inflammatory action, or with morbid sensitiveness of the mucous lining of that bowel. He says it is frequently accompanied by pain and tenderness of the right hypochondrium; and that the liver is often blamed when it really is not in fault. The last remark I well believe; but I am not so easily persuaded that the pain is duodenal. I believe it depends upon acidity in the *primæ viæ*. It has been ascertained by several chemists, that the acid which is present in the gastric juice is the muriatic. Dr. Prout holds that the source of this muriatic acid is the common salt which exists in the blood, and that the decomposition of this salt is owing to the immediate agency of some modification of electricity; and he conceives that the principal digestive organs represent a kind of galvanic apparatus, of which the mucous membrane of the stomach and intestinal canal may be considered as the acid or positive pole, while the hepatic system is the alkaline or negative pole. However this may be, it is certain that the muriatic acid contained in the stomach is often in excess: other acids are also found there—the acetic, and more especially the lactic; and when the food, now converted into chyme, passes into the duodenum, the remaining superfluous acid teases the stomach. I think this explanation of the cause of the pain is a more probable one than Dr. Abercrombie's, because you may generally mitigate or remove the pain by introducing an alkali into the stomach, whereby the acid is neutralized; even the swallowing a cup of warm tea, by which the acid is diluted or washed away, often stops the pain. And I have, in numerous instances, succeeded in *preventing* the recurrence of this pain by directing the patient to take a small quantity of alkali, in some aromatic water, immediately after his dinner. According to Dr. Abercrombie's theory the pain ought not to be so immediately allayed by these remedies; and, since the food is gradually propelled into the duodenum as it is digested, the pain should begin, I think (supposing him right), earlier than it does. Dr. Abercrombie has found nothing of more general utility in these cases than the sulphate of iron, combined with one grain of aloes, and five grains of aromatic powder, taken three times a day. He praises lime-water also, and small opiates, and a combination of bismuth and rhubarb. Bismuth is believed to restrain undue secretion, and to exercise a sedative influence upon the stomach. Whatever may be its *modus operandi*, I am sure that it is a very effectual remedy for some kinds of gastric distress.

Cases now and then occur in which this pain, succeeding a meal, and the deposit of lithates in the urine, would seem to indicate the propriety of an alkaline treatment, but which really are more benefited by the mineral acids. In such cases the microscope detects oxalate of lime in octohedral crystals mixed with the lithate of ammonia, or of soda

Pain in the stomach occurs in a fourth form, coming on at uncertain intervals in most violent paroxysms, and properly call *gastrodynia*. It is often accompanied by a sensation of distension, much anxiety, and extreme restlessness. In females it is frequently combined with hysterical symptoms. This form Dr. Abercrombie supposes to depend upon over distension of the stomach; and it may be so; certainly great quantities of the air are sometimes extricated; and the pain is not confined to the stomach, but shoots through to the back and between the shoulders. I suspect that the pain is sometimes neuralgic. It is often very intractable: occasionally it yields to carminatives; to a few drops, for instance, of cajeput oil suspended by means of mucilage in some aromatic water. Dr. Abercrombie states that he has observed the most effectual relief in such cases to have been obtained from exciting a brisk action of the bowels by means of a strong purgative enema. He makes this practical remark, which is worth attending to. From the facility with which such affections often yield to the remedy just mentioned, it appears not improbable that the pain may be sometimes situated in the arch of the colon. Wherever its seat may be, I know that it is frequently removed by a mustard poultice laid upon the epigastrium. Opium also is of eminent use in many of these cases; and bismuth; and cordials: but I have seen more rapid and decided relief afforded by the prussic acid than by anything else; and the cure so wrought is often permanent. It does not bring ease in all cases, nor is it a medicine that is any particular favourite of mine, yet its good effect is in some instances so striking, that if this were its only virtue I should esteem the hydrocyanic acid a most valuable remedial agent.

You will meet sometimes with what is called *spasm* of the stomach (and I suppose it is such) in gouty people; who are then said to have gout in the stomach. The pain comes on in sudden and severe paroxysms; and is removable in general by laudanum and stimulants, brandy for example; or by the mustard poultice. On these cases, however, we look with jealousy and apprehension. In some instances the attack is really inflammatory, and would then be aggravated by a stimulant treatment.

There is another modification of uneasiness and disorder of the stomach, of which the distinguishing characteristic is a burning sensation in the epigastrium, followed by the vomiting, or rather the eructation of a thin watery liquid, resembling saliva, sometimes sourish, but usually insipid and tasteless, and often described by the patients themselves as being cold. This is what Cullen calls *pyrosis*, the *water-brash*. It is a disorder much more frequent in the lower ranks of society than in the upper: and among women than among men. It is very common in Scotland, and is there ascribed to the large employment of farinaceous substances as food, and especially of oatmeal. But it is said to be still more prevalent in Lapland: and it is not at all uncommon in Wales, and in various parts of England, where the diet used is chiefly vegetable. Dr. Cullen, who saw a great deal of this disease, says that its paroxysms "usually come on in the morning and forenoon, when the stomach is empty. The first symptom of it is a pain at the pit of the stomach, with a sense of constriction, as if the stomach were drawn towards the back. The pain is increased by raising the body into an erect posture, and therefore the body is bended forward. The pain is often very severe; and after continuing for some time, it brings on an eructation of a thin watery fluid in considerable quantity." Such is Cullen's description of pyrosis. He states that the complaint often occurs without other evidence of dyspepsia: but this is not consistent with the experience of subsequent observers. It is a symptom sometimes of organic disease of the stomach. In one remarkable case of pyrosis which I saw, and in which not less than three pints of this thin tasteless liquid was brought up every day, the stomach, after death, was found to all appearance healthy; but it had been pressed upon by an enormous liver. I mention these facts that you may not suppose pyrosis to be always, as Cullen has described it, a substantive and idiopathic malady.

Dr. Budd supposes that the ejected fluid, when insipid and alkaline, comes, not from the stomach itself, but from the salivary and other glands in the mouth and pharynx, and that its secretion is provoked by the uneasy sensations of the stomach. When the fluid has an acid taste, we may be pretty sure that a part of it at least is furnished by the stomach.

When pyrosis is not caused by organic disease in the stomach or in the liver, it will yield in general to opium, and especially to opium in combination with astringents.

The *pulvis kino compositus* of the Pharmacopœia is an admirable remedy for it. But we often have to contend with this difficulty, that the bowels, in cases of pyrosis, are apt to be confined, and that the opium tends to aggravate this unnatural condition; so that it becomes necessary to administer some aperient daily while the kino and opium are given: the watery extract of aloes, or the confection of senna, or the compound colocynth pill.

I scarcely need say that when the disorder has arisen under the use of innutritious or unwholesome food, the adoption of a more varied and generous diet, including a sufficient proportion of meat, is essential to the permanent success of any remedy.

One more form of stomach disorder I have still to bring before you. Like the last, it is attended with vomiting and characterized by the nature of the matters vomited. It is one of the acquisitions of modern diagnosis. In the year 1842, Mr. Goodsir, observing signs of fermentation in the fluids cast up from day to day by one of his patients, examined them in the expectation of finding some minute algæ which are known to accompany that process. To his surprise, however, a new form of vegetable life presented itself. He discovered multitudes of small flat bodies, having a rectangular outline, and a slightly oblong shape, divided into four similar portions by cross lines, and thus somewhat resembling little packets tied lengthwise and across by a string. These bodies he therefore named *sarcinæ*. Each of the four portions is similarly divided by fainter markings, in the manner of the diagram which I here show you.



Instances of the same kind of disorder were soon after noticed and recorded by Mr. Busk, Dr. Todd, Dr. Bence Jones, Dr. Wm. Jenner, and Dr. Budd, who has devoted a lecture to this interesting subject.

The fluids vomited in these cases have a faint acid smell like that of fermenting wort; and it is obvious that they are themselves fermenting. After standing a few hours, they become covered with a thick, brownish, yeast-like froth, and they deposit a brown flaky sediment. In the frothy head, and in the flaky deposit are to be seen great numbers of *sarcinæ*, together with the *torulæ* proper to yeast. The fluid itself is always acid; or if not so, it contains no *sarcinæ*. *Sarcinæ* appear in the alvine excretions also.

Most generally the vomiting is copious, and sometimes it is enormous in amount, so that the stomach must have been vastly distended. It often takes place in the morning, after a night spent in distress from a sense of burning and distension in the epigastrium, and a feeling of bubbling or fermentation there. These painful sensations are relieved by the emptying of the stomach.

Vomiting of this kind is usually, if not always, indicative of some structural and incurable disease of the stomach; such disease as hinders the onward passage of its contents through the pyloric opening: any narrowing of that orifice, whether from cancer or from any other cause. Dr. Budd believes the disease to consist primarily and essentially in some organic change which prevents the stomach from completely or readily emptying itself, and which causes a secretion from the coats of the stomach capable, when mixed with the food, of undergoing or of exciting a fermentative process; and that the development of the *sarcinæ* bears to this fermentative process, or to some stage of it, the same relation as the development of *torulæ* bears to simple alcoholic fermentation.

The evolution of carbonic acid gas, and the formation of acetic acid, attending this process, produce heartburn, flatulence, and painful distensions; symptoms which alkalies may alleviate. But the most appropriate useful remedies are those which tend to prevent the process of fermentation. Of these, common salt, creasote, and the sulphite of soda have been found the most serviceable. They are all antiseptic. Salt, and creasote in the form of a pill, may be taken with the meals. The sulphite of soda owes its efficacy to its ready decomposition by almost any vegetable acid, this decomposition setting free sulphurous acid, which has great power in preventing the fetid fermentation. The sulphite may be administered soon after the meal, or when the fermenting process is commencing, in doses varying from a quarter of a scruple to a drachm, dissolved in water. This remedy, the most effectual in relieving the disorder which is probably always incurable, was introduced into practice by Dr. William Jenner.

Costiveness is a very frequent concomitant of dyspepsia, as Cullen's definition affirms

(plerumque cum alvo adstrictâ). And this sluggish state of the bowels often aggravates, if it do not produce, the dyspepsia. At any rate the defective powers and uneasy sensations of the stomach are rectified, in many instances, by measures which promote the regular and healthy evacuation of the intestines. Without professing to go into detail in this matter, I may state that, in our remedial attempts we should imitate, as nearly as we can, the processes of nature. The mixed contents of the small intestines furnish the natural stimulus of their peristaltic movements; and the excrement excites the larger bowels. When this natural stimulus is insufficient, the want may be supplied by some substance which is involved in the food, and accompanies it in its progress — as the bran of brown bread, already mentioned, or a few grains of rhubarb, or of aloes, swallowed immediately before dinner. In adjusting the proper quantity of the drug the patient must assist the physician. It should be just so much as suffices to effect what nature neglects to do, and no more. With some persons an aperient pill acts more comfortably and opportunely if taken at bedtime. Sometimes diarrhœa is associated with indigestion. This is usually connected with an excess of acid in the *primæ viæ*. The principle of treatment in such cases is obvious. Antacid and astringent remedies are proper; chalk, bismuth, catechu, kino, rhatany, logwood.

There are innumerable sympathies of distant parts with a dyspeptic stomach, in respect of which I can do little more than barely enumerate a few. Thus indigestion is often accompanied by pain in the head, with some confusion of thought: or at all events, with a loss of mental energy and alertness. Together with a violent headache there are frequently nausea and vomiting; and the complaint is popularly known by the name of the *sick-headache*; or, in the fashionable jargon of the day, as a *bilious headache*. I must refer you to a paper of Dr. Fothergill's in the sixth volume of the *Medical Observations and Inquiries* for a very good account of this troublesome complication.

I shall hereafter have a good deal to say respecting certain morbid conditions of the urine, which take their rise sometimes from faulty digestion in the stomach, sometimes from faulty assimilation of the digested aliment in the more advanced stages of the process of nutrition. These conditions of the urine, indicating grave derangements of the whole system, furnish the characters and the names of several distinct maladies.

I told you, in a former lecture, that certain affections of the viscera of the thorax are liable to be produced by mere indigestion. Palpitations of the heart, irregularities of the pulse, fits of asthma, are no uncommon accompaniments of a disordered stomach. This is partly to be ascribed to that reflex sympathy between the parts concerned, which I have so frequently mentioned; partly to the effect of flatulence, which, by resisting the descent of the diaphragm, impedes the free working of the lungs and of the heart. I told you, at the same time, that these symptoms torment many persons with the belief that they spring from organic disease. This notion is particularly apt to infest the minds of medical students. I suppose most teachers in our profession partake of that sort of experience of which Dr. James Gregory, of Edinburgh, used to speak in his lectures. He said that scarcely a winter passed over in which several of his pupils did not apply to him on account of palpitations supposed by them to depend upon structural disease of the heart: and in no single instance were their apprehensions well founded. They were all cases of mere dyspepsia and hypochondriasis.

Dyspepsia is often connected with phthisis, with leucorrhœa, with amenorrhœa and chlorosis: and some persons imagine that these diseases are *caused* by the dyspepsia. Indigestion may lead indirectly to the development of consumption, by producing debility; but the truer view of the matter seems to be that the dyspepsia is a consequence, rather than an exciting cause, of these complaints. When, for instance, leucorrhœa is cured by topical astringents, as it often may be, the indigestion frequently is cured too.

One of the worst occasional concomitants of dyspepsia is that peculiar state of the mind to which I just now alluded under the term *hypochondriasis*. This is, in truth, a species of insanity: but it is so often connected with disorder of the digestive organs, that Cullen, whose descriptions of disease are admirably clear and true, however faulty many of his theories may be, defines hypochondriasis to be "*Dyspepsia* —

cum languore, mæstitiâ, et metu, ex causis non æquis." In the following short paragraph, he completes the picture: "In certain persons there is a state of mind distinguished by the concurrence of the following circumstances. A languor, listlessness, or want of resolution and activity with respect to all undertakings: a disposition to seriousness, sadness, and timidity: as to all future events, an apprehension of the worst or most unhappy state of them: and therefore, often upon slight grounds, an apprehension of great evil. Such persons are particularly attentive to the state of their own health, to every the smallest change of feeling in their bodies: and from any unusual feeling, perhaps of the slightest kind, they apprehend great danger, and even death itself. In respect to all these feelings and apprehensions, there is commonly the most obstinate belief and persuasion."

Now when the attention of the hypochondriac is thus morbidly fixed upon the states and sensations of his digestive organs (as it is very apt to be) the patient becomes a plague to his physicians as well as to himself.

There are a few simple rules which ought always to be kept in mind in our *treatment* of dyspepsia; although we can seldom enforce them, as they ought to be enforced, upon our patients. What patients want, in general, is some medicine that will relieve them from their discomfort and uneasy feelings, and allow them, at the same time, to go on in the indulgence of those habits which have generated the discomfort. And such remedies have not yet been discovered.

One great and indispensable principle in the treatment of indigestion, is that of restricting the *quantity* of food taken at any one time. The gastric juice is probably secreted in a tolerably uniform quantity. The muscular contractions of the stomach must needs be impaired or impeded by such distension of that organ. For both these reasons the amount of food introduced into the stomach should be kept within the limits of its capacity and powers; and these limits are transgressed if an uneasy sense of fulness is produced by the meal. The great good which the late Mr. Abernethy unquestionably did to a host of dyspeptic patients, was owing much more, I am persuaded, to the rules of diet, and the restrictions as to quantity, which he laid down, than to his eternal blue pill.

Dr. Beaumont's observations led him to the conclusion that, within certain definite limits, the supply of the gastric menstruum was exactly regulated by the demand for it. So much aliment evoked so much gastric juice. But that the amount of the latter was never greater than the measure of the requirements of the frame: and therefore that whenever the food exceeded that measure, a portion of it remained undissolved, and even disturbed the due digestion of the rest.

Again, as Dr. Abercrombie has well remarked, and as Dr. Beaumont actually saw, various articles of food are soluble in the stomach with various degrees of readiness. Therefore, when the digestion is liable easily to be impaired, it is of great importance, not only to refrain from those substances which are known to be soluble with difficulty, but also to avoid mixing together in the stomach different substances which are of different degrees of solubility. Hence there are two reasons why it is salutary to dine off one dish. 1st, Because we avoid the injurious admixture just adverted to, and 2dly, because we escape that appetite and desire to eat too large a quantity, which is provoked by new and various flavours.

And another very important principle, greatly insisted on by Mr. Abernethy, is, that the stomach should have *time* to perform one task before another is imposed upon it. He always made his patients (at least he always strongly exhorted them) to interpose not less than six hours between one meal and another. Allowing from three to five hours for the digestion of a meal, and one hour over for the stomach to rest in, Mr. Abernethy's rule seems as much founded in reason as it is justified by experience. But we preach in vain on these topics. Mr. Abernethy was in the habit of saying that no person could be persuaded to pay due attention to his digestive organs, till death, or the dread of death, was staring him in the face. I have now in my mind a family consisting of a mother and three grown-up daughters, who are continually ailing and valetudinary. They profess to have great respect for my professional advice: yet I never can induce them to think that their plan of eating is a bad one. They are not early risers. They get to breakfast about half after ten or eleven. At two they think it absolutely necessary to eat luncheon, which consists of

a mutton chop or some hashed meat, with vegetables. At six they dine: and at eight they drink tea: and then they eat no more till the next breakfast. And this is just a picture of the habits of scores of families. They huddle all their food into the stomach, at four periods, within seven or eight hours; and leave it idle for sixteen or seventeen.

Dyspeptic patients are very importunate to know *what* they may eat, and (more so still) *what* they may drink. It is of course impossible to lay down any general rules, which will suit every case. The stomach has its idiosyncrasies. I remember seeing a publication, some years ago, one section of which had this startling title, "Cases of Poisoning by a Mutton Chop." Dr. Prout knew a person who could not eat mutton in any form. He was thought to be whimsical, and mutton was frequently served up to him under some other guise, without his knowing it; but it invariably caused violent vomiting and diarrhœa. Yet, for the average of stomachs, mutton is probably the most digestible of all meats. And for the average of stomachs some useful general directions may doubtless be given.

In fevers and inflammatory disorders, experience has taught us to forbid or to limit the use of flesh meat on account of its stimulating qualities. And when the stomach itself is affected with chronic inflammation, or with morbid sensibility, a diet restricted to farinaceous substances, and milk, is sometimes attended with the happiest consequences. I suspect that a false analogy has led some into the mistake of supposing that animal food ought to be refrained from, or taken in a scanty proportion, in merely dyspeptic complaints. Animal food is easier of digestion, in the human stomach, than vegetable food. A much smaller quantity of it is needed. It is not so likely to generate acidity. It is nearer, in its composition, to the textures into which it is to be incorporated by assimilation: or rather it contains precisely the same organic and inorganic substances as the body consists of. There is less of "conversion" requisite. Indeed we may look upon the appropriation of vegetable matter by granivorous and graminivorous animals, as one stage of the process by which such vegetable matter is prepared for the sustentation of carnivorous animals: even as one great end of vegetable life seems to be that of generating or concocting matter for the nutrition of the former class, out of inorganic materials, not fitted for that purpose. A more elaborate digesting apparatus is provided for the vegetable eaters. Man, indeed, is omnivorous. But his organs of digestion are more like those of the carnivorous than of the granivorous races. And it is notorious that vegetable food, when the stomach is weak, is followed by more flatulence, that is, is digested with more tardiness and difficulty, than animal food. Nevertheless, a mixture of the two, of well-roasted or boiled flesh or fowl, with a moderate portion of thoroughly-cooked vegetables, is better suited, in my opinion, for a feeble stomach, than a rigid adhesion to either kind of aliment singly. Water; earthy and saline substances; organic substances free from nitrogen, as starch, sugar, or oil; organic substances containing nitrogen, as albumen, fibrin, or casein; these all should be present in the daily food in such proportions as will suffice to repair the daily waste of the tissues, and the loss incidental to the process of respiration. After quantity, quality must be regarded. The more soluble in water, and the less irritating the food is, the more suitable is it for the functions of the stomach. All meats that have been hardened by culinary art, or by condiments, should be avoided by him, who, as the vulgar express it, has "a bad digester;" all cured meats I mean — ham, tongue, sausages, and so forth. Mutton is thought to be more readily digestible than beef. Pork, its lean part at least, is much less so than either. All raw vegetables also must be eschewed; salads, cucumbers, pickles. But if we press our prohibitions much more strictly than this, we incur the risk of fixing the patient's attention too curiously upon his diet, and upon the sensations of his stomach; and of rendering him hypochondriacally alive to the miserable subject of his feeding.

Again, you will be continually asked whether you recommend malt liquor or wine, wine or brandy and water, white wine or port, sherry or madeira. Now it would be very easy to propound some positive rules in this matter, but it would not be so easy to vindicate them. Some allowance must be made, no doubt, for custom. I believe, however, that most dyspeptic persons would be better without any of these drinks. But it is very difficult to persuade them that the habitual use of strong liquors in small quantities can have any injurious effects. "It is not easy (says Cullen) to

engage men to break in upon established habits, or to renounce the pursuits of pleasure; and particularly to persuade men that those practices are truly hurtful, which they have often practised with seeming impunity." They are too ready to believe that it is unsafe to abandon their accustomed indulgence. A friend of mine, who visited, some years since, many of the American prisons, told me that the health of even the most inveterate spirit-drinker improves, instead of suffering, upon the sudden and total abstinence from spirits, which the regulations of those prisons enforce. There certainly are cases in which the digestion seems to be helped by a moderate quantity of wine, or beer, or spirits; yet no one can say before-hand—at least I cannot—which of them is to be preferred. Upon these points patients should interrogate their own sensations and experience, instead of seeking the oracular counsel of a physician. Drinks which are followed by evident disturbance and discomfort are manifestly unfit. And even when a favourable effect, for the time, appears to be produced, there is always a risk of ultimate detriment to the powers of the stomach from this habitual excitement.

There are states of mind, and habits of life, which, having no direct relation to the organs of digestion, yet exercise a material influence over their functions. Mental distress; mental solicitude; mental toil; over-much study; want of exercise: these are all prolific sources of dyspepsia. Sedentary habits, when their injurious effects are known, may be altered: excessive intellectual labour may be abandoned, or abridged; but it is seldom that we can minister to a wounded or an anxious spirit. Our task is hardest of all when the patient's anxiety relates to his own complaints; when he is morbidly engrossed by his bodily feelings, and despondent about his recovery. The management of the mind of a hypochondriac is peculiarly nice and difficult. It will not do to treat him as if his ailments were imaginary. He disbelieves you, contemns your judgment, and deserts you: to be fleeced perhaps by some villanous quack. You must hear what he has to say; show an interest in his case; and prescribe for him: assuring him that you understand his malady, that it is curable, and that he will be cured provided he follows your directions. If you can succeed in gaining his confidence, and in persuading him of this, the battle is half won. To tell such a person, however, not to think of his grievances would be worse than useless. The very effort to drive a subject from our thoughts fixes it there the more surely. But you must endeavour to turn his attention to other things; and to awaken in him some new interest. Prescribe *change*: change of air; change of place and scenery; change of society. Get him to *travel* in search of health; and the chances are in favour of his finding it. A tour, in fine weather, and through a pleasant country, combines almost all the ingredients which are, separately even, desirable: the withdrawal of the mind from its ordinary pursuits and cares; the diversion of the attention from one's self, by new and varied objects; exercise, carried on in the open air; a holiday from intellectual toil. Six weeks among the mountains of Switzerland, or upon the rivers of Germany, will often do more towards restoring a dyspeptic hypochondriac, than a twelvemonth's regimen and physicking at home.

With these disjointed hints, gentlemen, I must request you to be satisfied in respect to the principles upon which dyspepsia—and the hypochondriasis, which is in general so closely linked with dyspepsia—are to be managed. A full discussion of these subjects in detail would furnish matter for several lectures.

LECTURE LXXI.

Enteritis: its symptoms; causes; treatment. Mechanical occlusion of the Intestinal Tube. Colic. Colica Pictorum: its symptoms, complications, treatment, and prevention.

SOME of the diseased states of the intestinal canal, while they differ much in their essential nature, have yet many characters in common. Colic; ileus; enteritis; mechanical obstruction of the tube. It will be convenient, therefore, to consider these disorders in succession, and in some degree of connexion with each other.

In *colic* we have pain of the abdomen; pain of a twisting or wringing kind, occupying generally the umbilical region; vomiting; and costive bowels. Similar pains are apt to occur in diarrhoea; but they are transitory, and are then termed *gripings*, or more learnedly *tormina*. When they are violent, and more permanent, and, above all, when attended with constipation, they constitute colic.

You have not forgotten the symptoms of *peritonitis*. They are, briefly, pain in the abdomen, increased on pressure; and fever.

Now if, to the symptoms of colic, you add the symptoms of peritonitis, you have the symptoms of *enteritis*: by which word I desire to express the disease that is commonly called *inflammation of the bowels*. The term has lately been extended so as to signify any and every form of inflammation which any portion or tissue of the intestinal canal within the belly may suffer; but I use it in its old-fashioned sense. Cullen makes two species of enteritis. One of these consists in inflammation of the mucous membrane of the intestinal tube: he calls it enteritis *erythematica*. That is not the disorder I am about to speak of; but the other of his species, the enteritis *phlegmonodea*. I say that in colic we have abdominal pain, constipation, and vomiting. In peritonitis, the functions of the stomach and intestines are not, necessarily, affected: in enteritis they are. There is inflammation, not merely of the peritoneal coat, but of the areolar tissue uniting the several tunics, probably of the muscular tissue also, and often of the whole substance of the bowel at the inflamed part.

When the intestinal channel is any how closed up, and a bar placed to the passage of its contents, the symptoms of colic are very apt to ensue; and at length, the obstacle continuing, fatal inflammation is set up.

The term *ileus* is applied, I believe, to those cases, whether inflammatory or not, in which, by an inverted action of the intestines, their contents are carried in a retrograde course, into the stomach, and thence out of the body by vomiting.

Having made these explanations, I shall now address myself more especially to the consideration of enteritis.

It is of much practical importance to discriminate between enteritis and the disorders that resemble it; and particularly to distinguish it from colic. When it commences, as it sometimes does, with distinct rigors, and is attended by thirst, a hot skin, and a hard and frequent pulse, there is no room for doubt. But it often begins insidiously, with mere colicky symptoms; the pain, at first, is not much augmented, it may even be somewhat eased by steady pressure. If we mistake colic for enteritis, the error is of no great moment; but the opposite mistake, which is more common, may be fatal. Blood-letting, and the other remedies of enteritis, will not aggravate the mere colic; they may even, though unnecessary, relieve the patient. Some of the remedies of mere colic, are, however, highly dangerous when there is inflammation of the bowel. Physicians may fall into this error: patients, who choose to prescribe for themselves, commonly do so. They take stimulants, cordials, carminatives—the pleasantest and nearest at hand is a glass of brandy, or of gin. And in true colic these means are frequently of great service; but they exasperate the symptoms and increase the mischief when the disease is enteritis. Indeed, treatment of this kind will sometimes urge colic into enteritis. If the case be ambiguous, you must act upon the most unfavourable supposition, and treat the complaint as if you were sure that inflammation was present.

The pain of enteritis is increased by pressure. The pain of colic is not only not made worse, but it is actually mitigated often, by pressure; and it usually intermits entirely. I know that when there is simply flatulent distension of the intestines, pressure does sometimes increase the patient's uneasiness; but the uneasy sensation is very different from that acute suffering which shrinks from the touch in inflammation of their peritoneal covering. In enteritis there are also *paroxysms* of severe pain, determined, probably, by the peristaltic movements, or by the temporary distension, of the inflamed parts of the bowel; and the pain has frequently a twisting character; but there is not any thorough intermission. There is a duller abiding pain between the sharper fits. It is to be observed also, as a diagnostic circumstance, that the patient lies on his back, with his knees drawn up, as in peritonitis, and is fixed in that position, and for the very same reasons. If, in his agony, he toss about his arms, the trunk is kept motionless, and the respiration is thoracic: whereas in mere colic the mode of breathing is not altered, and the patient is apt to be turning and writhing in all postures, and out of one posture into another.

The nausea and vomiting are often most distressing. The patient not only rejects immediately whatever food, drink, or medicine he swallows, but he has fits of retching when the stomach is empty. In some instances, matters are cast up having the appearance, and something of the odour, of liquid fæces; or resembling, at any rate, the offensive fluids that are found in the small intestines after death in these cases. I may say that I have seen genuine excrement ejected, unequivocal ordure: and this may well happen if it be true that clysters, introduced into the rectum, have been voided through the mouth. Such a phenomenon would show that the whole tube was pervious; that there was no mechanical obstruction.

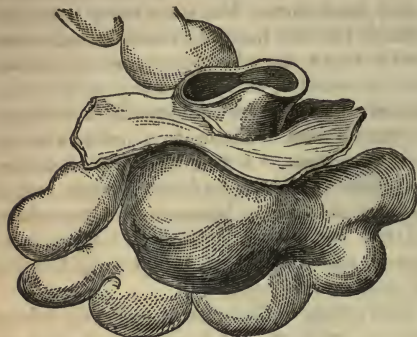
Although the fever, in the outset, may be high, and the pulse strong and hard, it soon becomes (as in all acute abdominal inflammations) small and wiry; or weak, and like a thread. In bad cases, as the disease proceeds, the abdomen begins to swell, becomes tympanitic; hiccup sometimes comes on; the pulse intermits or beats irregularly; the extremities grow cold; the features are sharpened and ghastly; cold sweats break out; the pain ceases perhaps; and the sphincters relax. The head is generally unaffected. Now and then delirium occurs late in the disease; but much more frequently the intellect remains clear to the very last. Death begins at the heart, and takes place in the way of asthenia.

You may probably have observed that the symptoms which I have been describing are just the symptoms which the surgeon so frequently encounters in cases of strangulated hernia. The symptoms of that surgical complaint are, in truth, most commonly at least, the symptoms of enteritis, caused by the forcible closure of the bowel. Obstruction to the passage of the contents of the gut gives rise to its inflammation. And we often find, after death preceded by the signs of enteritis, an internal mechanical obstruction—an internal strangulated hernia. In some cases, bands, or strings of coagulable lymph, the products of bygone inflammation, have formed snares (so to speak) for the gut, which at length they catch and constrict. They do no harm till some coil of intestine gets beneath or beyond them; and then they strangle it, as the phrase is. I have twice seen (as I think I formerly stated) the appendix vermiformis prove the immediate cause of fatal internal hernia. In one of these cases, the person had been a private patient of Dr. Macmichael's; and I went with him to examine the body. The free end of the appendix had become adherent to the mesocolon; and so a loop was made, through which a portion of the gut had passed. In the other instance, which I saw in Edinburgh, the appendix was literally tied round a piece of the intestine. Sometimes, again, there is intussusception: the upper part of the tube slips into the lower, fills it up, obstructs it; and inflammation fixes it there. Or it may be that a chronic thickening of the coats of the intestine has narrowed its channel; or a tumour presses upon the intestine from without; or some foreign substance, or morbid accumulation, plugs it up within; in all which cases a chronic disorder passes at length into acute inflammation. A very small hernia at one of the usual orifices—not large enough to manifest itself externally—is sometimes the cause of the obstruction; even though only a portion of one side of the gut may be nipped in the aperture.

Sometimes, but according to my experience less frequently, there is no mechanical impediment to account for the constipation. In all cases, whether there be mechani-

cal obstruction or not, the inflamed portion of the gut is of a red or dark colour; distended by its gaseous or liquid contents;

FIG. 109.

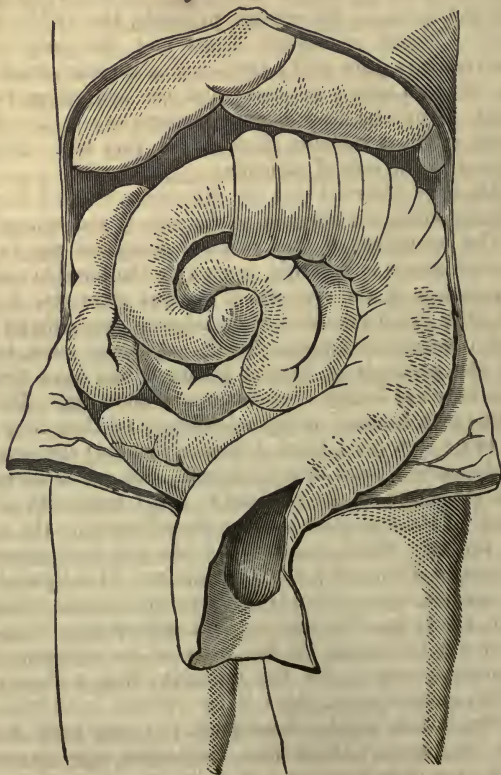


Strangulation of intestine by a portion of it slipping through an opening in the mesentery or omentum.

it is white, empty, and shrunk up into the semblance of a cord. The pathology of these last forms of the disease is full of difficulty. Some have held that the contracted

distended by its gaseous or liquid contents; covered often, on its peritoneal surface with coagulable lymph; or adherent to the contiguous organs. When the gut is mechanically closed, the part which lies on this side the obstacle, as we follow the natural course of the channel, is inflamed; the part which lies beyond it is pale, contracted, and to all appearance, healthy. The line of demarcation is abrupt and strong; and it is determined by the obstacle. And the distinction between the healthy and inflamed portions is usually as sudden and decided, when there is no apparent obstacle. I mean that up to a certain spot the intestine is red like a cherry, or dark like a grape, large, and smeared (perhaps) with lymph or with pus; while immediately beyond that point and throughout the remainder of its course,

FIG. 110.



Position of intestines in case of intussusception of cæcum and ascending colon into descending colon and sigmoid flexure; the commencement of the rectum is drawn from its position, to show the strangulated bowel within.—From *Habershon*.

part was the original seat of disease, namely, of *spasm*; and that the upper adjoining portion of intestine became distended and inflamed in consequence of such spasm. Without attempting to explain these phenomena, which are comparatively unfrequent, I content myself with observing that it is the distended part which is the really inflamed part, and that its muscular coat appears to have lost its natural contractile power—not (I fancy) from over-stretching, as now and then happens to the urinary bladder—but from the effect of the inflammation.

The inflamed portion of bowel is often of a very dark colour, and even almost black, from that great congestion of blood in its tissues. Now this black colour has sometimes been erroneously set down as evidence of gangrene. You must not trust to the mere colour, however. If the coats of the bowel are firm, and if the peculiar odour of gangrene be wanting, you are not to conclude that mortification has taken place, simply from the dark appearance of the intestine.

FIG. 111.



Appearance of intestine in a case of inflamed colon; (a) false membrane composed of granule cells; (b) surface of a portion of colon beneath false membrane; (c) follicle or crypt containing cells similar to those composing the false membrane.—From *Habershon*.

Enteritis, not dependent upon any mechanical occlusion of the bowel, may arise under the operation of the ordinary causes of internal inflammation: cold and wet, for instance, applied externally, and especially to the feet and legs. Cold is thought to be particularly injurious in this way when the exposure happens soon after a meal: probably because at that time the digestive organs, being in activity, receive a more copious supply of blood.

The mechanical impediments that occur *within* the bowel, are still more various in kind than those which constrict it from *without*. Hardened fecal matters: intestinal concretions. Some of these are curious, and I shall hereafter have a word or two to say respecting their composition. Persons who are in the habit of taking a good deal of magnesia, or of chalk, to relieve acidity and heartburn, are liable to have these substances accumulate, and become indurated in the intestines. They generally begin to collect, I believe, around some accidental nucleus: and such a nucleus may very readily be supplied. A cherry-stone, a fish-bone, a gall-stone that has found its way into the bowel. Even a pill, prescribed to cure, may thus come to kill one's patient. Dr. Prout was asked to analyze certain odd-shaped, triangular bodies which had been voided at stool, and were supposed to be gall-stones, but he found that they were specimens of Plummer's pill, which had defied the solvent action of the gastric juice, and had passed into, and lingered in, the bowels. Unbruised mustard-seed, and carbonate of iron, are other remedial substances which, injudiciously administered, have collected in hard masses, and caused intestinal disease. It is seldom that we can discover, during life, what is the exact nature of the mechanical obstacle: but I have observed this peculiarity in most of the cases in which I have known it produced by *intussusception*, that after severe and sudden symptoms of obstruction and inflammation, blood has been passed by stool. Sometimes the included portion of bowel itself sloughs away, and is expelled *per anum*.

Inflammation of the bowels requires very much the same kind of treatment as peritonitis: indeed, the disease is in most cases peritonitis, and something more. The patient must strictly abstain from every sort of stimulus, and confine himself as much as possible to the horizontal position. He must lose blood also from the arm, and the earlier the better. The lancet is not to be withheld merely because the pulse is small. If the artery become fuller, and its beating more distinct, under venæsection—nay,

if the patient do not become faint—the first bleeding should be a free one. This is not only the safer plan, but in the long run it is the most economical of the blood and strength of the patient. As in simple peritonitis, the abdomen must be covered with leeches, and afterwards with fomentation cloths. Should one spot or region be more painful and tender than another, on that part the leeches are to be accumulated. It may be requisite to repeat the venæsection and the leeching once, or twice, or oftener; the propriety of such repetition depending upon the urgency and obstinacy of the symptoms, and upon the age and state of the patient. I am speaking of idiopathic enteritis, and of its earlier stages. In the advanced periods, when feebleness of the pulse is associated with tympany of the belly and coldness of the surface, it betokens weakness and sinking, and the tendency to death by asthenia; and, consequently, it then indicates support rather than depletion. So also when the enteritis is not idiopathic, but consecutive—when we have reason to believe that it arises out of a firm mechanical impediment—our employment of blood-letting must needs be modified by that circumstance.

One point in the management of enteritis, requiring great caution and judgment, relates to the exhibition of purgative medicines. The costive state of the bowels is apt to be looked upon as the main evil, and their evacuation as the chief indication of treatment; but great mischief, I apprehend, is likely to arise from the exclusive pursuit of that indication. I am still speaking of the idiopathic disease, where it is presumable that no mechanical obstacle exists to render the passage of the *fæces* impossible. Purgatives given by the mouth are often rejected by the stomach, with great distress to the patient. If they are retained, and fail to operate, they must do more harm than good. Certainly they should not precede the venæsection. I well remember, though it is now many years ago, being myself badly treated for enteritis. Being ill, in a strange place, I sent for the nearest practitioner, who happened to be a very ignorant man. Finding that I was sick, and that my bowels did not act, he gave me, for two or three days in succession, strong drastic purges, with no other effect than that of increasing my sickness and adding to the abdominal pain I suffered. I was then seen by a most intelligent physician (this was before I had paid any attention to physis myself), and the first thing he did was to have me copiously bled; and the immediate effect of that bleeding was to send me to the night-chair. And I am persuaded that when evacuations follow the administration of purgatives in such cases, they are often owing to the blood-letting and other antiphlogistic measures that are employed at the same time. These are the opinions of Dr. Abercrombie also, who says, “I confess my own impression distinctly to be, that the use of purgatives makes no part of the treatment of the early stages of enteritis; on the contrary, that they are rather likely to be hurtful until the inflammation has been subdued.” It is an instructive fact, that when purgative medicines *do* operate during the height of the inflammation, the stools they produce are merely watery; and it is only after the inflammation has been reduced, that *fæces* are discharged, and then in such quantity sometimes as to show that they must have been shut up in the bowels during the period of active inflammation.

You should wait, then, for the effect of other measures, blood-letting, leeching, fomentation, before you give active purgatives by the mouth; and in all stages of the disease, whatever laxatives are given should be of the mildest kind, such as are least likely to excite irritation of either the stomach or bowels.

The same objections do not apply to enemata, which soothe while they evacuate the lower parts of the canal. You will read or hear of great service done by an infusion of tobacco, or by the smoke of tobacco, thrown into the bowels through the rectum. Dr. Abercrombie speaks of a weak infusion of the leaves as being a remedy of very general utility. It should, I apprehend, be a *weak* infusion to be at all safe; but, of my own knowledge, I can tell you little or nothing about it. I do know, however, that the injection of a large quantity of merely warm water is often of much use: from two to six pints, for example, thrown up gradually and gently. Indurated *fæces* are softened and brought away in this manner, and the warm water acts as an internal fomentation, and is comforting.

Of internal medicines I believe the best you can give is a combination of calomel and opium, in such proportions as will restrain the purgative qualities of the calomel. The opium allays pain, and, perhaps, relaxes spasm; mercury tends to arrest the in-

flammatory action; and the more immediate effects of this combination are often found to be, a settling of the irritable stomach, a disposition to diaphoresis, and an improved pulse.

To sum up then, bleeding, and calomel and opium, are to be resorted to for checking the inflammation: and when that is in great measure abated (and probably not before) it will be right to give some mild laxative to clear out the bowels. There can be none more suitable or convenient than castor oil. But before we venture upon purgatives by the mouth, it will be proper to wash out the bowels by lavements of tepid water. These injections *may* sometimes have mechanically a beneficial effect upon the parts concerned in the inflammation: untwisting, for example, a twisted bowel; or setting free the half-incarcerated intestine. It is often expedient to administer them through a long tube passed as far as possible into the rectum.

There is one very important point in the treatment of enteritis that I have yet to mention. I have told you that in the advanced stage of the disease symptoms of sinking often come on: a total cessation of pain, failure of the vital powers, and coldness of the body. These symptoms, this collapse and approach to syncope, are generally considered to denote gangrene, and, therefore, a hopeless state of disease. Yet, in many cases which have proved fatal after similar symptoms, not a trace of gangrene has been discoverable. So that this unpromising change in the symptoms does not always indicate a morbid condition which is necessarily mortal. And if the patients are to be saved at all, they are to be saved by wine and support. We must combat the obvious tendency to death by asthenia.

"A man, aged forty, was affected with enteritis in the usual form, for which he was treated in the most judicious manner by a respectable practitioner. On the fifth day the pain ceased; the pulse was 140, and extremely feeble and irregular: his face was pale, the features were collapsed, and his whole body was covered with cold perspiration; his bowels had been moved. In this condition (says Dr. Abercrombie, from whom I quote the case) I saw him for the first time. Wine was then given him, at first in large quantities, and upon the whole, to the extent of from two to three bottles during the next twenty-four hours. On the following day his appearance was improved; his pulse 120, and regular; the wine was continued in diminished quantity. On the third day his pulse was 112, and of good strength, and in a few days more he was well."

Dr. Abercrombie relates other cases to the same effect; and most physicians, I suppose, have occasionally seen such. They teach us that we must not abandon our patients in despair, even under the most adverse circumstances. If diarrhœa should supervene with this state of collapse, opiates must be joined with the wine. External warmth is also a powerful auxiliary.

I know of no cases of disease more painful to witness or to treat than those which result from invincible obstruction of the intestinal tube. They are usually attended, at length, with enteritis; but they differ much in some respects from what I have called idiopathic enteritis. The inflammation is an accident or consequence of the obstruction; or of the means used to overcome it. They happen usually somewhat in this manner. A person thinks it expedient to take some aperient medicine. It has no effect. He repeats the dose. It causes pain and gripings, and probably sickness also; but still the bowels are not moved. Enemata are employed. They bring away, perhaps, some hardened fœces, but there is no further relief. Meanwhile the patient may have a clean tongue, a quiet pulse, a cool and soft skin, and a supple and painless abdomen. Purgatives of a more drastic kind are tried, but tried in vain; and the physician is summoned.

Now the first thing that you have to do, when called to a stubborn case of "obstruction of the bowels," is to search narrowly whether there be not some unsuspected external hernia. All delicacy must be waived; and every part of the body, where hernia may possibly show itself, must be submitted to inspection. If none be found, the rectum, and in women, the vagina, must be severally explored. Stricture, or a quarry of impacted fœces, may possibly be detected in the one: a uterine or other tumour, pressing upon the bowel, may perchance be felt through the other. I shall never forget the shock I once experienced on being sent for to see a woman, of middle age, who was *in articulo mortis*, and who, as I was told, had for some days been

labouring under sickness, pain in the abdomen, and constipation. In her left groin there was a large palpable strangulated hernia, which had not been detected by the practitioner in attendance, simply because it had not been looked for; and which was discovered only when it was too late. Once since, I had the better fortune to retrieve a similar oversight in time. Much pain in the abdomen there had been, and frequent sickness, and no alvine evacuation for eleven or twelve days. During this time active purgatives had been given, and had been worse than useless. The matter vomited became unequivocally stercoral. Within half an hour after the hernia was detected, the strangulated gut was released by an operation; and a life was saved which I firmly believe another day's delay would have forfeited.

Remember, then, that in every case of obstinate costiveness, with signs of inflammation within the abdomen, it is absolutely necessary, for your own credit and subsequent comfort, as well as for your patient's safety, to make diligent and thorough inquiry after such herniæ as may be recognized externally.

But often you find nothing of the sort, and then you are at liberty to prosecute with more energy and decision the purgative plan of treatment. You prescribe strong doses of jalap and calomel; black draughts. The stomach being irritable, you give *pills* of cathartic extract, and repeat them at short intervals; or large doses of calomel, ten grains or a scruple, three or four times in succession. You inject stimulating clysters. Then you are driven to croton oil: and at last, in some vague hope of relaxing spasm, to opiates. If symptoms of inflammation spring up, you put fairly in force the remedies of inflammation; and especially blood-letting. But all is in vain. The medicines are vomited; or, if retained, they serve but to augment the patient's distress, producing or renewing the pain and the nausea. It is extraordinary how comfortable the patient sometimes becomes upon the intermission of these active attempts. Now and then he suffers tormina, or has fits of retching: but in the intervening periods his sensations and outward condition may be those of perfect health; only there is no alvine discharge.

Now, under these afflicting circumstances, the question will force itself upon you—how long am I to pursue the purgative system? Common sense, and common humanity, answer—you must stop it the instant you are convinced that there is a mechanical obstacle, which cannot be overcome. To persist in the use of drastic purgatives after that conviction, is to inflict wanton and needless torture upon the patient. But how are you to know this? That is one difficulty. And how are you, believing that it is so, to satisfy the patient's friends, that his disorder is irremediable; and to resist their importunity to try this and that: how persuade them to look passively on, while their relative is slowly perhaps, but surely, perishing? These are great and terrible difficulties.

You will be urged with all imaginable suggestions; even the most absurd. Crude mercury may perhaps be one. Pounds of this metal have been swallowed in such cases; in the hope, I suppose, that it would force a passage by its weight. But the obstacle may be in an ascending coil of intestine. And if not, experience does not teach us to put any faith in this rude mechanical remedy. It has often done mischief, and seldom or never done any good. The metal is apt to become oxidized in the body, and then to produce very distressing salivation. Yet in a case recorded by Mr. Adams, in which half a pound of quicksilver had been administered, two ounces and a half of the metal were voided, unchanged, five weeks afterwards. Profuse salivation is an evil which I have known to occur, and to trouble the patient greatly, some time after the ineffectual exhibition of large doses of calomel.

Dashing cold water over the abdomen and the lower extremities, is another rough expedient, which is sometimes successful in producing evacuations. It was adopted, after various other measures had failed, in the case in which the bowel was tied down by the adherent appendix vermiformis; and it caused the emptying of that part of the canal which lay beyond or below the internal hernia. It is plain that this partial success can be of little or no use; certainly of none that can compensate for the shock and annoyance of the cold affusion.

Let me not, however, be misunderstood. I do not say that you are to abandon all hope when purgatives (and, if need be, blood-letting) have failed, after a few trials to obtain evacuations from the bowels. Nor even that you are necessarily, for that reason, to give up the use of purgatives. The intestines may be torpid and insensible

to ordinary stimuli, and really require strong rousing. Dr. Abercrombie mentions the case of a gentleman whose bowels were locked up by an accumulation, as the result showed, of black hardened fæces. The obstruction, which had resisted the most powerful purgatives, and was accompanied by an evident and painful distension of a part of the abdomen, yielded at once to the repeated application of galvanism to that part: each application being immediately followed by a copious evacuation. Here the flagging muscular action was restored, apparently by the galvanic stimulus.

Two or three years ago I saw a similar case in consultation with Dr. Tyler Smith and Mr. Bryant. A gentleman of middle age presented the usual symptoms of intestinal obstruction. All the ordinary means of emptying the bowels were diligently tried, and tried in vain. Water was injected by means of a long tube passed through the rectum—with no better result. Almost in despair we at last had recourse to the electro-magnetic apparatus; and a current of the force thus generated was directed through the abdomen. You are aware that the strength of such a current is easily varied. When a low power was employed, the patient scarcely felt it. At a certain high power he suffered pain. But in a degree intermediate between these the sensations produced were agreeable. He said that he felt a “working” in his bowels; and in a short time they discharged their contents. This effect was repeated upon a repetition of the galvanic process: and our patient recovered perfectly. In this instance there was not much distension of the abdomen, and no rumbling movements of the intestines until they were excited by the electric current.

Another expedient which I have not yet mentioned is sometimes successful. This also I may as well describe to you by an instance. I was asked, one Sunday afternoon, by a physician, to see his little daughter, aged ten or eleven. On the preceding Tuesday she had gone to see the Queen open the Parliament, and was supposed to have caught cold there, for she began from that time to complain of pain in her belly. Some red discharge was reported by her nurse; and it was conjectured that this might be a show of commencing menstruation. However, she continued ill, and in pain, and was sick, and her bowels refused to act. On Saturday there was a discharge of blood and mucus. Dr. West was then consulted, who found no marks of menstruation. The discharges had been from the rectum. Purgatives, and ordinary injections, failed entirely of their purpose. It was pretty evident that the child was labouring under intussusception. As a last resource, and with a faint hope of rectifying the mischief, it was proposed to inflate the bowel with air. This was done, about midnight, by Mr. Erichsen, by means of the bellows used by the Royal Humane Society for producing artificial respiration. With my hand placed on the child's left flank, I could feel as well as hear the air enter with a rush and a noise upon each action of the bellows. Its entrance gave her some pain. She said, “There”—“that will do”—“oh don't,” &c. The inflation was continued for some minutes, yet the abdomen did not become much distended. After it was over the patient seemed easier; and in about two hours she passed a natural fæcal stool. She got well without another bad symptom.

Occasionally relief is obtained by a sort of happy accident. Some time since, I attended an elderly lady, who from Wednesday morning to the next Monday noon, had had no alvine excretion, notwithstanding the employment of the most active cathartics. She suffered frequent prooxysms of pain and vomiting: but the abdomen was scarcely, if at all, distended; nor was it tender. At length she complained that what she vomited was stercoraceous—to use her own words, “what came upwards ought to have passed the other way.” It was a thin, brown-coloured, ill-smelling fluid. Dr. Mayo and Mr. Arnott were now associated with me in the case, and they touched and felt the abdomen as I had previously done. The lady observed that their hands were heavy; and she fancied that the pressure they made had displaced something within. And I believe that it was so: for before our consultation in the next room was over, word was brought us that the bowels had acted. She had passed a liquid motion precisely resembling the stuff she had last vomited. The next day, with one of several similar stools, a hard lump was voided, which proved to be a gall-stone, as big as a small walnut.

As large quantities of warm water had without difficulty been injected, I infer that the concretion had been impacted high up in the bowel, probably at the valve of the cæcum. Some years previously the lady had suffered severe abdominal pains, which at the time were ascribed to the passage of a biliary calculus through the gall-ducts.

In this instance again there was no evidence of any strong contractile efforts of the bowel above the place of obstruction.

Large enemata—as much as the intestines will patiently receive—gradually and gently introduced, and repeated three or four times a day, may sometimes succeed in breaking down and washing away masses of hardened excrement. And if these enemata are composed of milk or beef-tea, and are suffered to remain as long as they will, they may answer another important purpose; they may contribute sensibly to the nourishment of a patient who cannot retain food in his stomach. They are generally very soothing and comfortable, allaying tormina, and abating sickness; and they are adapted to every stage and variety of the complaint.

There are, however, cases in which we arrive at the melancholy but sure conviction that some mechanical impediment has rendered the bowel absolutely and hopelessly impassable. We fear this when, the constipation being obstinate, we discover a tumour, or hardness, in some part of the belly: or when we receive a history of some former inflammatory attack, since which the bowels have been habitually difficult to regulate. Our fears are strengthened when the patient feels that the injections reach a certain spot, and there always stop; and that the intestines rumble, and roll, and propel their contents downwards to the same spot, and no further. And this is the distinctive symptom upon which I desire to fix your attention. The abdomen gradually enlarges, especially if the patient be able to retain food. The intestines fill up above the obstacle; and then throes of pain occur, *spasms* the sufferers usually call them, attended with sickness; and during these pangs you may feel, and if the abdomen be uncovered you may see, immense coils of intestine, as big perhaps as one's arm, rise and roll over, like some huge snake, with loud roarings and flatulence. When this takes place, the time for giving purgatives is certainly over. The distended bowel requires no stimulus; it acts, and strives with all its power, but strives in vain, to overcome the opposing barrier. If you would consult your patient's ease, if you would not embitter and abbreviate his slender remnant of life, harass him no further with active remedies. In some cases violent inflammation quickly supervenes, and death arrives in a few days: in others, the patient survives, without any evacuation from his bowels, but with long intervals of ease and comfort, for four, or five, or six weeks. I had the pain of attending one lady who lived forty-six days after the last *fecal* alvine discharge. Even a brief respite may be of infinite value; giving time for the arrival of distant friends—for the settlement of worldly concerns—and for preparation of the spirit against the inevitable hour.

It is to these circumstances of irremediable disease that *opiates* are eminently adapted. Even when the symptoms are less desperate they are not without their use. I cannot better express what I think upon this point than in the words of the first Heberden, who says: "The probability of advantage from anodynes has determined me to recommend them; and experience has strongly confirmed this judgment. 'Under the protection of an opiate I have successfully given more and stronger purges than would have staid without its help. The patient's strength has been kept up by some refreshing sleeps: and even in hopeless cases, in which the dying person is harassed by unspeakable inquietude, he may be lulled into some composure; and without dying at all sooner, may be well enabled to die more easily. Lord Verulam blames physicians for not making the euthanasia a part of their studies: and surely, though the recovery of the patient be the grand aim of their profession, yet where that cannot be obtained, they should try to disarm death of some of its terror: and if they cannot make him quit his prey, and the life must be lost, they may still prevail to have it taken away in the most merciful manner.'"

I have spoken of these cases as being irremediable: but perhaps I have spoken somewhat too absolutely. Life may, sometimes, be relieved by a surgical operation. The gut may be punctured above the seat of obstruction, and suffered to discharge its contents through what is called an *artificial anus*. In a very interesting paper on this subject, in the thirty-fifth volume of the *Medico-Chirurgical Transactions*, Mr. Cæsar Hawkins states that at the time when he was writing, "four persons were living and in good health in this metropolis, whose lives had doubtless been prolonged by the operation." This expedient is feasible only when the obstacle is situated in the large intestine. Here only we ascertain, with anything like certainty, the exact place of the impediment. The colon may be pierced from behind, through

either loin, without injury to the peritoneum: this is the procedure known among surgeons as Amusat's. The same gut may be opened through the peritoneum, in either flank: this is Littre's operation. I am not qualified to pronounce a judgment upon the comparative merits of the two. The circumstances of each particular case would tend, I imagine, to determine which was the most eligible. An artificial outlet in the loin would probably be less disgusting to the patient, and less offensive to others, than in front of the body. Mr. Hawkins's statistics are not very encouraging. Of forty-four persons operated on, no more than one-half survived the performance of the operation. Among those who did recover, nine only lived so much as one year, and one only had then lived more than three years after it. A respite, however, of a year, or of a few weeks, may well be deemed a most precious respite. For my own part, in an appropriate case I should think it my duty to *mention*, though I should be slow to *recommend*, this *anceps remedium*. The risk of failure, and the penalty of success, should both be set fairly before the patient. The choice between inevitable death, on the one hand, and the chance of living for a short time with a very loathsome bodily infirmity, on the other, must be made by himself.

I should express much the same thoughts concerning that more daring exploit of surgery, which proposes to lay open and explore the cavity of the abdomen in the living subject, and to disentangle or set free the intestine, strangulated somehow, within. This procedure differs materially from the former, inasmuch as it contemplates no compromise between certain death, and complete recovery. It is fearful to be called upon for counsel, in seemingly desperate emergencies, respecting these scarcely less desperate remedies. Our doubts and indecision however, as in almost all questions of conduct, are the doubts and indecision of ignorance: ignorance, in the earlier periods, when the operation might be the more hopefully attempted, whether the obstacle be really insuperable by other means; ignorance whether, if so insuperable, it may be vincible by the help of the operation; ignorance, in short, as to its exact nature and place. These are predicaments in which the patient must be admitted to the consultation: and if the peril were my own, and all other prospect of relief had failed me, I would submit myself to this forlorn hope of rescue.

I know of but two instances, though there may be more, in which this operation has been actually performed. They are recorded in the 30th and 31st volumes of the *Medico-Chirurgical Transactions*, the one by Dr. Golding Bird, and Mr. Hilton, jointly; the other by Mr. Druitt. Though the operations failed to save the lives of the patients, the narratives of these two cases plainly show how life may, peradventure, be saved under similar circumstances. In both cases the strangulated portion of intestine was found in the expected place—and was liberated. The sagacity which comprehended the nature of the mischance, the skill and courage which planned and attempted its redress, were sufficiently vindicated; but the patients ultimately sank: Mr. Hilton's from exhaustion apparently; Mr. Druitt's from rupture of an ulcerated portion of intestine not far from the place of the obstruction. Had the operations been performed earlier, they might perhaps have been successful: but consent to their performance was not obtained until the strangulation had already existed for fifteen days in the first case, for fourteen days in the second.

The cases best adapted to this bold measure—could we but discriminate them beforehand—would obviously be those of internal strangulated hernia, or of twisted bowel, occurring in persons previously and otherwise in good health.

A clue towards discovering the situation of the impediment may sometimes be obtained from the circumstances of the particular case. If large enemata find a ready passage, the obstacle can scarcely be lower than the valve of the cæcum. If urine be copiously secreted, it cannot be very high up in the small intestines. Dr. Barlow was the first to draw attention to this guiding symptom, and to explain it. If fluids, after being swallowed into the stomach, are unable to find their way into, or far into, the intestines, so as to allow of their passage by absorption or by imbibition, into the capillaries of the portal system, those fluids cannot reach the emulgent arteries, and very little urine will be secreted. The fluids swallowed will, in fact, be soon rejected by vomiting: so that early and frequent sickness is a presumptive sign that the impediment lies in the upper part of the intestinal canal. When it is situated in the large bowel, sickness is late, in general, to come on, unless it be excited by drugs, or by accumulating food. The same accomplished physician points out the lesson to be

learned from the degree of fulness of the abdomen. In the extreme case of obstruction of the duodenum, it is flat, or even sunken; in the other extreme, of the rectum, of the sigmoid flexure, it is distended and resonant; and intermediate obstacles give intermediate degrees of fulness. Sometimes the sensations of the patient guide us to the place: he feels that food and drink, or flatus, pass in the one direction, and injections in the other, as far as a certain point, but never beyond it. Occasionally, pain or tenderness in a particular spot, or a palpable tumour, may indicate the site of the obstruction.

It remains that I should say something more respecting *colic*; which may exist independently of enteritis, and of mechanical occlusion of the bowel: although the three are very often combined and intermixed in the course of the same disease.

In colic there is pain in the abdomen, constipation of the bowels, vomiting often: and these are symptoms which occur also in enteritis. The pain is a twisting or wringing pain generally, round the navel: and such is the character of the exacerbations of pain experienced when the bowels are inflamed. These are the points of *resemblance* between the two maladies: and it is of much importance, as I told you before, to observe their *differences*; and to note the marks by which the one may be distinguished from the other. It is, then, an essential difference that enteritis is attended with fever, and with tenderness of the belly. The pain is increased by all kinds and every degree of pressure; and the patient, fixed in the supine position, breathes with the intercostal muscles only, and carefully avoids any movement which would call the abdominal muscles into action, and so compress the inflamed bowels. In colic, on the other hand, the circulation is tranquil; there is no fever; and the pain is even mitigated by pressure. The patient will lie on his belly for ease; nay, he will press it with the whole weight of his body across the back of a chair, and obtain comfort by that expedient. During the paroxysms the pain is often most violent; what the old writers call *dolor atrox*—atrocious pain: but there are intervals of complete ease. Even when the pain is worst, the patient tosses and shifts from one posture to another in search of relief; and he does not wear that anxious and apprehensive aspect which we see in those who are labouring under enteritis. The pain arises, I imagine, from the distension of the bowel, here and there, by gas; or, it may be, from spasm; or from both these states at once. At any rate, it is often associated with audible flatulence, and with evident outward spasm. The abdomen is hard, and drawn spasmodically inwards towards the vertebral column; and its muscles are partially and strongly contracted, gathered up into lumps and knots. None of these circumstances belong to enteritis.

However, there is good reason for believing that, even in mere colic, the pain may sometimes be augmented by pressure. When a portion of the gut has become rapidly distended, considerable uneasiness may result from its forcible compression, although, as Dr. Abercrombie states, the kind of pain can generally, by attention, be distinguished from the sensitive tenderness of an inflamed peritoneum. Luckily, if such pain on pressure should lead us to mistake a case of pure colic for a case of enteritis, the error is on the safe side: and we must always bear in mind the tendency remarked in colic, when the complaint is neglected or badly treated, to run into actual inflammation. In fact, as any obstacle to the passage of the alimentary matters through the bowels may give rise to colic, colic is sometimes merely the first step towards acute inflammation arising out of a continuance of the obstruction. And having told you that colic may be thus produced, I have at once introduced you to a large class of its causes, which have already been spoken of in the present lecture as frequent causes of inflammation also.

But colic, like inflammation of the bowels, sometimes arises without any apparent or detectable obstruction, of a mechanical kind, to the free transit of the contents of the alimentary tube. And there is one particular form of colic that requires a separate notice. The *colica Pictorum*—so called from its great frequency, heretofore, among the Pictones, or inhabitants of Poictou—is produced by the slow introduction of the poison of lead into the body.

Now the colic which has this origin is not to be distinguished, in its ordinary symptoms, from any other kind of colic. But the abdominal pain is usually, in such cases, a part only of more general disease. It has received, in different places, a variety of names. *Colica Pictorum*; the painter's colic; the Devonshire colic; the

bellain of Derbyshire; the dry belly-ache of the West Indies. In all cases it acknowledges the same cause—the gradual entrance of lead into the system.

In this country we see the disease more often in painters than in any other persons. They use, as you know, white lead in the preparation of their colours; and they are perfectly familiar with this terrible colic. We see it also among all workmen whose occupations bring them habitually into contact with preparations of lead. No doubt there are very great differences in the susceptibility of this effect of the poison of lead. Persons have been known to suffer colica Pictonum, in consequence of their sleeping for a night or two in a recently-painted room. On the other hand, I have myself seen a patient who became affected with the disease for the first time, after working with white lead for nineteen years. Generally the first attacks of colic are well recovered from. The obstinate constipation of the bowels is at length overcome; the patient obtains ease; and forthwith recurs to his previous habits; and after a period, which varies in different individuals, he is again laid up with the colic. Even the primary attacks are usually attended with pains in the head and in the limbs; sometimes with cramps; sometimes even with epilepsy and coma. At length, in one of these attacks of colic, or after one of them, when the violence of the pain, and the costiveness, have yielded to treatment, the patient finds that he has lost the full power of using one or both of his hands. The wrists, as the patients express it, *drop*. You see at once what is the matter, by the characteristic state of the arms and hands. The extensor muscles of the hands and fingers, and the supinator muscles of the fore-arms, are palsied: so that when the arms are stretched out, the hands hang dangling down by their own weight; and the patient is unable, by any effort of his will, to raise them. The palsy is local; it does not proceed from any diseased condition of the nervous centres. The affected muscles waste; and the atrophy is very remarkably seen in the bundle of muscles composing the ball of the thumb. Even from this condition the patients often are capable of complete recovery. But if they persist in following their former calling—or if without knowing it, they continue to be habitually exposed to the exciting cause of the disease—they become miserable cripples, lose their power of sleeping, fall into a state of general cachexia, and sink at length under some visceral disease. The poison accumulates in the body, and saps the power of life. Francis Citois, a native of Poictou, who published an excellent, and one of the earliest, accounts of the disease, in the year 1617, has drawn the following graphic picture of its effects. Its cause was not at that time suspected. Speaking of the wretched sufferers, he says, “*Per vicos, veluti larvæ, aut arte progredientes statuæ, pallidi, squalidi, macilenti, conspiciuntur; manibus incurvis, et suo pondere pendulis, nec nisi arte ad os et cæteras supernas partes sublatis, et pedibus non suis sed crurum musculis, ad ridiculum ni miserandum incassum compositis, voce clangosâ et streperâ.*”

The course of the disease is usually such as I have just described it. The colic happens first, perhaps several times: and then arrives the palsy. But in a few instances I have known this order reversed. The wrists have dropped, when there had been no preceding colic.

The great cause of this fearful malady was first made out by our distinguished countryman, Sir George Baker. He set on foot an inquiry into the origin of what was called the *Devonshire colic*; so common was it in that county. He found, first, that it occurred chiefly in persons who drank the *cider* manufactured there: and, by degrees, he traced the source of the malady to the admixture of lead with the cider; either designedly, for the purpose of sweetening it; or by the inadvertent employment of lead in the construction of the cider mills and vats. It was under circumstances of the same kind that the colic of Poictou originated. Preparations of lead were used—not fraudulently, but openly and honestly—to prevent the wines of the country from turning sour; the injurious influence of lead upon the human body not having then been ascertained. So also, equally convincing proofs of the adulteration of *rum* by means of lead, giving rise to frequent attacks of dry belly-ache in the West Indies, are given by Dr. John Hunter, in the *Medical Transactions*. I invite your attention to his papers, and to Sir George Baker's on this subject. They afford a capital specimen of medical research and reasoning. Various causes, as you may suppose, had been assigned for this disorder. These are one by one investigated, and set aside; until, by this method of exclusion, the real source of all the mischief

is detected. Sir George Baker's papers contain a great deal of curious and useful information in respect to the various modes in which this poison of lead may find its way, without being suspected, into the animal economy. The subject is one of vast importance in its relation to medical police; but my limits will not allow me to follow it beyond the point where it ceases to be directly connected with the practice of physic.

Colica Pictonum is seldom fatal as colic; or during the persistence of the abdominal symptoms: yet instances enough of death occurring while the colic was present, but from other accidental causes, have now been collected, to enable us to say, that no appearances have been met with in the intestinal canal, calculated to explain the pain or the constipation. Andral relates five cases, in which the body was carefully examined after death preceded by the painter's colic. He found neither inflammation, nor any remaining trace of spasm. The intestines were neither dilated nor contracted, but of their natural texture and appearance. Mérat, who has written a good treatise on this form of colic, gives the dissections in four fatal cases: fatal, as I mentioned that they sometimes are, by the supervention of coma. There was no discoverable mark of disease; the alimentary canal was empty, and the large bowels contracted; as they were also found to be in rabbits which had died of lead colic. For animals are susceptible of the disease, and it may be produced in them by the slow impregnation of their bodies with the specific poison. Dogs, cats, and rats, that inhabit houses and manufactories wherein lead is much used or prepared, are known to be attacked both with colic and with palsy. With respect to the contraction of the large intestines in these cases, we must not be too ready to attribute it to spasm; for the bowel, when empty, is apt to be contracted.

When the palsy has been of any continuance, the affected muscles not only shrink and waste, but undergo a structural change, which is obvious to the sight. They become pale, almost white, dry. John Hunter examined the muscles of the hand and arm of a house-painter, who died, while thus paralytic, in St. George's Hospital. He found them of a cream colour and opaque; instead of being of a purplish red, and semi-transparent. And since his time, lead has been detected in the palsied muscles, and in the brain, by chemical analysis. It is doubtless conveyed by the blood, to all parts of the body. Why it fastens solely or chiefly on particular muscles, or particular nerves, nobody knows. The pain it occasions, whether in the abdomen or in the limbs, is generally thought to be neuralgic. It is one of the poisons that do not appear to find a ready exit from the body. Very recently, a most curious symptom, pathognomonic, I believe, of the presence of lead in the system, has been pointed out by Dr. Burton: and now that it has been pointed out, one can hardly understand how it escaped discovery so long. It is a blue or purplish line running along the edges of the gums just where they meet the teeth. Dr. Burton first noticed this six years ago, but wisely refrained from making his observations public until he had had time and opportunity enough to satisfy his mind that he was not mistaken. A paper of his on the subject was read at the Medical and Chirurgical Society last January (1840). I cannot resist the temptation to read to you from my notes the substance of a case which has subsequently occurred to me in the Hospital. Mary Anne Davis, a middle-aged woman, presented herself with dropped wrists. It was an exquisite example of palsy of the extensor muscles of the hands and fingers. She could raise her arms, but her hands hung down like the talons of a bird, or like the fore paws of an erect kangaroo. This began nine weeks before. She complained of pain, beginning under the nails, stretching up the backs of her hands, and reaching sometimes to the elbows. The bottoms of her feet had also been tender: and at night were burning hot.

Seeing the dropped wrists, we thought immediately of the poison of lead. But the patient was a female. We do not often meet with these effects of that poison in women; for obvious reasons. At first we could get no clue to the mode in which lead might have found its way into her system. Her husband was a broker. She had not been living in a newly-painted house: and had (she said) no concern with lead in any way. Lead often creeps in, however, through undetected channels, and I could not help suspecting it here. Mr. Pyper, my clinical assistant, soon elicited another part of her history, which added to our suspicions; namely, that before the palsy occurred, she had had pains in the abdomen, and costive bowels, for five days

together. Nay, she had a recurrence of colic after her admission. This was a strongly corroborating fact; but what clinched the proof was the discovery of a decided blue rim along the edges of nearly all her gums. This conclusive evidence led to further cross-examination; and at last it came out that some of her sons (she had seven) had occupied their leisure time in the preceding summer with making bird-cages, and painting them green, in the one room in which she habitually lived. The case was altogether a very neat one.

Mr. Tomes has taken advantage of the opportunities furnished by his office of dentist to the hospital, of inquiring into this remarkable phenomenon; and has come to the conclusion that the colour is produced by some chemical action between the tartar that forms on the teeth, where they meet the gums, and the lead which pervades the system. This woman's teeth, like those of many in her rank of life, were loaded with tartar. In one place was visible a stump level with the gum, and surrounded by a ring of tartar; and *there* was also a corresponding border of blue. In other places there were gaps, where teeth once were: here there was, of course, no tartar; and here there was no blue line on the edge of the gum. The presence of tartar is the thing necessary. The teeth are so, only as affording lodgment for the tartar. When the tartar is thoroughly removed from the neck of a tooth, the blue tinge gradually fades from the corresponding gum, while it persists around the teeth upon which the tartar is suffered to remain. Mr. Tomes extracted "an aching tooth for a lady who had taken two or three doses of acetate of lead, for the suppression of uterine hæmorrhage. Her gums exhibited the characteristic blue line. Nine days after the removal of the tooth, the gums had come together, and the union was marked by a transverse blue line. At the expiration of three weeks the blue line had wholly disappeared."

Some instances have occurred, under Mr. Tomes' observation, of strongly marked blue gum, without any other indication of the presence of lead in the system, and without any evidence of the exposure of the patient to the influence of that metal. In fact, he suspects that other metals may sometimes produce a similar discoloration of the gum. Should this hereafter be proved, the diagnostic value of this test of the operation of lead will be somewhat impaired.

"The colouring material is probably sulphuret of lead, or a similar salt of some other metal. Tartar, being very porous, admits into its substance fluids charged with animal matter, which may there be decomposed, and furnish sulphuretted hydrogen. Supposing a salt of lead to be present, a sulphuret of lead would be formed, which would give the colour in question to the tissue in which the formation took place."

"The saliva itself contains sulpho-cyanic acid, and from this source sulphur might also be furnished."

This discovery of Dr. Burton's is not a mere piece of curiosity, but is likely to be of use in various ways. In the first place, it may settle the nature and cause of many doubtful cases; as it did, indeed, of the one just narrated; and of another which I have met with since. I was sent for to Ventnor to see a lady who was thought to be dying of cerebral disease. She had arrived at that place from a distant part of the country, in a partially insensible condition; and she soon became completely comatose. A week previously she had had a fit of convulsions, followed by double vision, and ocular spectra. The coma had nearly passed away before I saw her. This lady had been ailing for some time. Two things in particular struck Dr. Martin (whom I met in consultation) and myself. She had suffered repeated attacks of abdominal pain and constipation; and the edges of her gums were blue. A faint shade of the same colour extended over the gums a little way below their edges. Further inquiry brought to light the fact that she had been living in a house where all the water used for drinking and for cookery was taken from a certain cistern; and this water, when subjected to chemical tests, was found to be impregnated with lead. Our patient recovered. Again, the poison of lead produces pains which resemble, and no doubt are sometimes mistaken for, the pains of rheumatism; it has other obscure consequences too: and an inspection of the gums may often greatly elucidate such cases. Dr. Burton found that small quantities of lead given as medicine will sometimes produce the phenomenon. In two instances it occurred within two days. One of these patients had taken fifteen grains of the acetate of lead in that time; the other twenty-four grains. A correspondent of his produced the blue rim in twenty-

four hours; viz., by four doses of five grains each, exhibited every six hours. He found too that the line remains distinct after death; even more so than during life. It may afford valuable information therefore to the medical jurist in cases of suspected poisoning with the salts of lead. It is, I say, an *early* consequence of the absorption of lead. It may teach us, as the mercurial affection of the gums teaches us, that the medicine is pervading the system; and admonish us to look out for, and guard against, colic. It may apprise workers in lead that their caution has been insufficient; that the poison has entered; and that they are in peril of belly-aches and palsy. It is a capital diagnostic sign also between colic so arising, and colic from other causes; and between colica Pictonum, and the pain of inflammation of the bowels.

In the year 1832, colic became suddenly epidemic among our troops stationed at Newera Ellia, in Ceylon. More than seven tenths of the whole force were attacked by this complaint, which in seven instances proved fatal. All the sick persons presented the characteristic blue marks. The disorder was traced by Dr. Alexander Smith, to the presence of lead in a sample of coarse sugar, distributed among the soldiers from one particular estate in the island. There was strong reason to suspect that, in the manufacture of this sugar, acetate of lead had been added to the cane-juice instead of lime. But Dr. Smith shows how the carbonate of that metal might easily get mixed, in small quantities, with the sugar, as it was ordinarily prepared there: plates of lead being laid over some parts of the apparatus employed.

Dr. Smith paid especial attention to the "blue-line" on the gums. He believes that it is produced by the gradual introduction of lead into the system, and by lead *only*. When caused by lead administered medicinally, he had seen it exist for weeks without the supervention of colic. It never appeared under the continued use of sulphate of copper, or of mercury. In all other points Dr. Smith's opinions respecting this marking of the gums coincide with those of Mr. Tomes.

In Dr. Hassall's remarkable book on Adulterations, some striking instances are given of lead poisoning, from a source which might easily be overlooked — namely, from the use of *snuff*, falsified by the admixture of the red oxide, or sometimes of the yellow chromate, of lead.

[The symptoms of colica pictonum, or lead colic, are in some respects different from those of ordinary colic. There is usually, at first, a loss of appetite, with some degree, more or less in different cases, of nausea, and very commonly an increased discharge of saliva. The patient's sleep is disturbed, or more frequently, there is an inability to sleep. According to M. Ranque, of Orleans, he exhibits various other symptoms of a disordered condition of the nervous system. The attack of colic usually commences with vomiting; the matter discharged being, at first, whatever has been taken into the stomach, mixed with bile and the gastric secretions, very generally changed from their healthy condition; if the vomiting continue, these latter are alone discharged. Pain of the abdomen is next complained of; it is most severe about the navel, the epigastrium, the hypochondria, the loins, and above the crest of the ileum. The pain, in general, commences at the hypogastric region, or, as a cramp, at the pit of the stomach, and from thence extends, in a short time, over the rest of the abdomen. The pain is at first paroxysmal, with intervals of ease; these intervals being the longest at the commencement of the attack. More commonly there occur remissions rather than intermissions of the pain; and it is remarked, that both the remissions and exacerbations are much longer than those of ordinary colic. As the disease advances the pain is almost constant, but is usually most severe at night. It is not increased by pressure; on the contrary, pressure very generally, but not invariably, affords relief, provided it be not made upon the epigastrium. According to Mérat, patients have been known to bear, with relief to the paroxysms, the weight of two or three persons standing on the belly. The abdomen is almost invariably hard and flat, from a contraction of its muscles; the navel is sometimes drawn in so as almost to approach the spine. In a few instances there exists some fulness of the abdomen, and occasionally it has an irregular, knotty feel. There generally occur, within the first day or two, sometimes within the first few hours of the attack, headache, and a dull, anxious, or depressed expression of the countenance. To these symptoms are soon added, pains of the limbs, especially of the inside of the thighs, calves of the legs, ankles, and soles of the feet; these pains are worst at night; they

are often accompanied with cramps, and according to Tanquerel, are relieved by pressure. The disease is seldom attended with febrile excitement. The skin is of a dull, dirty, cadaverous appearance; it is frequently hot, and almost invariably bedewed with irregular, clammy, and often cold, perspiration. The pulse, according to MM. Ranque and Mérat, is rarely accelerated, but often below 60° in a minute; others, however, notice a considerable acceleration of the pulse, which we have remarked in many cases, the pulse being at the same time hard, and communicating to the finger a peculiar thrill. Obstinate constipation of the bowels is an almost invariable symptom; although it does not always occur at the very commencement of the attack. In a few instances, it is said that the attack has been attended with diarrhoea; such an occurrence, however, we have never observed; on the contrary, in all the cases that have fallen under our notice, either no discharge has taken place from the bowels, or a scanty stool has occurred composed of a few lumps of dry, hardened feces, passed with much straining and pain. The urine, though generally natural in appearance, is often diminished in quantity.

From an attack of colica Pictonum, particularly a first attack, the patient may, under a judicious and timely treatment, recover perfectly; and with a careful avoidance of its exciting causes, and of the poison of lead, he may remain ever after free from a recurrence of the disease. When the disease, however, is neglected, or badly managed, or the patient is constantly or repeatedly exposed to the influence of lead, he becomes affected either with a peculiar form of partial palsy, which is well described by Dr. Watson in the text; or, instead of this gradual loss of voluntary motion in the muscles of the extremities, there may occur another set of symptoms, which very generally terminate speedily in death. The patient after a few days becomes affected with giddiness, great debility, torpor, and sometimes delirium: as the torpor advances, the pains in the abdomen and extremities abate; at length convulsions, and a comatose condition ensue, from which very few recover. Cases are recorded by Louis and others, in which death suddenly occurred during the first stage of the disease, without the occurrence of any symptoms that would cause such a result to be suspected. Drs. Elliotson and Copland each relate a case in which death was caused by perforation of the stomach. To a paralytic affection of the muscles of the extremities, as a very common effect of repeated attacks of colica Pictonum, we have already referred; and two cases are related by Dr. Alderson, of Hull, England, in which the disease was attended with paralysis of the nerves of vision; Tanquerel states that this is not uncommon in Paris, the paralysis being attended with dilated and immovable pupils; he met, also, with one instance in which deafness was present, no doubt from paralysis of the auditory nerve.—C.]

In the *treatment* of colic—and especially of the lead colic—the great indication is to get the bowels to act. If the pain of the belly be increased by pressure, if the pulse be at all accelerated, if the face be flushed, and there be the slightest approach to fever, it may be right—it can scarcely ever be wrong—to put leeches upon the abdomen, or even to take blood from the arm. This is a measure of safety as regards the possible existence of inflammation: and if there be no inflammation present or impending, it will tend to remove the spasmodic state of the muscles which goes along with, and perhaps chiefly constitutes, the disease. When there is no ground for suspecting inflammation, external warmth should be applied; diligent friction, with some stimulating liniment; or, what is much better, a mustard poultice, or turpentine stupe. My colleague, Dr. John Wilson, has been very successful in relieving these patients, by putting them into a hot bath, and having a large quantity of the water in which they are immersed thrown gradually into their bowels by means of a proper syringe. The bath presently becomes polluted, to the great solace and refreshment of the patient. It will generally be expedient to give a full dose of calomel and opium; ten grains of the one with two of the other. Sometimes the effect of the opiate is to suffer the bowels to empty themselves; showing that the previous difficulty was probably spasmodic. Usually the calomel and opium will soothe the vomiting, the restlessness, and the pain; and then a full dose of neutral salts, or of castor-oil, or (if these do not succeed) of the last named remedy, castor-oil, quickened by one or two drops of the oil of croton, will produce free evacuations from the bowels; and the patient soon returns to his ordinary state of health. It is sometimes

necessary to repeat this practice, this alternation of purgatives and anodynes: but when once the bowels have been freely moved, the disease, in general, becomes very tractable.

Although I cannot vouch the fact from my own experience, I must tell you that *alum* has been highly praised for its efficacy in saturnine colic. When given every three or four hours in full doses — from a scruple to a drachm — dissolved in some bland liquid, it is said to allay vomiting, to abate flatulence, to mitigate pain, and to open the bowels, with more certainty than any other drug; and to be often successful when other powerful drugs have failed.

I have mentioned hot baths. The late Dr. Pereira and others have recommended that these should be medicated, by dissolving the sulphuret of potassium in the water, in the proportion of two ounces to fifteen gallons, using a *wooden* vessel. This solution renders brown or black, and innocuous, any portion of lead that may be lying on the skin, or (perhaps) exuding through it. "The hands, arms, buttocks, and other parts of the bodies of painters, and workmen in white-lead manufactories, are sometimes completely blackened by the bath; but the blackness is readily removed by a brush." If it be true that the sulphuret of lead is thus repeatedly formed upon the skin in successive baths, the elimination of the poison through that channel cannot be doubted.

At La Charité, in Paris, there is what is called a specific mode of treatment followed. It is complicated and rough, but not a whit more successful than the simpler plan which is universally adopted in this country. You may see it described, if you are curious on the subject, in several of the French books. I think it is given in detail in RATIER's *Formulary of Hospital Practice*.

Though the *colic* may have been subdued, the cure of the patient is incomplete so long as any of the poison still lingers within his body; and of this there is often authentic evidence in the blue margin of his gums, and in his palsied muscles. The dropping of the wrist, which renders the patient incapable of earning his livelihood, he justly regards as a more serious evil than the temporary pain and constipation of his bowels. With a view to eradicate the lurking poison, full doses of the iodide of potassium should now be given. You may remember that when I was speaking of the mercurial tremor, I brought under your notice the experiments of M. Melsens, which seem to prove that this salt is a specific remedy for the chronic forms of both mercurial and lead poisoning. The theory of its sanative operation recommends itself by its simplicity. The poisonous substance is in intimate union with one or more of the tissues of the body, and is retained there in some form which is insoluble in the animal fluids. The iodide, carried to all parts by the blood, combines with the metallic poison, and forms a new and soluble compound, which is again taken into the blood, and discharged from the body through its natural emunctories, and especially with the urine. No good opportunity has fallen in my way of trying the iodide in these cases since I became aware of its alleged virtues. It has, however, been put to the test in this country, and its efficacy and mode of action have been fairly established. Thus Dr. Nicholson, of Redditch, relates an instance in which what he names *plumbism* was plainly marked. The patient was a house painter; he had suffered several attacks of colic; his gums showed the characteristic blue line; and the extensors of his wrists were partially paralyzed. Before he commenced the treatment, Dr. Nicholson tested this man's urine for lead, and could discover none. Then he administered the iodide of potassium; and after a while he again tried the urine, and his reagents testified that it contained lead. The medicine was continued about six weeks, by which time the patient was well, and his urine had ceased to furnish any evidence of the presence of lead. Dr. Parkes has published a precisely similar case. No better corroboration of M. Melsens' doctrines could well be had, or desired.

For helping to restore to the damaged muscles their lost power, electricity has been thought useful, applied in the way of sparks at first, and of slight shocks afterwards, along the muscular parts of the extensors of the fingers. In the electro-magnetic apparatus we have a more accessible, and probably a not less efficacious means, of furnishing the requisite stimulus. It accelerates the recovery to give the hand and fingers the mechanical support of a splint, made for that express purpose, and so contrived that they are kept extended through the greater part of the day.

[In a very interesting report on the diseases produced by lead poisoning, drawn up by Dr. Horatio Adams, of Waltham, Mass. (*See Trans. of the American Med. Assoc.* vol. v. p. 163), we have the following account of the remedies that have been found, by experience, to be the most successful in the treatment of lead palsy.

"The *sulphur bath* is frequently employed in paralysis with marked good effect. The temperature should be about milk-warm, and the patient should remain in it from three fourths of an hour to an hour. This will produce a glow and feeling of general heat. The baths will induce a copious perspiration, and a fine cutaneous eruption, attended by a general itching. This state of things will soon exhaust the patient, the baths consequently should never be continued for a longer time.

"*Electro-magnetism* is a valuable remedy when properly used. The shocks should be conveyed through the paralyzed muscles only; if passed through the entire limb, it will generally do more harm than good, exciting the sound more than the diseased muscles. One pole of the battery should be placed at the origin, the other at the insertion of the muscles about to be acted upon. It should be used daily from ten to twenty minutes, according to the ability of the patient to bear it.

"*Strychnine*.—This alkaloid is the most potent remedy we possess for the removal of lead paralysis. It, together with electricity, seems to keep the diseased muscles as it were alive or excited, and prevent that dormant state which soon results in a rapid diminution of their volume. If this artificial life in the muscles can be kept up at intervals for a few months, or till the system is cleansed from the lead introduced into it, we may reasonably expect the disease to be eventually removed.

"The best mode of administering it is in the form of pills containing one-twelfth of a grain each. The dose should be one pill, given once a day, increasing each dose by one pill, till a sufficient number be taken to produce a slight spasmodic action in the muscles. This dose (whatever it may be) should be repeated daily. In a few days, the patient will begin to move slightly the diseased muscles. The immediate effects of this remedy are generally very disagreeable to the patient, producing a tension of the muscles, and as this subsides, a feeling of exhaustion. Frequently, the patient will be very unwilling to proceed with it. Your committee believe, however, if the remedy be judiciously given, and persevered in for months, that the disease will generally yield."—C.]

It is observable of this disease, as of many, and, indeed, of most others, except certain contagious febrile diseases, that when once it has occurred, it is much more liable to occur again, upon a repetition of the exciting cause, than before. It is of very great moment, therefore, that they who are necessarily exposed to the poison of lead—as painters, plumbers, printers (who handle leaden types), colour-grinders, potters, and glass-blowers (who use the oxide of lead in their respective manufactories), shot-makers, workers in lead mines or lead manufactories, and so on—it is of great importance that these persons should be made aware of the means which are best adapted for their protection against the injurious agency of the poison: and we ought to be able to give them advice in that matter. The rules for their guidance are short and simple; and if carefully observed, I believe they will generally prove successful. They resolve themselves into cautions against the admission of the metal, or its compounds, into the body through any channel.

1. To prevent its introduction through the skin minute attention to *cleanliness* is necessary. The face and hands should be washed, the mouth rinsed, and the hair combed, several times in the day; and bathing and ablution of the whole body should be frequently performed: also, the working clothes should not be made of woollen, but of strong compact linen; and they should be washed once or twice a week at least; and they should be worn as little as possible out of the workshop: and some light impervious cap might protect the head while the workman is at his labour.

2. Care should be taken that none of the poison be admitted into the system *with the food*. The workmen, therefore, should not take their meals in the workroom, and should be scrupulous in cleansing their hands and lips before eating.

3. The entrance of the poison into the air-passages during respiration should be guarded against as much as possible. Masks have been recommended for this purpose: none, probably, would be more convenient or more effectual, than Mr. Jeffry's orinasal respirator.

There is a notion prevalent in some places, which apparently has some foundation, that the free use of fat, and of oily substances, as food, is a preservative against the colic. A physician, near Breda, informed Sir George Baker that the village in which he lived contained a great number of potters, among whom he did not witness a single case of lead colic in the course of fifteen years; and he attributed their immunity to their having lived very much on butter and bacon, and other fat kinds of food. De Haen also was told by a physician, the proprietor of a lead mine in Styria, that the labourers there were once very subject to colic and palsy; but that after they were exhorted by a quack doctor to eat a good deal of fat, especially at breakfast, they were exempt from these disorders for three years. This is a kind of prophylaxis that is very easily adopted.

More recently Liebig has asserted that "the disease called painter's colic is unknown in all manufactories of white lead in which the workmen are accustomed to take, as a preservative, *sulphuric acid lemonade*, a solution of sugar rendered acid by sulphuric acid."

If this be so, the lemonade must protect the system by converting any other salt of lead, which might find entrance, into an *insoluble* sulphate: solubility being requisite to give efficacy to any poisonous substance.

Mr. Benson, the manager of the British white-lead works in Birmingham, states (in the *Lancet*) that he has tried this method of prevention. Under his direction sulphuric acid was first added to the *treacle-beer*, used as a beverage by the workmen, in the summer of 1841. Lead colic, which had prevailed before "to a distressing extent," soon began to diminish in frequency: and from October in the same year, up to the date of Mr. Benson's communication in December, 1842—a period of fifteen months—not a single instance of the disorder had occurred amongst them. This is very encouraging.

[*Encephalopathy*, according to Tanquerel, is a distinct form of disease arising from the noxious influence of lead upon the animal economy, and would seem to depend immediately upon some lesion of the brain. It embraces all the various forms of cerebral affections that have been observed to arise from lead poisoning. The author referred to considers that it requires for its production the absorption of a larger amount of lead particles than is requisite to induce colic, arthralgia, or paralysis. He describes four distinct forms of these cerebral affections: 1, delirium; 2, coma; 3, convulsions; and 4, a combination of delirium, coma, and convulsions.

In the first of these forms of disease, *delirium* is the leading feature.* It either assumes the peculiar restless, talkative character of delirium tremens, or the more violent symptoms peculiar to acute meningitis. Two varieties have been described, viz., quiet delirium and furious delirium. In the former of these, the patient is generally quiet, his features and eyes being immovably fixed, with rather an astonished air; his countenance is sometimes composed and thoughtful; at other times his eyes are turned upwards, and his whole expression is that of ecstasy; when spoken to, his answers are at first sensible, but soon his ideas rapidly succeed each other without apparent connection. The patient is by turns either gay or sad, loquacious or silent. He will be in a constant state of restlessness, tossing his arms, and throwing off the clothes, and trying to get out of bed. Sometimes he will be affected with a light trembling, chiefly in the arms and face, and experiences a hallucination of sight and hearing which at times greatly harasses him.

In the second variety, the eyes are widely opened, menacing, furious, or haggard; the features are contracted; the patient cries, shouts, swears, tears his garments to pieces, bursts through the restraints by which he was confined, and, in short, is in a state of complete and raging insanity. The delirium is sometimes accompanied by spasmodic contractions of the muscles of the face, distortion of the eyes, chattering of the jaws, twitching of the tendons, or tremors of the limbs. Notwithstanding all this nervous disturbance, by suddenly fixing the attention of the patient on some very simple question, a rational answer will occasionally be obtained. The progress of both these varieties of delirium is very irregular, varying from instant to instant, without order; but when it is about to terminate favourably, a long, deep sleep supervenes, at the close of which, a complete change is observed, often an unexpected amelioration.

The second, or *comatose* form, is characterized by coma, more or less profound. Tanquerel distinguishes two varieties of this form, according as the coma exists simply, or as it is combined with low delirium. In the former variety the patient is immovable, his limbs are drawn up, and his eyes closed, or half closed; he utters occasionally some heavy groans, turns in his bed, opens his eyes only to close them immediately, and is not easily roused from the comatose state in which he is plunged.

In the latter variety, there is a partial rousing from the comatose state, during which the eyes are opened, and a few unintelligible words are uttered, and a degree of restlessness manifested. "These two varieties of comatose encephalopathy," says Tanquerel, "show themselves alternately or isolated, during all the course of the disease, and observe no order in their appearance or disappearance."

In the *convulsive* form of this disease, Tanquerel distinguishes five varieties: 1. Partial convulsions, in which the face, or one side, or one or more of the limbs, are agitated by repeated quick shocks; or there may be spasms of longer or shorter duration. These symptoms may coexist with delirium and somnolency, or with cephalalgia, more or less intense. 2. General convulsions. The cerebral disturbance, in this variety, may often be attributed to the violence of the colic or arthralgic pains. The convulsive shocks extend to the whole body, affecting the face and superior extremities most severely. They are distinguished from epilepsy by the consciousness being partially retained; the patients being often able to relate, in part, what they have experienced during the fit. Reason generally returns after a quiet sleep. 3. Epilepsy; the most common form in which cerebral disturbance from the deleterious effects of lead evinces itself. The paroxysm lasts from two to thirty minutes, and in no case have any traces of an *aura epileptica* been detected. When the attack is light, the patient suddenly falls, deprived of reason and general sensibility; he does not speak, but sometimes utters groans, and is but lightly convulsed. The more violent attacks are characterized by: "Immediate loss of reason, the globe of the eye being turned up, the head immovable, the face suddenly injected, and, in an instant, the florid red colour changing to the paleness of death. Some convulsive movements pass through the limbs, and the body is stiffened; soon these symptoms acquire a prodigious increase; the hand is clenched, and the thumb turned convulsively within; violent spasmodic shakes agitate the whole-body. Respiration is modified by the convulsive state of the muscles of the breast; it becomes short, interrupted, jerking, noisy, and afterwards stertorous; a foaming saliva, often bloody, is thrown out with noise and difficulty." 4. Epileptic convulsions. In this variety, the convulsive contractions are less energetic than in epilepsy; usually, there is no foaming at the mouth, or stertor; the spasms are almost continuous, and may last from one to twenty-four hours. 5. Catalepsy. The appearance of a patient, under this variety, is that of a person in a tranquil sleep; he gives no sign of sensibility or feeling when pinched or even burned, and it is impossible to awake him, or fix his attention. If his fingers, hand, arm, foot, or leg be placed in any position, constrained or easy, they are fixed for some seconds or minutes, then shake a little, and fall upon the bed. This state alternates with one in which there is some restlessness of motion and return of sensation. "After some hours, or entire days of alternations of repose and restlessness, the patient suddenly opens his eyes, feels physical wants, and asks to eat or drink."

In the fourth form of encephalopathy, there is a *combination* of all the symptoms occurring in the three other forms; it presents the type of the disease. The utmost irregularity prevails in the various combinations. Tanquerel gives the following as the most usual and most regular: "The patient is, at first, affected with delirium, sometimes so light that the physician does not recognize it; at the end of some hours, or one or two days, an attack of epilepsy supervenes, in consequence of which, the patient becomes drowsy for some minutes; then he seems to awake, and talks all day; delirium, tranquil or furious, is then more decided than before the fit of convulsions; the same day, the night following, or the next day, one or more attacks of epilepsy supervene again. After each fit, the drowsiness is more profound and long; it is interrupted only for an instant, by a half waking for some minutes, during which the patient murmurs some words, then sleeps again. If the epileptic fit is renewed often, coma becomes very profound, then death takes place. In the contrary case, the patient seems to wake suddenly from his drowsiness, after some hours, or a day."

The encephalic nervous system seems to be the seat of this disease. All the pathological phenomena belonging to it point to alterations in the encephalon. Pathological anatomy, which has shed so much light on all organic affections, has revealed nothing, however, satisfactory concerning lead encephalopathy. If there are organic changes in the brain, they are too subtle to be appreciated by our senses, even with the aid of the microscope.

In regard to the treatment of encephalopathy, Tanquerel says: "At present, the best means known, for preventing the fatal termination of encephalopathy, is the expectant method. The physician who witnesses such violent accidents of the brain, with difficulty resists the desire of prescribing some treatment, in hopes that it will do no harm, and may, perhaps, prevent a fatal termination. It is painful for him to fold his arms before the disease, but experience is the greatest of masters; the facts referred to are sufficiently conclusive to persuade every practitioner that the best mode of treatment, to oppose to one of the most formidable diseases to which man is subject—lead encephalopathy—is that pursued at the "Charity" by Rayer; that is, the expectant method, of which diet and diluted drinks form the basis." C.]

LECTURE LXXII.

Diarrhœa. Sporadic, or Summer Cholera. Epidemic Cholera.

THE morbid *fluxes* which proceed from the long tract of mucous membrane lying between the stomach and the anus are many in number: and they vary much, both in kind and in cause. *Hæmorrhages* are not uncommon. I have already described the disease called *melæna*, which is characterized by the discharge of black semifluid matters, resembling tar, from the bowels, and in most instances from the stomach also by vomiting. The matters vomited, and the matters passed by stool, are composed principally of blood, which has been rendered black, and otherwise modified in appearance, during its progress outwards in the one direction and in the other. Again, hæmorrhage from the bowels is apt to occur in *typhoid fever*; as I shall show you when we come to that disease. Hæmorrhage takes place also from the rectum in *hæmorrhoids*, or *bleeding piles*: a malady that falls chiefly to the care of the surgeon. Blood comes away, too, mixed with a greater or less quantity of mucus, in *dysentery*.

The remaining forms of profluvia from the intestinal canal I shall proceed to consider *seriatim*: at least the most important of them.

There are several very different affections classed together under the head of *diarrhœa*: by which term is usually signified the occurrence of frequent, loose, or liquid alvine evacuations. Thus diarrhœa is a very common symptom of pulmonary phthisis; and this form of the disorder has been already mentioned. It is very often met with also in typhoid fever, and during the decline of the febrile exanthemata, of which I have yet to speak. Stubborn diarrhœa attends malignant disease of the lower bowel. But diarrhœa is not unfrequently the main symptom of the illness under which the patient labours; and constitutes, at any rate, the chief object of our treatment. I shall touch briefly on some of its varieties.

In the first place there is that common form of the complaint which proceeds from over repletion of the stomach; or from the ingestion of food that is not wholesome: food that disagrees (as the phrase is) with the patient's stomach and bowels at that particular time. We may call it, with Cullen, by way of distinction, *diarrhœa crapulosa*; in which fæces are discharged in a more liquid state, and more copiously, and more often, than is natural. These cases are in truth slight cases of irritant poisoning. The ingesta irritate the mucous surface, and probably the muscular coat also; the secretions into the intestines are poured forth in unusual abundance, and

the peristaltic motions become more strong and active; the object of these changes being that of getting rid of the offending substances: a salutary and conservative effort, which we assist and imitate in our *treatment* of this form of diarrhœa.

The symptoms by which this species of diarrhœa is marked, must be well known to us all. There are often nausea, flatulence, griping pains in the bowels, succeeded by stools of unnatural appearance and odour, and of fluid or watery consistence. There are often, also, a furred tongue, and a foul breath: but the disorder is attended with little or no fever; the pulse remains of the ordinary frequency; and the temperature of the body does not rise.

There are certain things which, more than others, tend when taken into the stomach to cause this crapulous diarrhœa: and there are certain circumstances which increase the disposition to be affected by the ordinary exciting cause.

We frequently see this disorder supervene upon a debauch, in which case the *mixture* of various articles of food, and of drink, each of which in itself might have been perfectly innocent—and the actual *quantity* of the mixed ingesta—have occasioned the irritation and disturbance. But where there has been no intemperance in eating or in drinking, some kinds of food are more likely than others, *cæteris paribus*, to provoke diarrhœa. I do not speak of idiosyncrasies, which show the truth of the old proverb, that what is one man's meat is another man's poison, and which cannot be reckoned upon beforehand; but I refer to the average of systems and stomachs. And among these less-digestible and irritating substances we may place *raw vegetables* of many kinds; such as cucumbers and salads, sundry sorts of fruit, especially if they are hard, immature, and acid; plums, melons, pine-apples, nuts, and so forth. Mushrooms may be added to the list, even when they are cooked. *Putrid* food, or food which, in the more refined phraseology of gastronomers, is termed *high*, has the same effect upon some persons: and so, in a particular manner, have some kinds of *fish*; shell-fish, crabs, and muscles for instance, in this country: and in other countries, in the West Indies, there are several species of fish which are actually poisonous, and cannot be safely eaten at all. And similar disorder is frequently produced in children by any sort of food, other than the natural sustenance furnished by the mother. The new kind of nutriment disagrees with them: and the very same thing is apt to occur in adult persons. An article of diet which is perfectly wholesome and digestible, and which the stomach bears well after a little habit, will sometimes cause griping and purging, when it is taken for the first time. It is upon this principle that the diarrhœa to which Englishmen are subject upon their first visiting the towns upon the continent, is to be explained. I do not know that it is so, but I think it very likely that Frenchmen, and Germans, and Italians, suffer in the same way when they first come to this country, and adopt our habits and regimen.

Another curious exciting cause is to be found in certain *mental emotions*, and especially the depressing passions: grief, and above all, fear. A sudden panic will operate on the bowels of some persons as *surely* as a black dose, and much more *speedily*. Among the circumstances which *predispose* most persons to this kind of malady, we may particularly specify *season*—the hot weather of summer and autumn. And it is probably consistent with the experience of most of you, that the atmosphere of the dissecting-room has a similar tendency.

Now this diarrhœa, from occasional irritation, produced by the presence of substances that offend the stomach or bowels, will generally cease of itself. The purging is the natural way of getting rid of the irritant cause. We may *favour* the recovery by diluent drinks, and by making the patient abstain from all further use of food which is not perfectly easy of digestion; and we may often *accelerate* the recovery by sweeping out the alimentary canal by some safe purgative, and then soothing it by an opiate. Or we may give the aperiant and the anodyne together, and the one will not interfere with the operation of the other. A table-spoonful of castor-oil, with six or eight minims of laudanum dropped upon it: or from fifteen grains to a scruple of powdered rhubarb, with half as much of the *pulvis cretæ compositus cum opio*. By some such medication as this, emptying the bowels, and quieting them, the cure is generally accomplished with ease, and speedily: *tutò, citò, et jucundè*.

We sometimes however meet with cases in which diarrhœa *runs on*: the stools being composed of fecal matter in an unnaturally fluid state; and the precise condition on which this disposition to an over-loose state of the bowels depends, escaping

detection. If the disorder be slight, it will often yield to the astringent and bitter medicines. The infusion of *cusparia*, with the tincture of cinnamon, may supply a convenient formula. If it be more severe, or obstinate, we have recourse to chalk mixture, which neutralizes acidity; combined with catechu, or with rhatany, which are direct astringents of the tissues; and with laudanum, which calms irritation. And in extreme cases the sulphate of copper has been found to have a powerful effect in restraining the flux. It is apt to gripe, and should be combined therefore with opium. A quarter of a grain of each, in a pill, given three or four times a day, I have frequently found successful, when previous attempts to remove the diarrhœa had failed. Tannin is another substance which is often effectual in arresting chronic diarrhœa; and it is very useful in cases where opium is not well borne. Three or four grains of it may be given at intervals of four or six hours.

[A much more effectual remedy is the acetate of lead, combined with opium and ipecacuhana; one grain of the first, from a fourth to the half of a grain of the second, and from half a grain to a grain of the latter, combined in the form of a pill, or in a powder, mixed with a little simple syrup, may be given to an adult, and repeated every three or four hours according to circumstances.]

Diarrhœa in a chronic form is that which the practitioner will be most frequently called upon to treat in the adult; and it, in general, requires for its complete removal a cautious and judicious course of treatment, persevered in for a length of time. The slightest deviation from the strict diet and regimen required in each case, will often very considerably protract the cure, while a too early abandonment of the appropriate remedies will frequently be quickly followed by a return of all the worst symptoms of the disease.

In chronic diarrhœa there exists a morbid excitability of the intestinal canal, so that almost everything taken into the stomach, as food or drink, brings on quickly repeated discharges by stool consisting of the ordinary secretions of the digestive tube, more or less changed in character, mixed with portions of half digested aliment; and the looseness continues, often unattended with griping, or any other uneasy sensation save those connected with the debility and emaciation produced by the interruption to the digestive and nutritive functions generally, which the rapid passage of the aliment through the bowels occasions. There is no doubt, that frequently the morbid excitability of the digestive canal is due to a chronic inflammation, often follicular, and attended with ulceration of some portion of its mucous membrane; when this is the case, we have repeated discharges by stool, without apparently any exciting cause other than the morbid secretions of the liver, pancreas, or of the stomach and intestines themselves. The discharges are, in general, dark-coloured and offensive, very fluid, and small in quantity, and are often preceded and accompanied by griping pains more or less severe. There is, very commonly, some degree of nausea, and occasionally vomiting; the appetite for food is generally destroyed, though in many cases it continues unimpaired. The patient becomes more and more emaciated and debilitated; his skin assumes a dirty sallow hue and a dry harsh feel; the palms of his hands become hot and dry; his countenance has, in many cases, a dull, desponding expression; his features acquire considerable sharpness, and his eyes become sunken and surrounded by a broad leaden-coloured ring. The abdomen is frequently flaccid, and exhibits no tenderness upon moderate pressure; occasionally, however, it becomes swollen and tympanitic, and is now and then decidedly tender to the touch. We have known in cases of chronic diarrhœa, an effusion of serum to occur within the peritoneal cavity, and to produce a very decided intumescence of the abdomen. In protracted cases, the body exhales a peculiar sickly odour, the tongue becomes of a dark mahogany hue, and often, together with the parietes of the mouth, is covered with aphthæ. The pulse is usually small and feeble, often quick and frequent. Febrile symptoms are not generally present; in many cases, however, there is observed some degree of febrile excitement towards evening—very protracted cases we have repeatedly known to be accompanied with well-marked hectic symptoms. More or less puffiness of the face, and œdematous swelling of the extremities, very commonly occur in the course of the disease. The discharges by stool, while they are always fluid and vitiated, exhibit considerable variety in their appearance; most generally they are dark-coloured, and exhale a rancid or fetid odour; occasionally, however,

they have a jelly-like consistence, and very little smell; at other times they consist of a small quantity of a dirty yellow fluid, and when they contain solid matter this will generally be found to consist of portions of half-digested aliment. All these changes in the character of the discharges may present themselves in the same case, and often within a very short period. In protracted cases, the discharges would appear to acquire an acrid property, producing an erythematous inflammation of the verge of the anus, and often of the nates. The frequency of the stools varies very much in different cases, and at different periods in the same case. Occasionally, the diarrhœa takes place only after the ingestion of food or drinks, or of particular kinds of food, and the discharges from the bowels continue to recur at short intervals until the offending matters are got rid of. In many cases, repeated stools occur in the course of the day, whether food is taken or not, and are suspended during the night; in other instances, the evacuations from the bowels often cease for a day, or even longer, and then return, and for a short period with increased frequency.

The duration of chronic diarrhœa is very various; unless arrested by a proper course of treatment—its spontaneous cessation being a thing of very rare occurrence—it will run on for weeks, often for months, and the patient finally sinks from extreme exhaustion. Occasionally perforation of the intestines occurs from ulceration or softening, and the fatal event is preceded by peritonitis.

The causes of chronic diarrhœa are the same as those of the acute or simple form of the disease. Frequent attacks, within a short period, of an ordinary bowel complaint, will very commonly induce a chronic affection.—Improper articles of diet, and acescent drinks, habitually indulged in; exposure to a cold, and, at the same time, humid atmosphere; the abuse of purgatives, and intemperate habits generally, are among the most common causes of chronic diarrhœa. It is an affection much more readily induced in those of a lax, and feeble, excitable and broken-down constitution, than those of an opposite condition.

The state of the intestinal tube in those who have fallen victims to the disease is very various. In some cases, the mucous coat, particularly of the large intestines, is somewhat thickened, spongy, and pale—in others its anatomical characters are entirely changed, large portions of it presenting a smooth, glassy, mottled appearance, as though its surface had been covered with a thin coating of dirty varnish. Occasionally, large patches of the mucous membrane of the colon or rectum are of a dark mahogany or of a slate colour. Traces of follicular inflammation, or of ulcerations, more or less extensive, are not unfrequently met with, especially in the ileum and colon. Dr. Stokes notices a form of chronic diarrhœa as of common occurrence, dependent upon ulcers situated close to the verge of the anus; these ulcers occur chiefly in persons of a broken-down constitution, and those who have taken a great deal of mercury; we have repeatedly observed them, also, in individuals, who have been in the habit of using almost daily the various pills composed chiefly of aloes, soap, and scammony, or gamboge, of which vast quantities are vended in the United States as a popular remedy for almost every ailment. The ulcers situated just within the anus produce irritation in the colon, tenesmus, griping, frequent discharges by stool, and most commonly, during the straining, a little blood is passed. The presence of the ulcers may be at once detected by an examination of the rectum; which examination, as Dr. Stokes very correctly remarks, should invariably be made in all cases where the diarrhœa has been of long standing, and has resisted a great variety of treatment; where it is attended with tenesmus, and a desire of sitting on the night-chair after a stool has been passed; and, finally, where the patient's health does not appear to be so much affected as it naturally would be from long-continued disease of a large portion of the great intestine.

In the treatment of chronic diarrhœa our leading indications are, to control the morbid irritability or excitability of the intestinal mucous membrane, and restore it as quickly as possible to its healthy condition and functions. To effect these objects is not always, however, a very easy task; it is one which always demands considerable judgment on the part of the practitioner, and considerable patience, and an implicit obedience on the part of the patient to the medical directions and dietetic rules laid down. The first and all-important consideration is that of diet—so that, while the patient is supplied with aliment calculated for his support, as little irritation as possible of the intestines shall be excited by it. The food taken by an individual labour-

ing under chronic diarrhœa should be easy of digestion, of the mildest quality, and such as leaves, after undergoing digestion, but a small quantity of excrementitious matter; and even of such food but a small portion should be taken at a time. Rice is probably the best article of diet in the generality of cases of chronic diarrhœa; when well boiled, with the addition of a little salt, while it is sufficiently nourishing it is extremely mild and unirritating, by no means difficult of digestion, and scarcely affords any excrementitious matter to be transmitted along the intestines. It may, generally, be eaten mixed with a very moderate quantity of plain beef or mutton broth. Plain meat broths, prepared with the addition of a large amount of rice, will often furnish a very suitable food in chronic diarrhœa, and are to many stomachs more palatable; rice, also, boiled with milk and sweetened, but not too heavily, with the best of loaf sugar, or fresh milk thickened with rice flour, may be occasionally given. Should either preparation, however, be found to disagree with the patient, or to augment or keep up the diarrhœa, it should be at once relinquished. We have indeed, in numerous instances, found plain broths, when well prepared, or the juice of roasted meats, with a portion of stale bread or cracker broken into it, agree better than any preparation of rice. Tapioca, sago, or arrow-root, we have seldom found an appropriate aliment for persons labouring under the chronic form of diarrhœa. As soon as it can be borne, and this can only be ascertained by a cautious trial, a small portion of tender chicken, turkey, or mutton, plainly boiled or roasted, may be eaten with rice. Pure water, toast water, or rice water, taken cold, and in very moderate quantities at a time, should be the only drink allowed.

Next, or more properly perhaps, equal in importance to a well-regulated diet, is an attention to the clothing and regimen of the patient. Individuals affected with chronic diarrhœa, are particularly susceptible to the influence of a cold and damp atmosphere—a slight exposure to which will often increase their disease, or when we have succeeded in diminishing the frequency of the discharges, will cause them to return as before. It is essential, therefore, that independent of cautiously avoiding every species of exposure, the patient should be suitably lodged and clothed. The chamber he occupies at night, as well as during the day, should be dry, of a comfortable and equable temperature, perfectly clean, and well ventilated. His clothing should be adapted to the season and state of the weather—flannel next the skin should always be worn; a belt of flannel round the abdomen, or enveloping this part with a flannel roller nicely adjusted, and renewed daily, will always be found advantageous. In obstinate and protracted cases, the removal of the patient from a cold, damp, and changeable, to a more equable, warmer, and drier climate, whenever practicable, is a measure from which the very best results are to be anticipated; it has, in numerous instances, been known to effect a speedy cure, when all other means have failed.

In regard to exercise, even the gentlest kind, whether passive or active, cannot sometimes be taken, from the frequent and pressing calls to evacuate the bowels which occasionally are found to be excited by motion of every kind: in other cases, short walks in the open air, in suitable weather, or a gentle ride in an open carriage, or sailing in a boat, are advantageous, and should be repeated daily if the patient's strength will admit of it.

The warm bath, followed by brisk friction of the surface, is a remedy from which the best effects are to be anticipated in most cases of chronic diarrhœa; it may be repeated daily, the temperature of the water being carefully graduated by the condition of the patient's surface; when this is dry and warm, a tepid bath should be preferred, but if the surface is cool, or its heat is not well sustained, the water should be decidedly warm. The temperature of the bath should never be so low as to cause the patient when immersed in it the slightest sensation of chilliness on the one hand, nor so high on the other, nor the continuance in it so long, as to produce profuse perspiration.

By a few physicians the effects of leeches to the abdomen or to the anus, in cases of chronic diarrhœa, are spoken of in the highest terms of commendation. Drs. Crampton and Forbes, in their very excellent essay on the disease under consideration, contained in the *Cyclopædia of Practical Medicine*, (*Philadelphia Edition*, vol. i., p. 640,) speak of leeches applied to the anus, as a remedy "possessed of remarkable powers—often working," according to the common expression, "like a charm, even in cases of diarrhœa of *very long standing*, and of different external characters.

Combined with proper diet, indeed, we are persuaded," they remark, "that the greater number of diarrhœas, *both acute and chronic*, will yield to this method, with little or no aid from other medicine."—"Although, in our practice, we always," they add, "combine with the use of leeches, in the cases where they are indicated, a proper diet, yet we have had ample evidence of their unassisted powers in checking or removing diarrhœa of great severity and obstinacy."

It is certain that most of the more obstinate cases of chronic diarrhœa are kept up by a subacute inflammation of some portion of the mucous membrane of the large intestines, and in such cases a well-timed application of leeches to the anus will be productive of the best effects. In such cases, the earlier the leeches are applied the better. In detecting the cases in which leeching is indicated, will demand a good deal of tact, and close habits of observation on the part of the practitioner—his judgment must be made up from an inquiry into the history, and a careful analysis of all the circumstances of each case, for we know of no leading phenomena by which those cases where leeches are calculated to do good can be distinguished from those to which they are not adapted; as a general rule, however, we are persuaded that in well-marked cases of chronic diarrhœa, particularly when the disease has existed for any length of time, leeches are at best a doubtful, if not an improper remedy.

Blisters to the abdomen will, we apprehend, be found more generally advantageous than leeches; we have found them to produce a speedy, marked, and prompt amelioration in the prominent symptoms of the case; the frequency of the stools, under their use, being speedily diminished, and the discharges assuming a more natural appearance. In many instances, however, we confess that no benefit whatever has resulted from repeated blisters. They are a remedy, nevertheless, which so generally do good that they should not be overlooked. Keeping on the blister for a few hours, and then replacing it by a soft emollient poultice; repeating it as soon as the vesicated surface has healed, is a preferable plan to allowing the blister to remain on until complete vesication is produced, and keeping up the irritation thus produced by stimulating dressings.

The principal internal remedies from which any good effects are to be anticipated are, opiates and astringents. Opium, either by the mouth, or introduced into the rectum, in the form of an injection, acts beneficially by quieting the pain and other uneasy sensations under which the patient labours, as well as by allaying the irritability of the bowels, and thus suspending the frequency of the stools; it aids in this manner the efficacy of whatever astringent is made use of. The Dover's powder will frequently be the best form in which the opium can be administered; or, if this be found to disagree with the stomach, as will sometimes be the case, the opium may be given in the form of a pill; combining it with a small portion of ipecacuanha, we have generally found advantageous: or the opium may be given as an enema rubbed up with starch. The dose of the opium, and the period of its repetition, must be left to the judgment of the practitioner. We have not derived the same advantage in cases of chronic diarrhœa, from the salts of morphia as from the opium itself.

Of astringents, nearly the whole list, both vegetable and mineral, has been recommended, and each one is praised by different practitioners as particularly efficacious in the disease under consideration. The vegetable astringents most deserving of attention are, the catechu, kino, galls, logwood, blackberry root, and the root of the geranium maculatum. The first may be given in the form of the *infus. catechu comp.* Dr. Pemberton prefers the kino to all other astringents, in chronic diarrhœa; he gave it in doses of a scruple made in a bolus with opiate confection (on *Diseases of Abdom. Viscera*). More recently, an extensive series of experiments on the effects of kino in diarrhœa were made in the hospital *La Pitié*, in Paris, by M. Bally. In chronic diarrhœa, unaccompanied by fever or marks of inflammation in the mucous membrane, the continued use of the remedy, even for a short time, was found almost invariably to be effectual in stopping the diarrhœa. In one case of three years' standing it effected a cure. But the most remarkable result of M. Bally's experiments is the alleged fact—that the kino, given in doses of twelve or fifteen grains, for several days in succession, succeeded in curing diarrhœa attended by febrile and inflammatory symptoms. (*Crompton and Forbes, from Med. Gazette*, v. 700.) We have repeatedly employed both the catechu and kino in cases of chronic diarrhœa where astringents were evidently indicated, and although their beneficial effects were often

promptly exhibited, they have, nevertheless, repeatedly failed in diminishing the frequency of the discharges. We have found the galls, either in powder—combined with camphor or opium, and not unfrequently with both—or in decoction, a much more effective astringent in protracted and obstinate cases of diarrhœa, than either the kino or catechu. The logwood in decoction is a favourite remedy with many practitioners; we have, however, been disappointed in its effects; it may serve as a useful vehicle for more active remedies. The dewberry-root is unquestionably a very powerful astringent, and well adapted to the disease under consideration; in infusion or decoction it is extensively employed by the physicians of the United States, as is also the root of the *geranium maculatum*, the effects of which latter, in chronic discharges from the bowels, are very highly spoken of by many practitioners.

Of the mineral astringents, we know of none superior, in cases of chronic diarrhœa, to the acetate of lead; in the dose of one grain, combined with a quarter of a grain of opium and the same quantity of ipecacuanha, repeated every three hours, it will, in a large number of cases, promptly arrest the disease. The alum will, also, be found a good astringent in chronic diarrhœa; it may be administered in the form of alum whey, or in substance, combined with opium. We have given it in conjunction with powdered galls and with the best effects. The sulphate of copper, which has been employed in combination with opium by Dr. Elliotson with the most decided success, has repeatedly succeeded in our hands in arresting the disease under circumstances the most unpromising. The mode in which we have generally employed it, has been in a quarter or one-third of a grain doses combined with two grains of extract of quinia and a fourth of a grain of opium every three hours. The protocarbonate of iron, the tincture of the chloride of iron, or the ammoniated tartrate of iron, and the solution of the persesquinitrate of iron, we have repeatedly employed, and in cases of long standing have found them, especially the first three, very efficacious. They are particularly well adapted to protracted cases of the disease, attended with great prostration, and more or less infiltration of the subcutaneous cellular tissue.

The balsam copaiba and spirits of turpentine are among the remedies from which, in numerous cases of chronic diarrhœa, the very best effects may be anticipated. When the discharges from the bowels are small in quantity, and resemble in consistence thin starch or mucilage, or when they are dark-coloured and of a rank offensive odour, we are acquainted with no remedies from which the same amount of good may be anticipated. They may be combined with opiates and astringents where these are considered necessary. The spirits of turpentine we employ more frequently than the copaiba; it is, we believe, equally efficacious, while it is more easily taken by the patient, and agrees better with the stomach: it may be combined with simple syrup and water, by adding a few grains of calcined magnesia.

A variety of other remedies are recommended by different writers, the efficacy of which is highly extolled. The most prominent are *Hope's Mixture*, which is a mixture of nitrous acid, camphor water and laudanum—the *nux vomica* and its active principle; the ferrocyanuret of iron; the nitrate of silver, and the resinous extract of the *artemisia vulgaris*. Of the effects of these we have had no experience.

In those cases in which the diarrhœa appears to be kept up by ulceration seated just within the verge of the anus, very speedy and permanent relief will always be obtained by touching the ulcers with nitrate of silver.

We have given above an outline of the treatment demanded in the chronic form of diarrhœa; in conclusion, we would remark, that the management of this disease always requires the exercise of great judgment and discretion on the part of the practitioner. Nothing would appear more easy than by opiates and astringent remedies to arrest the inordinate discharges from the bowels, and thus to effect the cure of the patient, but it will be found that, in many cases, when astringents are too early resorted to, the disease, instead of being removed, is aggravated; or if by our astringents we do succeed in suspending the discharges from the bowels, a swollen and painful state of the abdomen quickly succeeds, producing greater distress to the patient, and terminating more promptly in death, than had the diarrhœa been allowed to run its course. In numerous instances, chronic diarrhœa may be very effectually cured by a proper regulation of diet and regimen, the warm bath, blisters to the abdomen, and internally the blue pill, or calomel combined with opium and ipecacuanha, and, perhaps, the use, at the same time, of moderate doses of copaiba or turpentine, without

astringents. There are, however, unquestionably, many cases, in the course of which astringents, and of the most powerful kind, are demanded in order to effect a removal of the disease, and others, in which mild astringents, early administered, will very materially shorten its duration; and it is in rightly determining the cases and period of the disease, in which astringents are indicated, that the success of the practitioner in effecting its cure will frequently depend. — C.]

I have alluded to the influence of *hot weather* in predisposing the system to be affected by the exciting causes of diarrhœa. Dr. Farr remarks that diarrhœa “is as constantly observed in English towns when the temperature rises above 60°, as bronchitis and catarrh when the temperature falls below 32°.” And there is a complaint — of which diarrhœa is one prominent symptom, but which is something more than mere diarrhœa—that shows itself in this country more or less every autumn, and prevails extensively in some years, as a minor epidemic. It is rightly enough named *cholera*; for it is attended with, and consists mainly of, a remarkable flux of *bile*. Sydenham held that the disease is limited to the month of August: and that bowel affections, with vomiting, occurring at other times, are not genuine cases of cholera. But this was one of that great man’s crotchets. The symptoms that mark this complaint are vomiting and purging of liquid matter, deeply tinged with, and principally composed of, bile; violent pains in the stomach and bowels; cramps of the legs and of the abdominal muscles; a great depression of the vital power, and a tendency to syncope or collapse.

The attack is generally sudden. At first the contents of the alimentary canal are evacuated; and then a quantity, an enormous quantity sometimes, of a turbid, yellowish, acrid fluid is expelled with violence both from the bowels, and by vomiting. The patients complain of a burning sensation in the epigastrium. As the vomiting and purging go on, clonic spasms of the lower extremities, and especially of the gastrocnemii, occur; the surface of the belly is drawn up into knots: and after a while, the patient, exhausted by the pain and the spasms, and still more by the copious discharges, grows cold and faint. Sometimes actual syncope happens: and sometimes death.

Death, however, is an uncommon event of this form of cholera, in this country.

The chief cause of cholera, such as has now been described, appears to be casual exposure to cold, after a continued high temperature of the atmosphere: and the great irritation of the stomach and bowels evinced by the symptoms, proceeds from the presence of bile in the intestines in undue quantity, and rendered more acrid than usual by some morbid alteration of its quality. The attack seems to be often determined by some of those causes of irritation which I just now mentioned when speaking of simple diarrhœa: and particularly by imprudence in eating and drinking.

I believe that no better treatment can be followed in this disease than that long ago laid down by Sydenham. He observes that any attempt to stop the purging and vomiting by strong drastic aperients, under the notion of expelling the irritant matter, would be like endeavouring to extinguish fire by pouring oil upon it; and that to try to lock up the acrid discharges in the alimentary canal by means of narcotics or astringents, would be equally hurtful. He therefore was accustomed to dilute the contents of the stomach and bowels by emollient drinks, and injections, especially by chicken broth; and so to favour their expulsion: and when any faintness or sign of sinking began to show itself, to administer *laudanum* in full doses. We are seldom summoned to these cases in the outset. Generally the vomiting and diarrhœa have continued for some hours before we see the patient; so that it is expedient to give the opiate as soon as we can. If the stomach be very irritable, solid opium in the form of pill may be preferable to laudanum; or an opiate clyster — or an opiate suppository — may be introduced into the rectum.

[In such cases we know of no remedy more effectual than a pill composed of opium, camphor and acetate of lead, in the proportion of one grain each. When the stomach is so irritable that the pill is quickly rejected, a solution of acetate of lead, eight grains to two ounces of water, with the addition of one grain of the acetate of morphia, in the dose of a teaspoonful, repeated at proper intervals, will very generally be retained. — C.]

When the skin is cold, and the pulse sinking or irregular, carbonate of ammonia, or brandy and water, may be given by the mouth: and a mustard poultice, or a bag of hot salt, or a moist and hot flannel sprinkled with oil of turpentine, should be applied to the abdomen. The cramps of the extremities may be relieved by diligent friction with the hand; or some stimulating liniment may be rubbed upon the affected muscles. When the collapse is great the patient should not be allowed to raise himself out of the horizontal posture, lest fatal syncope should follow. Opium, however, is our sheet-anchor in this complaint: it sustains the flagging powers, while it quiets the gastro-intestinal irritation.

After an attack of severe cholera, the patient is apt to be left extremely feeble; with soreness of the muscles of the trunk and limbs: and sometimes, symptoms of *inflammation* of the mucous membranes will supervene; pain and tenderness of the belly, a white tongue, thirst, and fever. And these symptoms may require some of the *remedies* of inflammation.

Such is the disease which has long been familiar to English practitioners, as *cholera*: but about the end of the first third-part of the present century, this country was visited by a severe epidemic disorder, which was also called cholera; or by way of emphasis, *the cholera*; or sometimes *spasmodic cholera*; or *Asiatic cholera*; or *malignant cholera*. The symptoms of this new disease resembled, in some points, those of the old-fashioned cholera: but differed from them in more, and in more important, particulars. So that the application of the term cholera, or cholera morbus, to both these morbid conditions, is very much to be regretted, for it has produced a great deal of confusion and inconvenience.

I scarcely know how to name the newer and severer disorder. I have no right to alter the received nomenclature; and choosing from among the many appellations which have been given to the complaint, that epithet which seems the least objectionable, I may call it *epidemic cholera*: although this term is not unobjectionable, since the other malady, to which Dr. Farr has given the appropriate name of *summer cholera*, is sometimes also epidemic.

The *epidemic cholera* so far resembled the *summer cholera*, that it was attended by profuse vomiting and purging, by extreme prostration of strength, and by cramps. But it differed remarkably in *these* respects; in the circumstance that the matters ejected from the stomach and bowels contained no bile (and this alone is a good reason against calling the disease *cholera*); in the early supervention of the symptoms of collapse; and in the great mortality of the disorder.

The amount of the fluid matters thrown up from the stomach and discharged by the bowels, was really in many cases wonderful. At first, perhaps, the patient would have so copious a stool—a consistent dejection it might be, but so large in quantity—as to lead him to conclude that the whole contents of the intestines had been evacuated at once. Yet soon afterwards a turbid whitish liquid would again and again pour from his bowels in streams, and be spouted from his mouth as if from a pump: not in general with pain or much effort, but easily and abundantly. The matters thus discharged were thin, and for the most part of a whitish colour, like water in which rice has been boiled; without faecal smell; and containing small white albuminous flakes. There were some varieties in the evacuations, but the kind I have mentioned, resembling rice-water, was the most common and the most characteristic: and however *else* their sensible qualities might vary, *this* circumstance was universal, that they contained no *bile*.

With all this there was early sinking, and collapse, as it was called. This term *collapse* expressed a general condition, made up, in the most exquisite cases, of the following particulars:—A remarkable change took place in the circulation, and a striking alteration in the appearance of the patient. The pulse became frequent, *very* small and feeble, and at last, even for hours sometimes, extinct at the wrists. The surface grew cold; and in most, or in many instances, blue as well as cold. The lips were purple; the tongue was of the colour of lead, and sensibly and unpleasantly cold to the touch, like a frog's belly; and the breath could be felt to be cold. With this coldness and blueness there was a manifest shrinking and diminution of the bulk of the body. The eyes appeared sunk deep in their sockets; the cheeks fallen: in short, the countenance became as withered and ghastly as that of a corpse. The cada-

verous aspect that sometimes precedes death in long-standing diseases, would come on in the course of an hour or two, in this complaint. If the physician left his patient for half an hour, he found him visibly thinner on his return. The finger nails became blue; the hands and fingers shrivelled, white, corrugated, and sodden, like those of a washerwoman's after a long day's work. The skin was bathed in a cold sweat. The voice became husky and faint. So peculiar was this change, that the sound was spoken of as the *vox cholericæ*. These are the symptoms which the single word *collapse* was meant to express.

Another very striking feature of the disorder was the muscular cramp; affecting the muscles of the thighs and calves of the legs, rendering them as hard and rigid as wood; and drawing up into knots the muscles of the abdomen. These spasmodic contractions were attended with severe pain, and constituted the greater part of the patient's suffering. During the continuance of the symptoms that I have been endeavouring to describe, not a drop of urine was passed or secreted. One man who was under my own observation and care, and who recovered, did not void a drop of water from Sunday morning till the afternoon of the following Wednesday.

Even in the extreme state of collapse the intellect remained quite clear: the patients would continue to talk rationally to the last moment of their lives; and, for the most part, they seemed singularly indifferent and apathetic about their condition.

In the fatal cases—and a very fearful proportion of the whole number *were* fatal—death took place sometimes in the course of two or three hours; and it was seldom delayed beyond twelve or fifteen. In those that recovered, the favourable symptoms were the cessation of the vomiting, purging, and cramps; the return of the pulse, of the voice, and of warmth to the surface; the disappearance of the blueness of the skin, and of the hippocratic countenance; the reappearance of bile in the alvine evacuations; and the restoration of the secretion of urine.

The course of the symptoms varied a good deal in different persons. Sometimes the vomiting and purging soon ceased, and sometimes there was neither sickness nor diarrhoea at all, but rapid collapse and sinking. These were thought the most formidable cases. However, the peculiar secretions were *poured forth*, in some, at least, of the instances in which none of them were *ejected from the body*. A patient died of cholera in the Middlesex Hospital without any vomiting or purging: but on examining the dead body, we found the intestines quite full of the rice-water serous fluid. Sometimes the cramps were not very troublesome. The cutaneous blueness was not a universal phenomenon. The patients were in general tormented by thirst: and when attempts were made to bleed them, the blood was found dark and thick, like treacle, and scarcely moving, if moving at all, in the veins: in some cases it could not be made to flow out. Considerable hurry and anxiety of the breathing were also symptoms that I omitted to mention before.

Examination of the dead bodies threw no light, that I know of, upon the nature of this frightful disease. The alimentary canal generally was found to contain a white liquid, having whiter flakes in it; such as had previously issued from the bowels: and the mucous glands of the intestines, both the solitary and the agminated, were unusually large and conspicuous. The veins were loaded with thick, black, tar-like blood; and the urinary bladder was always found empty, and contracted into the size of a walnut. Even when the blue colour had existed in a marked degree during life, it often quickly disappeared after death. And another most singular phenomenon was occasionally remarked in the dead body. A quarter, or half an hour, or even longer, after the breathing had ceased, and all other signs of animation had departed, slight, tremulous, spasmodic twitchings and quiverings, and vermicular motions of the muscles would take place; and even distinct movements of the limbs, in consequence of these spasms.

The disease, of which I have drawn but a faint outline, was not known in this country till the autumn of the year 1831. There are persons, I am aware, who hold that it has always existed among us; only not in such numerous instances as at that period; and they appeal to Morton, and other early writers on the diseases of this country, in support of their opinion. But the malady was too striking to be overlooked, or ever forgotten, by any one who had once seen it. Certainly, till that year, I never saw anything like it. To be sure I had not at that time been very many years in practice here. The late Dr. Babington, however, told me that it was quite

new to *him*. He had, for a very long period, been in extensive business, in those parts of the metropolis and its vicinity where the epidemic cholera raged most; and when it first came among us he had the curiosity to ask every medical man whom he met, whether he had seen any case of the cholera; and if the answer were "*yes*," he went on to inquire whether, before that year, the person had ever met with the same complaint; and the reply was always, without a single exception, "*no*." Yet I say there were, and are, a few practitioners who denied, and deny, that it was anything more than the common and well-known English complaint, raging with unusual frequency and violence.

But we have evidence of a different kind of the newness of the epidemic cholera to these kingdoms. Its approach was discerned afar off, as distinctly as a storm is foreseen by the rising of the clouds from the horizon in the direction of the wind. The disorder began to rage with terrible severity, in the Delta of the Ganges, in the year 1817. I do not mean that it then broke out there for the first time. It had again and again desolated those regions before. But from its irruption in the year I have mentioned, when it committed frightful devastation in our armies in the north-eastern districts of India, its course can be distinctly traced to our own shores; towards which it approached with slow and halting, but with sure steps, in a north-western direction. From India it spread to Persia; from thence to Russia; and across through Poland to Germany: and at length it was found at Hamburg. It was predicted before that time, that the distemper *would* at length reach Great Britain. Our government had even sent two physicians into Russia to meet it, and to investigate its nature, in the fearful anticipation that its march across the earth would continue progressive; and accordingly, at the expiration of fourteen years, it made its appearance on the *eastern coast* of this country; in Sunderland: and in due time extended over every part of these islands. I say its arrival had been *foreseen* and *foretold*; and it is absurd to suppose that a vast number of persons would fall sick, and die, with symptoms quite strange to the great mass of practitioners here, merely to fulfil this prediction.

The progress of the disorder did not end here. Crossing the Atlantic, it invaded America; turning, at the same time, in a south-easterly direction, it ravaged France and Spain, and the north coast of Africa, and Italy.

Moving thus onward, as it did, in defiance of all natural or artificial barriers, under opposite extremes of season, temperature, and climate, in the teeth of adverse winds, over lofty mountain chains, across wide seas, through "hot, cold, moist, and dry"—in what manner, you will probably ask, was this wasting pestilence *propagated*?

Upon this point various and discordant opinions are entertained. Many persons believe that the complaint spread by contagion: more, however, that it was not contagious at all, but arose from some deleterious cause with which the general atmosphere of the place was pregnant. Now I cannot reconcile the phenomena of the appearance and extension of the malady with either of these hypotheses, *exclusively*. It must, I think, be granted that the complaint, in every instance, was excited by the application of some noxious material to the body, some positive poison. It is certain, also, whichever hypothesis may be chosen, that many more individuals were exposed to the agency of this poison, than were injuriously affected by it. This exemption from the disease no more invalidates the doctrine of contagion, than it invalidates the doctrine of some diffused atmospheric influence: nay it is more explicable upon the former than upon the latter supposition; for while many may avoid a specific contagion, all are immersed in, and all breathe, the common atmosphere. But the exemption shows *this*: that the exciting cause, to be effective, required a fit recipient; that the susceptibility of being hurt by the poison in its ordinary dose and intensity varied much in different persons; and in the majority was very faint, or wanting. It is clear that the poison travelled. It is equally clear to my mind, that it was *portable*; and therefore communicable from person to person. I even believe that it was capable of being conveyed, and was actually conveyed from one spot to another, by persons who were themselves proof against its effects; or who, at any rate, were unaffected by it. The innumerable authentic instances of coincidence, in point of time, between the first outbreak of the disorder in a particular place, and the arrival at that place of some person or persons from an infected locality, prove that the poison could be thus carried. Of this direct importation of the disorder into new and dis-

tant places, by infected individuals, and of its subsequent extension from those individuals to others who had intercourse with them, you may see a vast number of examples collected by Dr. James Simpson, in the 49th volume of the *Edinburgh Medical and Surgical Journal*. The evidence there adduced of the portability of the poison is abundant, and to my mind irresistible. Whether the malady was contagious in the same sense in which small-pox is contagious—whether, I mean, the cholera poison had the power of multiplying and reproducing itself in the human body, as yeast multiplies itself during the fermentation of beer—is a different and a much more doubtful question. A disorder may be contagious, without this property of reproduction in the animal fluids. The itch is contagious. The itch is produced by a minute parasitic animalcule, the existence of which has, of late years only, been assured to us by the microscope. Suppose that these itch insects could fly, or were capable of being wafted through the air—they would then represent what is conceivable enough of the subtle exciting cause of cholera. Between the two epidemic distempers, influenza and cholera, there were numerous and striking points of similitude and analogy. They have observed the same, or very nearly the same, geographical route. Both, issuing from their cradle in the east, have traversed the northern countries of Europe, till, arriving at its western boundary, they have divided into two great branches; the one proceeding onwards, across the Atlantic, the other turning in a retrograde direction, towards the south and east. The main differences between them have been, that whereas the poison of influenza spared very few of the community, inflicting a disease which, of itself, was seldom fatal—the poison of cholera, on the contrary, smote very few, but with so deadly a stroke that as many sank beneath it, probably, as recovered. Both were *general* disorders, affecting the whole system, but in both the most prominent of the symptoms had reference, in the majority of cases, to the mucous membranes: to those of the air-passages in the influenza; to those of the alimentary passages in the cholera.

Now this strong analogy has been made use of as an argument that the cholera was not contagious. “The influenza (say the objectors) had no contagious properties; therefore it is, *a priori*, likely that the cholera had none.” But I demur to the major proposition. Cullen thought the influenza *was* contagious, and I adverted, in a former lecture, to some facts which favour that belief. Supposing it, however, to be so, the proof of its contagious property must, from the very nature of the case, be extremely difficult. Its visitations are so rapid, widely spread, and multitudinous, that there is no time for its transference from house to house, or from person to person; yet it may nevertheless be transferable. Its inherent rate of locomotion outstrips and precludes the tardier conveyance of the poison by man. Its contagious qualities (granting them to exist) are hidden in its universality, and can seldom be traced but by accident. I therefore esteem this argument from analogy as worthless; and my own creed respecting the cholera is, that it *was* contagious in the limited sense already explained; but that its contagious power was not very great: that a comparatively small part of the population, of this country at least, was susceptible of its operation; and that few were in much danger of suffering from exposure to the physical cause of the disease, except under circumstances of predisposition. At the same time I believe that a great majority of the cases of cholera were not attributable to direct contagion, but to the poison diffused through the atmosphere. There is nothing inconsistent in the supposition that this noxious matter travelled sometimes by its own peculiar powers, sometimes made use of vehicles.

[During the prevalence of the cholera in Philadelphia, in 1832, we closely investigated every fact calculated to throw light upon the question of its contagious or non-contagious character, and for this investigation, our position in the Board of Health and as chief of a large hospital, afforded us ample opportunities—but we were unable to discover the slightest evidence of the disease having been in any one instance communicated from the sick to the well. And yet, there are many curious facts upon record which would seem to give countenance to the idea suggested by Dr. Watson, that the aerial poison productive of epidemic cholera may be conveyed from one spot to another, by persons who are themselves proof against its effects, or who at any rate were unaffected by it. We agree with Dr. Watson that a malarious disease may be portable without its being, therefore, necessarily contagious in the proper acceptation of the term. — C.]

This, I say, is my creed upon the vexed question of contagion. Respecting the special nature of the poison I can only guess; and my guessing, as you may have perceived, takes the same direction as before. I adverted, when speaking of the influenza, to what Sir Henry Holland has called "the hypothesis of insect life as a cause of disease." I shall not repeat the observations I then made; but I would refer you, for such curious thought and information upon the subject, to Sir Henry's very interesting essay. The hypothesis in question squares more readily than any other that I know of, with the ascertained history of the disorder: with its origin, after an unusually wet season, in the low marshy country, and hot atmosphere of Bengal: with its irregular but continuous migrations: with its dying away after a while, and its occasional and partial revivals. But still, remember that we are dealing merely with a *hypothesis*.

Whatever obscurity may overhang the *exciting* causes of the epidemic cholera, we are quite sure that certain circumstances exercised a strong *predisposing* influence upon the human body, to render it more than usually susceptible of the disease. The predisposing causes, as might well be imagined, were such as tended to debilitate the system: and therefore *poverty*, which implies scanty nourishment, and frequently also confinement of several persons to a narrow space, and want of fresh air; poverty, which includes these and other evils, was found to predispose the body to a ready reception of the malady. But to *intemperance*, more than to any other *single* cause, may the proclivity to become affected by this species of cholera be ascribed; and especially to the intemperate and habitual use of distilled spirits. This fact was peculiarly manifested in the selection, by the disease, of its victims in this country; and it has been remarked almost everywhere else.

I have all along spoken of the visitation of epidemic cholera in the past tense, because, for the last eleven or twelve years, we have heard but little of it. Yet we can scarcely venture to hope that the stranger pest has altogether forsaken us, for we have had slight sprinklings of the disease in and near London most summers, I believe, since 1832; but it has never again been extensively prevalent or epidemic. Certainly, it dealt lightly, upon the whole, with our country. It was much more general, and more widely fatal, in France, which it visited subsequently to its arriving here: it was very destructive also in its subsequent course, both westward and towards the south-east.

The epidemic cholera made its attack in two different modes. In one it seized upon the patient suddenly, and without warning. This was comparatively rare. Much more commonly the specific symptoms were preceded, for some little time, even for some days perhaps, by diarrhoea. And this I take to be the most important practical fact that was ascertained during its prevalence among us. When the disease was once fairly formed, medicine had very little power over it; but in the preliminary stage of diarrhoea it was easily manageable. Unfortunately people are inclined (especially those classes of the community among whom the cholera most raged) to regard a loose state of the bowels as salutary: and to make no complaint of it, and to do nothing for it: or, in other cases, they conceive it to proceed from some peccant matter within, which requires to be carried off, and they take purgative medicines to get rid of it. Both of these are serious and often fatal mistakes. Mere neglect of the diarrhoea frequently permitted it to run into well-marked and uncontrollable cholera; and the employment of purgatives hastened or insured that catastrophe. The proper plan of proceeding, I am convinced, was, to arrest the diarrhoea as soon as possible after its commencement, by astringents, aromatics, and opiates. You may object perhaps that the cases that were cured in this way were not cases of cholera at all, and never would have been; but simply ordinary diarrhoea. It is impossible to *prove* the contrary, no doubt; but the presumption is strong that the diarrhoea would, in many, and perhaps in most instances, have run on, if not checked, into the more perilous form of the disease. In many places, when, taught by experience, the authorities established *diarrhoea dispensaries*, to which those attacked by looseness of the bowels were warned and invited to apply, that the looseness might forthwith be corrected; in many such places the cholera, which had before been cutting off the inhabitants by scores, and by hundreds, began instantly to decline in frequency. I venture to advise you, supposing the disease should re-appear, or whenever in the autumn a suspicion arises that this form of cholera is present in the community, not to try, in cases

of diarrhoea, to carry off the presumed offending matter, but to quiet the irritation and to stop the flux as soon as you can.

But when the regular symptoms, peculiar to the severe form of cholera, had set in, medicine, I repeat, had very little influence upon it: and accordingly, as might have been expected, a hundred different cures of the disease were announced, most of them all but infallible. Some persons held that timely bleeding would save the patient: others relied confidently upon mustard emetics. Hot air baths were manufactured and sold to a great extent, to meet the apprehended attack in that manner without delay. Certain practitioners maintained that the disease was to be remedied by introducing into the system a large quantity of neutral salts, which were to liquefy and redder the blood, and to restore the functions of the circulation. But of this practice it was said in a sorry but true jest, that, however it might be with pigs or herrings, *salting* a patient in cholera was not always the same thing as *curing* him. In a great number of the sick the blood was mechanically diluted by pouring warm water, or salt and water, into their veins. Some physicians put their trust in brandy, some in opium, some in cajuput oil, which rose to I know not what price in the market; some again, in calomel alone.

Now, I would not willingly mislead or deceive you on this point, by speaking with a confidence for which I really have no warrant, of the success or propriety of any of these expedients. I believe that each in some cases did good, or *seemed* to do so; but I cannot doubt that some of them did sometimes also do harm. I had not more than six severe cases under my own charge; and I congratulated myself that the mortality among them was not greater than the average mortality. Three died, and three (I will not say were cured, but) recovered. The three that died I was called in to see when the disorder was at its height: in each case it went on with frightful rapidity, in spite of all the means adopted, and proved fatal a few hours afterwards. The three that recovered I saw somewhat earlier, but still not till the specific symptoms were present: one was a girl in the hospital. They all recovered under large and repeated doses of calomel. Yet (as I said before) I do not venture to affirm that the calomel cured them. In the first case which was treated in that way, I merely followed up the plan that had been begun by Dr. Latham, who had visited the patient for me when I was accidentally absent. I found that he had felt better, less sick and less faint, after taking half a drachm of calomel at a dose; and I repeated the same dose many times, for after every dose his pulse rose somewhat, and he appeared to rally. This was the same man whom I mentioned before as having made no urine from the Sunday to the Wednesday: all that time he kept discharging rice-water stools. At last, on the fourth day, he passed a *little* water, and his alvine evacuations became rather more consistent, and began to look *green*: and from that time he gradually got well. Afterwards I treated my hospital patient in the same way, and with the same event. Yet I will not pretend to say that these persons might not have done quite as well if they had been left entirely to themselves.

Some of the expedients recommended had certainly a very marked and immediate effect upon the condition of the patients, especially the injection of warm water into the veins. Many instances of this were related at the time. One I myself saw. The patient was a young man, who was nearly moribund apparently. His pulse had almost, if not quite, disappeared from the wrist; his voice was faint and husky; he was very blue, and his visage was ghastly and cadaverous: in one word, he was in an extreme state of collapse. Out of this he was brought in a few minutes by injecting warm water into one of the veins of his arm. The pulse again became distinct and full; and he sat up, and looked once more like one alive, and spoke in a strong voice. But he soon relapsed; and a repetition of the injection again rallied him, but not so thoroughly: and in the end he sank irretrievably. Dr. Babington told me of a patient whom he saw, speechless, and all but dead, and whose veins were injected. He then recovered so as to sit up, and talk, and even to joke, with the by-standers: but this amendment did not last either. Yet even this temporary recovery might be sometimes of great importance: might allow a dying man to execute a will, for example. And some of the persons thus revived got ultimately well. We had for some time a woman in the Middlesex Hospital acting as a nurse, who had been rescued, when at the verge of death in cholera, by the injection of warm water into her veins.

It was remarked of those who recovered, that some got well rapidly, and at once; while others fell into a state of continued fever, which frequently proved fatal some time after the violent and peculiar symptoms had ceased. Some, after the vomiting and purging and cramps had departed, died comatose; *over-drugged* sometimes, it is to be feared, by opium. The rude discipline to which they were subjected might account for some of the cases of fever. And the process of artificially replenishing the veins was certainly attended with much danger. The injection of *air* with the water—inflammation of the vein from the violence done to it—an over-repletion and distension of the vessels by the liquid—*might*, any one of them, and sometimes, I suppose, *did*, occasion the death of a patient. Never, certainly, was the artillery of medicine more vigorously plied—never were her troops, regular and volunteer, more meritoriously active. To many patients, no doubt, this busy interference made all the difference between life and death. But if the balance could be fairly struck, and the exact truth ascertained, I question whether we should find that the aggregate mortality from cholera, in this country, was any way disturbed by our craft. Excepting always the cases in which preliminary diarrhœa was checked, just as many, though not, perhaps, the very same individuals would, probably, have survived had no medication whatever been practised.

[Soon after the appearance of the cholera in Philadelphia, we were persuaded that had we a remedy capable of arresting the inordinate serous discharge, which in this disease is poured out by the mucous membrane of the alimentary canal, we should very readily succeed in its cure. We were, therefore, induced to try the effects of the acetate of lead, with the remedial powers of which, in a somewhat analogous disease, the cholera of infants, we were familiar. We administered it in pills, combined with opium and camphor, or when the stomach rejected it in this form, in solution with the acetate of morphia, and at the same time, by the rectum, in injections composed of a strong solution with the addition of laudanum. Under this plan of treatment, the discharges were in numerous instances promptly arrested, and the patients recovered, even after approaching collapse was indicated. From our subsequent experience with this plan of treatment, we are convinced, that had the remedy been generally resorted to from the commencement of the epidemic, the mortality of the disease would have been materially reduced. Since 1832 cases of the cholera have occurred almost every year; all that have fallen under our notice were treated by the acetate of lead, and we have lost none.—Dr. Graves subsequently recommended the same remedy, and speaks of its effects in the highest terms.—We usually administer the acetate of lead in the dose of one grain, combined with one of opium and the same quantity of camphor, every hour, or two, or three, according to the violence of the attack. The thirst of the patient was assuaged by small portions of ice held in the mouth and allowed slowly to dissolve—large and frequent draughts of cold water we always found to be injurious.—Cups to the abdomen were frequently employed, and we have reason to believe always with advantage.—From warm bathing, dry heat to the surface, sinapisms, or stimulating pediluvia, or from the internal use of stimulants, we have never seen any good effects result.—C.]

Since the foregoing lecture was written and delivered, the pestilence of cholera has twice revisited this country. I suffer the lecture to remain unaltered, because I still believe it to present, so far as it goes, a faithful outline of that terrible disorder in its intrinsic features, and in its external relations. The larger and the riper experience gathered by a host of competent and keen observers during the prevalence of the last two epidemics, demands however a few additional words of summary and comment.

In their general course and character, the three epidemics of 1831–32, of 1848–49, and of 1853–54 have manifested a strong mutual resemblance. Each had a period of invasion—then a pause—and then again a subsequent fiercer outbreak, determined apparently by atmospheric conditions. All three fell with unequal severity upon different parts of the kingdom; and the parts which suffered the most, and the parts which suffered the least, were, with few exceptions, the same in them all. In each the absolute mortality from cholera was the highest in the months of August and September. More persons died of it in 1849 than in 1832, fewer in 1854 than in

1849. To give you some notion of the desolating power of the disease, I may tell you that during its second visitation there died in England of cholera and diarrhœa upwards of seventy thousand persons. I couple the two advisedly. Each of the three cholera epidemics was preceded and accompanied by diarrhœa, which was unusually fatal as well as unusually frequent. There can be now no doubt that the excess of diarrhœa above the average of common years was due to the exciting cause of cholera; or rather, that most of the cases registered under the head of diarrhœa were really cases of cholera, running a comparatively slow course, and shorn of its more striking symptoms. This fatal choleraic diarrhœa occurred most often at the two extremes of life, while the deaths from fully developed cholera were most numerous in its middle period. It appears from the statistical tables published under the authority of the Registrar-General, that the average duration of fatal diarrhœa was about sixteen days, of summer cholera about five days, of epidemic cholera about two days.

Many converts have been made to the doctrine which I have held from the beginning, that epidemic cholera is *catching*: that it results from a material poison which is portable, capable of being conveyed from place to place, and communicated from person to person—or from inanimate substances to which it clings, such as articles of furniture or clothing. That the morbid matter floats also in the air, and may be wafted about by its currents, is a general and well-founded belief. I think, with Dr. Baly, that when it travels over great distances, as from one country or region to another, it uses the vehicle of human intercourse; but that it may be and often is diffused over smaller spaces, as from one part of a town to another, or from a tainted port to a ship anchored to leeward, by the movements of the atmosphere. The long migrations of the disease are not made rapidly. Its rate of progress never exceeds, and is often slower than, that of modern travelling. Its primary appearance in an island or a kingdom is always at its outer boundary. In our own country, for example, it first planted its foot in a seaport town on the *east* coast, over against the main land where cholera was raging, and whence ships had very recently arrived. The same is true of its two subsequent visitations. On the other hand, the crews of vessels sailing from healthy places remain free from the disease until they have entered an infected port, or held intercourse with an infected shore.

That the poison may thus be carried about is now, I think, beyond dispute. Whether it multiplies itself in the human body as the virus of small-pox does, is still a vexed question. That it must somehow multiply and increase is certain. A theory has been broached by Dr. Snow that the poison is *swallowed* with the food which we eat, or the water which we drink; and that its multiplication takes place in the alimentary canal, whence a new and abundant stock of it is voided. He shows how easily portions of the rice-water excretions, colourless and inodorous as they are, may without our notice come to adhere to our food during its preparation, or its consumption. And the horribly disgusting fact has been made too certain by the unchallengeable disclosures of the microscope, that the water which is supplied by the several water companies for domestic purposes to this great city does habitually contain visible particles of human ordure. Some striking facts have been collected by Dr. Snow, which warrant the presumption that a most fearful outbreak of cholera in Soho was attributable to the water of a certain pump, contaminated from a neighbouring sewer. A remarkable converse fact has been reported by Mr. Lawrence. Bethlem Hospital, and an asylum for children called the House of Occupation, stand near together on an open space of ground between fourteen and sixteen acres in extent, lying in the parish of St. George, Southwark. Being dissatisfied with the filthy water then supplied by the Lambeth Company, the Governors some thirty years ago sank Artesian wells on the premises, and the water thus procured is used exclusively in the two institutions, which number between them about seven hundred residents. There has not been a single case of cholera in the Hospital or in the House of Occupation in any of the three epidemics; although the disease has prevailed extensively in the parish, and in the streets in their immediate vicinity.

The result of an inquiry suggested by the Board of Health into the effects of the consumption of impure water during the last two cholera epidemics, is favourable to Dr. Snow's theory. Mr. Simon reports that "the population drinking dirty water

appears to have suffered $3\frac{1}{2}$ times as much mortality as the population drinking other water."

But granting that the material poison may find its way into the body in company with the food and drink, we must believe that much more commonly it is suspended in the air, and enters with the breath. There are certain conditions of the atmosphere which seem to be almost essential to its activity, and its power to spread. It appears from the extremely interesting report of Mr. Glaisher on this subject, that "the three epidemics were attended with a particular state of atmosphere, characterized by a prevalent mist," (he is speaking of London and its immediate neighbourhood,) "thin in high places, dense in low. During the height of the epidemic in all cases, the reading of the barometer was remarkably high, and the atmosphere thick. In 1849 and 1854 the temperature was above its average, and a total absence of rain, and a stillness of air amounting almost to calm, accompanied the progress of the disease on each occasion. In places near the river night temperatures were high, with small diurnal range. He goes on to enumerate, as characteristic of the atmosphere at these periods, "a dense torpid mist; and air charged with the many impurities arising from the exhalations of the river and adjoining marshes; a deficiency of electricity; and (as shown in 1854) a total absence of ozone, most probably destroyed by the decomposition of the organic matter with which the air in these situations is strongly charged."

The ozone here mentioned is endowed, as I told you formerly, with peculiar purifying properties. It has a high oxidizing power, in virtue of which it unites with, decomposes, and so destroys miasmata, while it is at the same time itself proportionately destroyed. There is no ground for ascribing cholera, as some have done, to the absence of ozone—except in the sense of there not having been a sufficient quantity of it in the atmosphere to counteract all the poisonous miasm which actually produces that disease. The total absence of ozone affords presumptive evidence of the presence of atmospheric impurities.

A remarkable law of *altitude*, that is of elevation above the level of the Thames, has been announced by Dr. Farr as governing the mortality of cholera in this metropolis: and if here, so doubtless, under similar circumstances, elsewhere. "The elevation," he says, "of the soil in London has a more constant relation with the mortality from cholera, than any other known element." The mortality is inversely as the altitude.

This law of altitude — so important and so practically valuable — is but an expression of the result of many concurrent circumstances. The material poison of cholera will be likely to gravitate, as the marsh poison gravitates, with which it has many points of analogy, to the lowest part of the atmosphere; where the high barometrical pressure is the greatest, and vaporous diffusion therefore the least; where unwholesome exhalations from the soil and from the water are the most abundant; where the dispersing and diluting influence of winds is least felt. Indeed the air may be completely stagnant while on the neighbouring heights a brisk breeze is blowing. The lower regions of the atmosphere are the hotter also as well as the moister; and under the agency of a high temperature the organic impurity with which the air is charged runs more readily into decomposition. The inverse law of altitude is therefore an intelligible law. We see also how it may sometimes be disturbed or broken, under exceptional circumstances.

Bear in mind the notorious and lamentable fact, that of this enormous and ever-growing town, the river Thames is made, without metaphor, the common sewer. Foul with the daily and hourly influx of abominable filth, it is offensive to the senses, and a cause of added foulness to the incumbent atmosphere. When we learn from Mr. Glaisher that during the summer months the night temperature of the river is considerably above the minimum temperature of the air, and that its vast area is simmering all night long, and throwing off clouds of noisome and noxious vapour, we need be at no loss to account for the special unhealthiness of those quarters of the town which lie nearest to its banks.

But however unwholesome and pernicious the atmosphere may thus become, it cannot generate cholera, unless the specific exciting poison of that disorder be present also. On the other hand, there is good reason to believe that this poison, although it may strike and destroy individuals here and there, can never create a spreading pes-

tilence, unless it meets with a congenial atmosphere. The foul air lends force and diffusion to the poison, and aids, or causes, its increase. It is the doctrine of some of the soundest physiologists of this country, that the hitherto undiscovered cause of the disease "acts, in its wanderings, after the manner of a *ferment*; that it therefore takes effect only amid congenial circumstances, and that the stuff out of which it brews poison must be air or water abounding with organic impurity. Either in air or in water it seems probable that the infection can grow. The impurity of one commonly implies the impurity of both. But on the whole evidence it seems impossible to doubt that the influences which determine in mass the geographical distribution of cholera in London, belong less to the water than to the air."

This notion of there being two factors requisite for the perfection of the cholera poison, namely, a ferment, or leaven, and a material upon which the ferment operates, was first proposed, I believe, by Dr. Pettenkofer, of Munich.

There is another theory respecting the propagation of cholera to which I must direct your attention, because if it shall turn out to be a true theory, it points to a very plain and easy method of preventing, or checking the extension of any future epidemic. The theory assumes that the cholera poison is an animal poison, and that it is generated in conformity with a law which regulates the development of some other known animal poisons. On a former occasion I spoke of the *cadaveric* poison which is evolved during a particular stage only of the decomposition of dead animal tissues. Now it is held by some eminent pathologists that the characteristic rice-water discharges in cholera, become poisonous after the same rule. At first, for a day or so, they are innocuous. Then, as decomposition proceeds, they become morbid; capable of exciting the specific disease of which they were the product. After a few days more, when decomposition has reached a further stage, the contagious property ceases. Experiments are appealed to in support of this theory. Pieces of filtering paper, soaked in the rice-water fluids and afterwards dried, have been given, re-moistened and mixed with their food, to white mice. Papers steeped in the very recent, and papers steeped in the older discharges, proved alike harmless. But of thirty-four mice that ate papers impregnated with discharges of an intermediate date, thirty were disordered, and twelve died; and the symptoms, and the appearances noticed after death, are said to have been similar to those that are proper to cholera as it is seen in the human subject. The question of the soundness of this doctrine must still, however, be regarded as unsettled.

Whichever of these theories concerning the cause and diffusion of cholera may be the true one—and indeed whether any or none of them be true—they all bear a sufficient amount of likelihood, and are sustained by evidence enough, to render it our imperative duty, in the face of so great a danger, to enforce by our counsel the measures of precaution which they severally suggest. That a strict observance of the requisite precautions would disarm any future return of the pestilence of most of its terrors, and confer upon thousands of individuals the privilege of absolute security, I firmly believe.

And in order to avert so great a prospective peril, from individuals and from the community, no care can be too minute, too early, or too constantly exercised. There is much reason to fear that this comparatively new distemper is already domiciled among us, even as scarlet fever, small-pox, and measles are domiciled. Seventeen years elapsed from its first to its second visitation. The interval between its second and its third was only five: and, what to my mind is still more significant, there has been, since its first arrival, a fearful increase of fatal *diarrhæa* in this country. In 1838, the deaths by diarrhœa were 2482; in 1847, they were 11,595; in 1850, 11,468; in 1851, 14,728; and in 1852, 17,617. In this enumeration I have purposely excluded the years and part-years of epidemic cholera; so that the annual mortality from diarrhœa has increased sevenfold in fourteen years. It can scarcely be doubted that the milder form of disorder thus largely multiplied, partakes of the choleraic character; or that the embers of the pestilence not yet extinct may easily be roused by favouring circumstances into a fresh conflagration.

Of the circumstances which concur to augment the intensity and to enlarge the operation of the exciting cause of cholera, some are beyond human control; some may be obviated by the efforts of society as a body; some may be got rid of or avoided by each man for himself. We cannot regulate the temperature of the air; nor determine

its barometrical pressure; nor influence, on any large scale, its movements. The removal of decomposing filth, the promotion of ventilation by opening up close and crowded neighbourhoods, the enforcement of effectual drainage, the tapping of gully holes in the streets, the prevention of a perpetual leakage of gas into the subsoil, the constant supply of pure water, the prohibition of intramural sepulture, the emancipation of our noble river from needless pollution, — these are practicable objects, fairly within the scope of legislative action, and imposing very solemn obligations upon our rulers in the State.

But our business lies rather with man in his individual capacity.

Upon the first appearance or threatening of epidemic cholera in this town, I would counsel every one who is not kept within it by necessity or by duty, to fly from the danger; to betake himself, with his household, to the moors of Scotland, to the Welch hills, or to some upland place in one or other of those districts of England which the pestilence has hitherto passed by, or touched but lightly. Which are those districts, may be gathered from the 17th Annual Report of the Registrar General. And there is ample room for choice. People, in these alarms, are apt to hurry to the sea-coast; but that is a mistake. From the same book we learn that a person living in the coast-districts in a cholera season is four times as likely to die of that disease as a person living inland. The east coast, facing the continent, is the worst. The south is better than the west.

In London itself (and what is true of London is true, *mutatis mutandis*, of great towns generally) the more elevated situations are, *cæteris paribus*, the safer, in proportion to their elevation. Wherever he may dwell, every prudent person will look to the efficiency of his drains, and the thorough ventilation of his apartments. During the prevalence of the epidemic, no water should be drunk which is derived from any questionable source; or, if no unblemished source be accessible, all that is used for drinking or for culinary purposes should first be filtered, and afterwards boiled. The artificial waters, soda water or seltzer, when they can be obtained and afforded, are preferable to water of which the purity is doubtful. It is almost superfluous to add that water drawn from a river into which sewers flow, or which is navigated by persons living in boats, should be rejected. Domestic and personal cleanliness, at all times a duty, is now an obligation. No rotting rubbish should be allowed to accumulate in dust-bins. No stench should pass without challenge and correction.

Meanwhile all things which tend to weaken the body, and to dispose it to a ready reception of the disease, should be carefully avoided; intemperance especially — unwholesome food — the use of drastic purgatives.

If, in despite of all precaution, cholera should appear in a house, strict attention to cleanliness becomes even more imperatively necessary. Nurses and others should wash their hands before they eat or handle food. All discharges from the body of the sick person, and all linen soiled with them, should forthwith be immersed in some disinfecting fluid, buried, burned, or otherwise disposed of, before that period of decomposition at which, according to one theory, they become contagious. It has been wisely suggested that the alvine excretions should be received on some worthless material (tow for instance) which might be buried or consumed without delay. A pan containing recently-burned charcoal may rationally be placed in the sick chamber.

Among measures calculated to protect the *community*, by checking the extension of the disease when it is already prevalent, the principal are—

1. House to house visitation by competent inspectors, for the two-fold purpose of enforcing cleanliness and ventilation, and of detecting the malady in its earliest manifestations.

2. The conveyance of the poor who are smitten with the disease to cholera hospitals, or to cholera-wards in the general hospitals of the place.

3. The removal of still healthy poor to houses of refuge, while their tainted dwellings are being disinfected and purified.

The last of these expedients has not hitherto been adopted in London; but it is an excellent expedient, and has doubtless been the means of saving many lives elsewhere, and especially in Scotland. It is stated by Dr. Alison, that of more than 1000 persons in Edinburgh and Glasgow, all of whom were taken from *rooms* in which the disease existed, 40 alone were attacked with cholera, and only 15 died, while 978 escaped

with their lives. In Oxford a similar measure was resorted to in 1849, and not a single case of the disorder occurred among 70 persons to whom shelter was given in these houses.

I can add but little to what I told you formerly respecting the proper treatment of this disorder. Diarrhœa—especially painless, copious, and exhausting diarrhœa, which often is nothing less than incipient cholera—is to be checked at once if that be possible: almost every one has his own drug, and method of proceeding, for that purpose. Sulphuric acid has been highly praised by many. I mention it because it is one of the newest remedies so used, and used with apparent success. In the autumn of 1854 I spent five weeks in a Welch village which (wonderful to relate) did not possess a medical inhabitant. Diarrhœa was exceedingly prevalent there during most of that time. It yielded readily to scruple doses of the *pulvis cretæ compositus cum opio* of the Pharmacopœia. The disorder frequently returned it is true; but that was owing, I believe, to some atmospheric condition which was in continuous operation. The same tendency to recur would probably have shown itself after a cure by any other means. The patient should be kept in bed; which implies quietude, warmth, and (what is very important) the recumbent posture. Most observers agree that in the early periods of an epidemic the prevailing diarrhœa is less easily restrained than during its decline.

In the stage of collapse, I know of no drug upon which any reliance can reasonably be placed. For myself, I should permit the patient to drink as much cold water as he wished for. If he suffered severe pain from cramps, I should seek to give him ease by the cautious use of chloroform: and to these measures I should add full and repeated doses of calomel.

For the fever which in some cases succeeds to the recovery from collapse, and which occurs more rarely without previous collapse, no particular directions can be given. Special attention must always be paid to the kidneys and their functions. These glands become congested in cholera, and the thick blood is loaded and poisoned with undischarged urea. While this unnatural state of things continues, structural mischief is very apt to arise in the kidneys themselves, and in other organs of the body. The remedies are bland diluent drinks, the warm hip-bath, and (perhaps) the removal of a few ounces of blood from the loins by cupping-glasses.

The literature of cholera is endless and perplexing. Dr. Farr's letters in the Reports of the Registrar General; the Report on cholera to the College of Physicians, by Doctors Baly and Gull; Dr. Acland's memoir of the cholera at Oxford; and the several Reports issued on the subject by the General Board of Health, are the best sources of authentic information that I have seen.

[Of the diseases to which children are liable in the middle and southern portions of the United States, few produce a greater amount of mortality annually, than CHOLERA INFANTUM, or the Summer Complaint of children. It is an endemic of all our larger cities, during the season of the greatest heat; attacking children between four and twenty months of age, or at the period of the first dentition. So generally is it confined to this period of life, that an infant's second summer is considered by mothers as one of unusual peril, and should it escape an attack at this period, or pass safely through the disease, it is considered to have a fair chance of surviving the period of infancy.

The first symptom of the disease is usually a profuse diarrhœa, the stools being very fluid, generally of a very light colour, though often of a pale yellow or green; to the diarrhœa is soon added an extreme irritability of the stomach, everything taken into it being rejected immediately, and with violence. The irritability of stomach, in most cases, continues throughout the attack, and in many cases frequent spontaneous vomiting is a prominent symptom.

After the disease has continued for a short time, the discharges from the bowels are ordinarily composed entirely of a perfectly colourless and inodorous fluid, containing often minute mucous flocculi, and are discharged without the least effort. They are occasionally, however, very small in quantity, and squirted, as it were, from the anus. In these cases, there is usually more or less tormina and tenesmus.

Very frequently the vomiting becomes suspended, even at an early period of the attack, while the discharges from the bowels continue, or augment in frequency and

in quantity, the irritability of the intestinal canal being often such, as to cause whatever food or drink is taken to pass off rapidly, without having undergone the slightest change.

The infant becomes quickly affected with extreme languor and prostration, and is rapidly emaciated—being reduced in a few days, often hours, to an extent that would scarcely be credited by those unacquainted with the disease.

The pulse, from the very commencement of the attack, is usually quick, frequent, small, and often tense. The skin is dry and harsh, the head and abdomen are often hot, while the extremities retain their natural temperature, or are even decidedly cold. The tongue is moist, and covered with a white slimy mucus. There is always intense thirst—whatever fluid is taken being almost immediately ejected from the stomach. The child suffers more or less pain in the abdomen, as indicated by its fretfulness, low moaning cries, frequent change of posture, the drawing up of its knees, and its occasional acute screams. The abdomen is sometimes tumid, and generally tender to the touch. Towards evening, there occurs, in most cases, a decided febrile reaction.

In many cases, some degree of delirium, with an injected and wild appearance of the eyes, and a tossing of the head backwards and forwards, is early manifested. When this is the case, we have frequently seen the patient attempt to bite or scratch his attendants.

The disease usually runs a protracted course. The discharges from the bowels continue to be frequent and profuse, but dark-coloured, like dirty water, or the washings of stale meat, and often very offensive. They not unfrequently, however, are small in quantity, and composed entirely of a dark-coloured mucus, mixed with the food and drinks that have been taken. The emaciation of the patient becomes extreme; his eyes are languid, hollow, and glassy; his countenance pale and shrunk; his nose sharp and pointed; and the lips thin, dry and shrivelled. The surface of the body becomes cool and clammy, of a dirty brownish hue, and often covered with petechiæ. The tongue is dark-coloured, smooth and shining, or covered, as well as the parietes of the mouth, with aphthæ. In many cases the child lies constantly in an imperfect doze, with half-closed eyelids, and so insensible to external impressions, that flies will frequently light upon the half-closed eyeballs, without the patient exhibiting the least consciousness of their presence. The abdomen becomes more or less tympanitic, and the hands and feet of a leaden hue, or pallid and œdematous. The fauces, becoming dry, causes a sense of uneasiness, which often induces the patient to thrust his hand deep in the mouth, as if to remove some offending substance.

In many of the protracted cases, an eruption of very minute white vesicles occurs upon the neck and breast. This Dr. Dewees considered to be invariably a fatal symptom; but we have seen many patients recover, even when this eruption has been the most extensive and distinct.

The patient, unless relieved from his suffering by a judicious treatment, becomes daily more and more exhausted, rolls his head about when awake, and utters constantly short, plaintive, scarcely audible cries. He falls at length into a state of complete coma, death being frequently preceded by a convulsive attack. Not unfrequently, at an early period of the disease, the brain becomes affected, and the child dies with all the symptoms of acute meningitis.

Cholera infantum is of very variable duration. In violent attacks, the prostration which suddenly ensues is occasionally so extreme, that the patient is destroyed within the first twenty-four hours. Usually, however, the disease is of many days, or even weeks' continuance, and the patient generally sinks, apparently from a total cessation of the nutrition of the system.

The lesions exhibited by the post-mortem examination of those who have died from cholera infantum, vary according to the period of the disease when death takes place. When the disease has been of short continuance, the mucous membrane of the alimentary canal has been occasionally found of an abnormal paleness, and the liver more or less congested. When the case has been of a more protracted character, increased redness in points or patches, in different parts of the stomach and intestines, is often present. The red points are sometimes very minute and isolated, and spread over a considerable portion of the stomach and duodenum, or over the small intestines

only. They have the appearance, generally, of minute extravasations of blood. In the lower intestines the points occur in clusters, so as to form patches or bands of redness, varying in size, though never of any great extent, and often slightly elevated, from a thickening of the mucous tissue at the parts occupied by them. Occasionally, portions of the mucous membrane are more or less softened — often without the slightest inflammation. In other instances, increased redness of some portion of the intestine exists, with contractions, often extreme, of its calibre.

The mucous follicles of the intestines are very generally enlarged, often in a state of inflammation, and occasionally of ulceration. Dr. Horner describes the appearance of the enlarged follicles, as resembling a sprinkling of white sand upon the surface of the mucous membrane. The intestines are generally empty, or contain merely a small amount of thick tenacious mucus. Drs. Page and Lindsly describe an appearance of dark spots upon the mucous membrane of the stomach, above its pyloric orifice. We have never detected it.

The liver is almost invariably enlarged, and more or less congested; while the gall-bladder is filled with dark-green bile, or a pale and almost colourless fluid. Dr. Page describes the liver as being in some cases large, soft, and spongy; and Dr. Horner, as being usually of a light yellow or mottled colour.

In a few cases, indications of inflammation of the meninges of the brain are present.

The prognosis in cholera infantum will depend very much upon our ability to remove the patient from the influence of the impure, damp, and heated atmosphere by which the disease has been produced and is kept up, as well as upon the period of the attack at which the treatment is commenced. Without this removal, it is scarcely possible to effect, in any case, a permanent cure; while in most cases—in their commencement, at least—little else is required to arrest the disease. Even at a later period, its effects are often strikingly evinced in the rapid improvement of the patient, from almost the very moment the removal takes place. In cases where the disease has continued for many days, and reduced the patient to a state in which a fatal termination would seem inevitable, by removal to the free open air of the country, and an appropriate course of treatment, a very rapid recovery has been often effected.

Cholera infantum is evidently produced by the action of a heated, impure, damp, and stagnant atmosphere, directly upon the skin, and indirectly upon the digestive mucous surface, at an age when the latter is strongly predisposed to disease from the effects of dentition, and from the increased development and activity of the muciparous follicles which takes place at that period.

The dependence of cholera infantum upon a high degree of atmospheric temperature is shown by the fact, that its prevalence is always in proportion to the heat of the summer; the disease increasing and becoming more fatal with the rise of the thermometer, and declining with the first appearance of cool weather in the autumn. That, however, the disease is not produced by heat alone, in its more aggravated forms, is proved by its occurring almost exclusively in the larger and more crowded cities of the Middle and Southern States, and by its especially prevailing, and being most destructive to life, among the children of the poorer classes, inhabiting small, damp, ill-ventilated houses, situated in narrow, confined lanes, courts, and alleys, or in situations abounding with accumulations of filth. When it occurs in the country, which is rarely the case, it is almost exclusively in low, damp, and otherwise unhealthy situations.

The process of dentition is unquestionably a predisposing cause of the disease; while premature weaning and errors in diet act often as exciting causes.

In regard to the treatment of the disease, this is very simple, and generally successful, whenever we are able to remove the patient from the heated, confined, and impure atmosphere by which the disease has been generated, to a situation where he may enjoy the advantages of a cool air and free ventilation.

The infant should be confined to the breast, or, if weaned, to a diet of fresh rennet-whey, with the addition of gum acacia, calves' foot jelly, tapioca, or plain meat broths, with some cool, perfectly bland, and slightly mucilaginous fluid for drink. He should be immersed daily in a bath, warm or tepid, according as the temperature of the skin is deficient or increased.

If the removal of the patient is impracticable, he should be placed in as pure, cool,

and free an atmosphere as possible, and carried frequently abroad in any open and healthy situation in the neighbourhood of his residence, in a carriage, or in the arms—or where his residence is near a large river, he should be taken on the water in a boat. His clothing should be perfectly clean and dry, and sufficient to guard against the influence of sudden changes of temperature, but not so warm as to overheat the patient: fine soft flannel, or soft, coarse muslin, worn next the skin, will be proper in all cases. His sleeping apartment should be, if possible, large, dry, and airy. He should sleep upon a mattress, or on a blanket folded and laid upon the sacking-bottom of the bedstead, or upon the floor of the crib, his body being defended by a light, loose covering.

The gums should be carefully examined, and if they are hot, swollen and inflamed, they should be freely lanced.

When the disease commences as a simple diarrhoea, the warm bath, repeated daily, or even night and morning, and followed by gentle friction over the surface of the body, with a hand or soft dry cloth; cold mucilaginous drinks, and a combination of a sixth of a grain of calomel, about four grains of prepared chalk, and a half a grain of acetate of lead, repeated every three or four hours, will ordinarily arrest it.

To subdue the irritability of the stomach, from a sixth to a fourth of a grain of calomel, rubbed up with a little dry loaf-sugar, and sprinkled upon the tongue, will very generally be found sufficient. When, however, this fails, a few drops of the spirits of turpentine, or a solution of camphor in sulphuric æther, repeated at short intervals, will usually succeed. When the vomiting is violent and frequent, particularly if there is any pain or tenderness of the abdomen, a few leeches to the epigastrium, followed by a light emollient cataplasm or warm fomentations, will be found decidedly beneficial. The effects of fomentations to the abdomen, with a strong decoction of hops, have been spoken of by many practitioners as peculiarly soothing.

A teaspoonful of cold water may be allowed every fifteen or twenty minutes; it is grateful to the patient, and will assist in allaying the gastric irritability.

When the irritability of the stomach is so far quieted as to allow of the remedy being retained, the disordered action of the bowels will in most cases be promptly restrained by a combination of a fourth of a grain of calomel, three grains of prepared chalk, one grain of acetate of lead, and a fourth of a grain of ipecacuanha, given every three hours. Under the use of this prescription, the inordinate discharges will be quickly suspended, and replaced by regular, natural stools. The acetate of lead should be omitted as soon as the watery discharges are arrested, and the calomel, prepared chalk, and ipecacuanha, in the same proportion as above, continued until regular and healthy stools are procured. The use of some one of the light vegetable astringents will complete the cure. The full restoration of the patient to strength will be greatly promoted by a mild unirritating diet, daily exercise in the open air, and the most scrupulous cleanliness of person and clothing.

In cases where much heat of the head, a wild injected state of the eyes, aversion from light, with delirium, or other symptoms of cerebral disease ensue, leeches should be applied to the temples or behind the ears, cold lotions to the scalp, and warm sinapisms, pediluvia, or some stimulating embrocation to the lower extremities. In these cases, blisters behind the ears, kept open by the use of some irritating ointment, will often be found beneficial.

In the chronic stage of cholera infantum, the most efficacious remedies are the warm bath, repeated daily; blisters to the abdomen, kept on until the skin is reddened and then removed, the part being covered with a soft emollient poultice; injections composed of starch and a few drops of laudanum; small doses of Dover's powder at night, with light astringents, as kinó, decoction of dewberry-root, or of the geranium maculatum, with change of air, and a diet of boiled milk thickened with rice-flour, or of plain meat broths with the addition of rice. A solution of the tartrate of iron, or of the ammoniated tartrate of iron, twenty grains to the ounce of water, with the addition of a drachm or two of ginger syrup, or the persesquinitrate of iron, may be administered with good effects. In many cases the sulphate of quinia in solution, besides exerting a beneficial influence upon the disordered condition of the bowels, will be found useful in restoring strength to the patient, who is always in a state of extreme prostration.

When the stools are thin, small in quantity, dark-coloured, and highly offensive,

with flatulence, and a tendency to a tympanitic condition of the abdomen, or when frequent griping pains are experienced, the best effects will be derived from the use of turpentine. It may be given in the following mixture:—R. Mucil. g. acaciæ ʒiij.; sacch. alb. pur. ʒij.; spir. æther. nit. ʒiij.; spir. terebinth. ʒij.; magnes. calc. gr xiiij.; lavend. spir. comp. ʒij.—M. The dose of which is a teaspoonful three times a day—or oftener, when the child is over two years of age. The addition of ʒiij. of the tincture of kino, and the same quantity of the camphorated tincture of opium, will be proper in cases attended with great irritability of the bowels.

When the discharges are acrid, offensive, and dark-coloured, we have administered with advantage pulverized charcoal, two to eight grains, in combination with two or three grains of powdered rhubarb, a fourth to a half a grain of ipecacuanha, and a grain of extract of hyosciamus every three or four hours.

The utmost attention must be paid to the diet of the patient; he should be confined to meat broths—the juice of roasted meats, with boiled rice—rice milk and tapioca in moderate quantities at a time—with toast or rice water, or rennet-whey, with the addition of a portion of gum acacia, for drink.—C.]

LECTURE LXXIII.

Dysentery. Diarrhœa Adiposa. Intestinal Concretions. Worms.

ANOTHER of the morbid fluxes from the alimentary tube, of which I have yet to speak, is *dysentery*.

Its characteristic symptoms are, griping pains in the abdomen, followed by frequent mucous or bloody stools, straining, and tenesmus. In chronic cases pus is sometimes discharged from the bowels. The acute form or stage of the disease is attended with fever.

The difference between dysentery and diarrhœa are obvious enough. Both of them may be accompanied by griping pains: in both the stools are frequent and loose: but in diarrhœa they are fæcal; in dysentery there is retention of the natural fæces, or they are expelled from time to time, in small, hard, separate lumps, termed *scybalæ*. Again, straining, and tenesmus, and the excretion of mucus, which is often tinged with blood, form no necessary features in diarrhœa; whereas in dysentery those symptoms are prominent and constant. These nosological distinctions are true and useful, although in our actual intercourse with the sick we do not find them always or strictly observed. Some of the worst forms of dysentery *commence* with the ordinary symptoms of diarrhœa.

Dysentery consists, essentially, in inflammation of the mucous membrane of the large intestines; yet not, I apprehend, of the whole of that long surface indiscriminately. Observation of the course of the disorder, during life, and of the morbid appearances visible after death, leads to the conclusion that in simple dysentery, marked by tormina and tenesmus, and frequent dejections of sanguinolent mucus, without fæcal matter, the inflammation chiefly affects the *rectum* and the *descending colon*. When the earlier portions of the large intestines are involved in the diseased process, the stools at the outset are often composed in great measure of excrement in an unnaturally fluid state, and mingled with blood and slime. We generally speak of these circumstances as constituting *dysenteric diarrhœa*.

Slight and simple dysentery may occur and run its course with very little or no disturbance of the circulation. When it is acute and severe, it is attended with more or less pyrexia. The acute disease may terminate in recovery; or in early death; or in chronic dysentery, which usually, in the end, is fatal.

The wards of our metropolitan hospitals place frequently under our notice severe cases of chronic dysentery in the persons of soldiers and sailors, who bring the disease

home with them from hot climates. With these exceptions, dysentery, now-a-days, is neither a very common nor a very serious disorder in this country. I say now-a-days, for the time was when it raged in London like a plague. The second Dr. Heberden, in his valuable essay, *On the Increase and Decrease of different Diseases*, shows, that in the seventeenth century the number of deaths set down, in the weekly bills of mortality, under the titles of *bloody flux*, and *gripping in the guts*, was never less than 1000 annually, and in some years exceeded 4000. For five-and-twenty years together, viz. from 1667 to 1692, they every year amounted to above 2000. During the last century, the number gradually dwindled down to twenty. Dysentery is one of the pests of hot climates. In all tropical regions at certain seasons of the year it is very prevalent and destructive. But it is in fleets and armies, and especially among troops in actual service, that the distemper most displays its terrible power. There is no single malady which is so crippling to an army in the field as this. Sir James M'Grigor, to whom was entrusted the superintendence of the medical department of the army on "the two greatest services on which the military force of this country has, of late years, been employed, namely, that in Walcheren, and that in the Peninsula," calls dysentery "the scourge of armies," and the "most fatal of all" their diseases. In two years and a half, the British army in Spain lost no less than 4717 men by this complaint.

How are these facts to be explained? Wherefore is dysentery, which was so familiar to our ancestors, so happily rare among us? Why does it thus wait upon and afflict the march of armies? Upon what depends its frequency in hot climates? We may expect to obtain some answer to these questions by searching into the *causes* of the disorder.

It has been ascribed to exposure to wet and cold; to the use of unwholesome food; to the agency of malaria; to contagion.

Weather and season have a manifest influence in the production of dysentery. In temperate climates, like our own, it is an autumnal disorder. In tropical countries it is observed to be more common and more severe when rains succeed to a long-continued drought. In respect to this, as to other bowel affections, a high diurnal temperature of the air appears to be the predisposing, and exposure to cold the exciting cause. I stated, on a former occasion, that great vicissitudes of temperature are very frequent and very pernicious, even under the torrid zone. Scorching days are followed by extremely cold nights. The dysentery which arises under these circumstances is apt to run on into the ensuing winter. Soldiers in the field against an enemy are peculiarly obnoxious to the agencies which favour or generate the complaint. Marching, or engaged in actual conflict, during the day; bivouacking at night, often in the open air, and under every variety of weather; ill-provided too often with clothes and bedding; their food scanty, precarious, or of bad quality; seizing the many opportunities which their dreadful trade supplies of license and intemperance; depressed, it may be, by disaster or defeat; we need not wonder either at the prevalence of dysentery among them, or at its untractableness while they remain subject to the same morbid influences. Neither can the causes be warded off from the patient; nor, in general, can the patient be removed from the causes. Yet occasions do arise which show distinctly enough this alleged relation of cause and effect. *Præsens morbum facit—sublata tollit*. Take on the one side, the following facts from Sir John Pringle's book *On the Diseases of the Army*. The men who had fought at Dettingen lay that night on the field of battle, without tents, exposed to a heavy rain. For the next night or two they encamped on better, but still wet ground; and they wanted straw. Nearly half of these troops were soon after affected with dysentery; while three companies which had not been engaged in the battle, nor exposed to rain, nor lain wet, escaped the complaint entirely. Take this converse fact, related by Desgenettes. Four hundred of the French "army of Egypt," reduced to a state of extreme weakness and emaciation by dysentery there contracted, embarked at Alexandria on their return towards France; were carried away, in short, from the alleged causes of their disorder. Nineteen died at the very outset of the voyage; which had, however, so good an effect upon all the rest, that before they reached Malta they were thoroughly convalescent.

The very frequent coincidence or alternation, in some places, of dysentery with intermittent fever, has given rise to the opinion that both these diseases are alike

attributable to the malarious poison. But dysentery prevails where there is no other evidence of the presence of malaria. You may recollect that when we were upon the subject of ague, I showed you that its repeated paroxysms were attended with extreme and increasing congestion of blood in the internal organs; of which congestion the tumid spleen, the ague-cake, was an effect and a token. Now whatever gorges the splenic vein, gorges its tributary, the inferior mesenteric, which carries the blood from the rectum and the descending colon. Upon such congestion of the mucous membrane, inflammation is readily engrafted; and in this indirect way dysentery may be said to result from the marsh effluvia. Ague is an effect of malaria; and dysentery is, sometimes, a sequela of ague. In precisely the same manner, dysentery is apt to supervene, in hot climates especially, upon *hepatic* congestion and disease. On the other hand, Dr. Budd has taught us how dysentery often leads to a peculiar kind of suppuration in the liver.

That dysentery is, in itself, a *contagious* malady, we have no satisfactory evidence. In its sporadic form, in this country, we never see it spread from person to person. But it is a prominent symptom in some epidemic visitations of typhoid fever, which undoubtedly is contagious. To this I am inclined to attribute the notion, formerly much more common that it now is, that simple dysentery is catching.

The remarkable decline of dysentery in this metropolis, has been contemporary with that of some other severe disorders: and is due to the same combination of causes. For nearly two centuries we have had no *plague* among us. *Agues*, formerly very rife in London, have almost disappeared. *Continued fevers*, which used to break out annually in hot weather, are comparatively unfrequent. I believe that we may trace these great blessings to an event which was regarded by many, at the time, as a national judgment; I mean the great fire that, in 1666, consumed everything between Temple Bar and the Tower. The streets and houses thus destroyed had been filthy in the extreme, close, densely crowded, and consequently most unhealthy. The impurity of the air excited, perhaps, some maladies; and it certainly predisposed those who dwelt in it to various kinds of disease, "the seeds of which (says Dr. Heberden) like those of vegetables, will only spring up and thrive when they fall upon a soil convenient for their growth." To the better construction of the houses and of the streets in the rebuilt city; to the increased means of ventilation; to the general formation of drains and sewers; to the more copious supply of water; and to the more temperate and cleanly habits of the people; we may fairly ascribe our present comparative exemption from dysentery, from ague and continued fever, which are often the parents of dysentery, and from the plague itself. In very many parts of this overgrown place there is still too much room for improvement.

The pyrexia that accompanies dysentery, sometimes begins before the local symptoms declare themselves; more frequently it succeeds their manifestation. Occasionally the fever runs high, the pulse is hard and frequent, the skin hot, the face flushed, and the tongue furred; and the patient complains of headache and thirst. But in this as in other abdominal diseases, the pulse soon becomes small and weak, the strength rapidly declines, and the temperature of the body sinks.

In acute cases the pain is often severe; but it is subject to remissions and exacerbations. It occupies the hypogastrium, or some part of the course of the colon, where there is usually more or less tenderness on pressure. The patient is tormented by a sensation as if there were some excrement ready to be dislodged, goes perpetually to the night-chair, and is irresistibly impelled to strain violently to get rid of the irritation. But the efforts are ineffectual; he discharges but little; and what is voided is either altogether a jelly-like mucus (in which case the complaint has been called the *dysenteria alba*, and the *morbus mucosus*), or more commonly it is mucous and bloody (the *bloody flux* of our old authors), mixed with films, and membranous shreds, and fragments that resemble flesh. In many of the dejections there is no genuine faecal matter at all; or the small indurated balls which I just now mentioned come away occasionally. Frequently the ejected mucus is variegated in colour; green, or black, or reddish, like the washings of meat, and horribly foetid. Sometimes pain and difficulty in making water are added; there is *dysuria*, the irritation of the rectum being reflected upon the bladder through the lower portion of the spinal cord. Sometimes the stomach sympathizes, and nausea and vomiting ensue. With all this local suffering there is a continuance of febrile distress; the patient passes sleepless,

or dreamy and disturbed nights, and is low-spirited and desponding. In the fatal cases the pulse becomes very small and rapid, the features sharpen, and the surface grows cold. Death begins at the heart.

Inspection of the dead body discloses more or less ulceration, chiefly of the large intestine. The glands that lie scattered over its surface are enlarged and prominent, looking somewhat like small-pox pustules, for which indeed they have been mistaken. They probably form the foci of most of the ulcers, which are sometimes narrow and oblong, lying across the gut; sometimes very large and irregular, with here and there islands or ridges of thickened mucous membrane. In the worst cases the whole extent and circumference of the bowel presents, internally, one irregular, confused, and tattered mass of disorganization.

[In the ordinary cases of dysentery, the morbid appearances detected after death are, inflammation with thickening of the mucous membrane of the colon and rectum; occasionally mortification and sloughing of this membrane, but more generally, in protracted cases, deep and extensive ulcerations, in the course of the transverse bands of the colon, or enlargement and ulceration of the follicles of the large intestines. In the more violent forms of the disease, especially those which occur in hot and inter-tropical climates, in addition to the inflammation, ulceration, mortification, or sloughing of the inner coat of the large intestines, there is often morbid vascularity of the mesocolon, mesentery and omentum, with adhesions of the omentum to the adjacent viscera, and of contiguous portions of the intestines to each other. The latter usually happen only when ulcers have perforated nearly all the coats of the bowels. The glands of the mesocolon and mesentery are often enlarged, sometimes inflamed, and more rarely in a state of suppuration; the corresponding portion of the intestine being usually the seat of a deep and extensive ulcer. The omentum occasionally adheres to these diseased glands, forming a band by which a portion of intestine may become strangulated and be the cause of death.]

The ulcerations of the large intestines are commonly most numerous and extensive in the cæcum and upper portion of the colon. The ileo-cæcal valve has, in some cases, been found entirely destroyed by the ulceration; the lower portion of the ileum forming then an intussusception into the cæcum, and becoming there strangulated has caused death. In a few more fortunate instances, the strangulated portion of the ileum sloughs off, after adhesion has taken place between the adjacent parts, so as to maintain the continuity of the canal, when the patient may recover. The right portion of the omentum is not unfrequently found adhering to the cæcum, and this morbid attachment gives rise to symptoms which may be mistaken for hepatic abscess. Sometimes, in the whole course of the colon, we find not more than eight or ten deep ulcerations, with sloughing, thick, abrupt, raised edges, surrounded by an extensive thickened base, into which sinuses and undermining cavities are seen to penetrate. These appearances, Mr. Twining states, (*on the Diseases of Bengal*), have reminded him of the foul ulceration at the centre of a small carbuncle; he has seen several patients die with only six or eight of these ulcers in the colon. The patients had a flushed face, restlessness, and symptoms of continued fever, which were with difficulty controlled by any course of treatment.

The last three or four inches of the ileum are generally studded with superficial ulcerations, and have a rough appearance from the enlarged follicles and glandular bodies; with this exception, we rarely meet with any traces of disease in the small intestines in those who die of dysentery—excepting in those cases in which the dysenteric symptoms occur towards the termination of protracted fevers.

In a few instances the coats of the colon are so much thickened, that, when a transverse section is made, its canal stands open like a thick leathern tube, the interior of the intestine being covered with numerous large ragged ulcers, in the intervals of which the mucous membrane is partly destroyed, and hangs in shreds. In several of these cases Mr. Twining has observed the cæcum, and lower portion of the ascending colon, to be nearly covered with a thick layer of coagulable lymph, deposited beneath the peritoneal coat, and extending a considerable distance along the iliacus muscle; in some instances an unusual amount of fat is found at the same part mixed with the lymph. In other cases, when the disease has been more protracted, the whole of the great intestines are contracted in diameter, resembling a cord, their

mucous coat being at the same time covered with numerous small superficial ulcers. In such cases, also, the internal surface of the cæcum, and of four or five inches of the colon, will now and then be of a livid red colour inclining to brown, having a fleshy appearance, as if from a growth of granulations. Mr. Twining has met with this condition but seldom, but when it has existed, he has found it to extend also to a small part of the sigmoid flexure of the colon. The patients in these cases had become much emaciated, with flat contracted abdomen, dry skin, tongue of a slate colour, glossy and morbidly clean, as if skinned; the stools consisting of an opaque dirty-brown water.

When death takes place after protracted suffering from dysentery, we often find the cellular structure at the root of the mesentery and mesocolon, and across the bodies of the lower lumbar vertebræ, deprived of its usual elasticity and pliability, and to a certain degree indurated; and in many cases entirely devoid of fat. This change is probably the result of a previous inflammation at this part—giving rise to an exudation of coagulable lymph into the interstices of the cellular substance.—C.]

When submitted to early *treatment*, and when its exciting causes can be averted, or avoided, dysentery is not an intractable disorder. Sir James M'Grigor remarks of the camp dysentery in the Peninsula, that it had two stages, which it was of consequence to note, because they required different and almost opposite modes of treatment: the inflammatory stage, and the stage of ulceration. A plan proposed by Dr. Somers appeared to Sir James so judicious, and proved so successful, in the first attacks of the pure unmixed disease, that he recommended its general adoption in the army. It was this.

First, the patient was freely bled. Immediately afterwards twelve grains of Dover's powder were administered. This dose was repeated three times, at intervals of one hour. Plenty of warm barley-water was at the same time given, and profuse sweating encouraged for six or eight hours. A pill, containing three grains of calomel and one of opium, was directed to be taken every second night; and to be followed on the following mornings by two drachms of Epsom salts, dissolved in a quart of light broth. The venesection was repeated, while the strength and the pulse permitted it, until the stools were free, or nearly free, from blood; and Dover's powder, as a sudorific, was always given after the blood-letting. When the pains were great, and attended with much tenesmus, the warm bath gave instantaneous relief. "This plan being steadily persevered in for a few days, the inflammatory diathesis of the intestinal canal, which had excited symptomatic fever throughout the general system, was found gradually to yield, and to make way for returning health."

If the disease were not cut short by this method, but advanced into the second stage, and became chronic, the most effectual remedies appeared to be laxatives, and opiates, given alternately; and combined with such medicines as promote perspiration. The abdomen should be swathed with flannel, or covered by a warm adhesive plaster. Much benefit may be obtained from the employment of clysters, if there be not too much tenesmus to admit of the introduction of the pipe of the injecting syringe. Warm starch, with laudanum in it—not exceeding in quantity a couple of ounces, lest the irritable bowel should expel it again—will sometimes afford signal relief. Or if the pain and tenesmus are so great that a clyster-pipe cannot be used—or the enema be not retained—a grain or two of solid opium inserted into the rectum, beyond the sphincter ani, will often allay the distress. The food should be farinaceous and simple; and great care must be taken during the convalescence to prevent a return to improper diet, and any fresh exposure to cold.

There is one important point in the treatment of dysentery, concerning which a striking discrepancy of opinion exists, even amongst practitioners who have had large experience of the disease: I allude to the employment of mercury as a remedy. I have no data for settling the question: but the amount of evidence appears to be against its indiscriminate use. It seems (as we might expect) to be powerful both for good and for evil. Sir James M'Grigor has probably hit the distinction which should guide us to prescribe, or to withhold, this drug. It was, he tells us, when the dysentery was complicated with disease or disorder of the *liver*, that mercury proved so highly useful: when along with the dysenteric symptoms there were present a dull pain in the hepatic region, and in the right shoulder, a yellowish brown colour of the

skin, and of the conjunctiva, and uneasiness when the patient lay in any other posture than on the right side. He adds: "In the early stage of the acute and unmixed disease, and before venæsection has been performed, mercury will aggravate the symptoms. In the more advanced stage of the disease, particularly when there is hectic fever, with extensive erosion or ulceration of the intestine, it is invariably found to hurry it on to a fatal termination."

The sporadic dysentery which we chiefly see in this country seldom requires the lancet. Leeches, however, are to be applied, in the track of the colon, wherever there is much tenderness on pressure. A full dose of castor oil may then be given; and after that an opiate. It is the practice of some physicians to prescribe laxatives and opium together; but in this complaint it is better to alternate them. Opium enemata are of service for relieving tenesmus. These remedies will be much assisted by the warm bath; by hot fomentations to the abdomen; and by such means as promote the natural secretions of the skin. If there be any reason to suspect that the portal system is gorged with blood, complete relief to the dysenteric symptoms may often be obtained by the practice which I recommended as proper in *melæna*; viz., the exhibition of five grains of calomel at bed-time, and of a senna draught the next morning, for two or three days in succession. Should the symptoms still drag on, it may be necessary to give mercury, even to the extent of making the gums tender; but it should be introduced gradually. Equal parts of *hydrargyrum cum cretâ* and of *Dover's powder*, constitute a very good combination, either in pill or powder, for such forms of the complaint. But in the milder cases of simple sporadic dysentery there is no occasion, I had almost said there is no excuse, for giving your patient a sore mouth by the lavish employment of mercurial remedies.

[There are many cases of dysentery in which the lancet, or the application of leeches or cups to the abdomen cannot be dispensed with, without endangering ulceration, thickening, or other structural changes in the mucous membrane of the intestines — by which the sufferings of the patient are prolonged and his life endangered. When we consider that the disease consists in fact in an inflammation of the lining membrane of the great intestines, proceeding on, more or less rapidly, to ulceration, and in the more violent cases to gangrene and sloughing, but little doubt can be entertained of the propriety of blood-letting in its early stage in all the more acute cases, occurring in young, robust, and plethoric subjects; even in many of the milder cases, the application of leeches or cups to the abdomen will very generally be attended with marked relief, and will materially shorten the duration of the attack. The more acute forms of the disease can be successfully managed only by the prompt, free, and even repeated use of the lancet, and the application at the same time of leeches upon that portion of the abdomen where pressure causes the most pain; and this system of active depletion must be continued, so long as fever or tenderness of the abdomen continues, more especially if the stools continue to be bloody. The use of the warm bath, in a few hours after the bleeding, and its daily repetition, will always be attended with benefit; or when there is no convenience for the employment of the warm bath, the abdomen may be enveloped with cloths wrung out of warm water, and repeated as they lose their heat, or covered with a light, soft and warm poultice. A large number of cases, if treated at their commencement by bleeding, and the warm bath or fomentations, with a full dose of *Dover's powder* at night, to ensure repose, will be very readily and speedily cured by these means alone.]

There exists much discrepancy of opinion in regard to the employment of purgatives in dysentery.—Some practitioners insisting that they should be early commenced with, and repeated at short intervals until natural stools are procured. While we cannot perceive any indication that purgatives are calculated to fulfil, which cannot be as effectually met by other less exceptionable remedies, we are convinced that the operation of purgatives cannot fail to increase, in most cases, the irritation of the bowels, and to augment considerably the sufferings of the patient. In cases where ulceration of the intestines is suspected to exist, we consider that purgatives are altogether inadmissible.

Subsequent to bleeding, as well as in those cases in which direct depletion is not demanded, the internal remedy from which we have derived the most advantage, in nearly all the forms of dysentery that have fallen under our notice, is opium in full

doses. We usually administer it, combined with ipecacuanha and nitre, from half a grain to one grain, in conjunction with a third of a grain of ipecacuanha, and three to five grains of nitre, repeated every three hours. After the discharges from the bowels have lost in some measure their slimy, bloody appearance, and though still small, liquid, and frequent, have acquired more of a fecal character, one grain of acetate of lead, with half a grain of opium and one-third of a grain of ipecacuanha, will be found in general quickly to diminish the tormina and tenesmus, and to afford marked and often very prompt relief to the patient.

Blisters are not a remedy from which any advantage can be anticipated until after the more acute symptoms of the disease have been reduced by bleeding — previously they will often do harm, but at this period they will be often serviceable. We have repeatedly observed very great relief to follow their application.

To remove the painful affection of the bladder, with suppression of urine, that so frequently attends violent cases of dysentery, Mr. Twining recommends injections of cold water into the rectum as a remedy the most prompt and certain in its effects. These injections will also be found an excellent remedy for the copious discharges of blood which frequently occur; these discharges will in general, however, be promptly arrested by an injection of ten grains of acetate of lead in six ounces of water.

In cases where the tenesmus is severe during the night, an injection of sixty drops of laudanum in two ounces of cold water will usually remain in the rectum until morning, and promote greatly the comfort of the patient.

The intense sufferings which the patients often experience from the severe tormina and almost constant and distressing tenesmus attendant upon most cases of the disease, are most effectually controlled by full doses of opium. In the more severe forms of dysentery, opium, however, is in general inadmissible until after the violence of the disease has been subdued by direct depletion. It is, when given earlier, apt to mask the more prominent symptoms, and by the calm it thus produces, may lead the physician into a false security as to the condition of his patient while a fatal disorganization is taking place in the intestines. After, however, bleeding, either general or local, or both, according to the violence of the case, has been carried to a proper extent, opium in full doses, combined with ipecacuanha and nitre, by quieting the tormina and tenesmus, will give great relief to the patient and contribute materially to shorten the duration of the most distressing symptoms of the disease. Opiate injections are certainly one of the most prompt and efficacious means we possess of relieving the distressing tenesmus, which often continues to torment the patient after the violence of the attack has been reduced. As an injection, we prefer the solid opium rubbed up with olive oil, flaxseed tea or thin starch. The tenesmus is usually dependent on ulcerations low down in the rectum; and may frequently be relieved by introducing within the anus, three times a day, a portion of the size of a hazelnut of an ointment composed of thirty grains of acetate of lead, mixed with an ounce and a half of fresh lard. When tenesmus remains after the more prominent symptoms of the disease have been removed, an injection of one drachm of acetate of lead dissolved in eight ounces of tepid water was a favourite prescription with Mosely, and in many cases it will afford prompt relief.

The utmost caution should be observed in regard to the nature and amount of the food and drinks allowed to the patient as well during the disease, as in the stage of convalescence. It is all important to keep the colon as nearly as possible empty so long as any degree of inflammation exists. — The practice followed by some practitioners of exhibiting to the patient large quantities, daily, of flaxseed tea and other mucilaginous fluids, under the notion that by so doing we soothe the inflamed intestine, is productive of far more injury than good. Whilst the disease continues the mildest diluents only should be allowed, and these but in moderate portions at a time. Even after convalescence has been fully established, the patient should be restricted to the blandest and most unirritating food, and this in very moderate quantity. Errors in diet, as Mr. Twining very properly remarks, are among the principal causes of a tardy recovery and frequent relapses.

In describing the post-mortem appearances in fatal cases of dysentery, we noticed various morbid conditions of the cæcum as of occasional occurrence; to these Mr. Twining, in his work on the *Diseases of Bengal*, (page 69), directs especial attention. No particular change in the general treatment of the case, however, is de-

manded. All that is necessary is for the practitioner to bear in mind the possibility of the occurrence of more or less violent inflammation of the cæcum, and by the most undeviating perseverance in the employment of the remedies demanded for its removal, especially the local application of leeches, to prevent the disorganization of the intestine, and the death of the patient. "When," remarks Mr. T., "a considerable enlargement of the cæcum takes place, during dysentery, it is usually first noticed about ten or fourteen days after the commencement of the attack. There is not usually so much pain as to make the patient complain, particularly of the part affected; and unless the practitioner habitually examines the abdomen of those he is treating for dysentery, he will be occasionally told of the swelling at a period of the disease when the patient is past recovery: or he will find, upon dissection, such a mass of disease and inflammation, with induration, in the right iliac region, that he will be surprised how it could have escaped his notice during life. Excepting in fat persons, the rounded, doughy, inelastic tumefaction of the cæcum is easily detected by examination with the hand; and, in fact, is often visible on inspection.

The inflammation of the cæcum is to be treated by the daily application of leeches to the right iliac region; the discharge of blood being promoted by warm fomentations or poultices. When by this means the morbid sensibility is removed, and the tumefaction considerably reduced, a blister should be applied and kept open by some stimulating dressing. Mr. Twining recommends as a means of reducing the remaining induration, "a course of Plummer's pill, and extract of colocynth, at night, followed by the compound power of jalap in the morning." "We must remember," he adds, "that no part of the disease can be left unsubdued without hazard to the patient's life."

In chronic dysentery, there is frequent watery purging, attended often with tormina, and very generally with tenesmus, by which the patients are much distressed and soon greatly exhausted. The discharges are mostly of a pale gray colour, often mixed with more or less of a mucous or white slimy substance, and not unfrequently portions of undigested food. They sometimes consist of a copious paste-like brown mass, in a state of fermentation; occasionally, they are frothy, with a whitish or pale grey sediment like a mixture of chalk and beer; in a few very protracted cases, the discharges are very frequent and copious, quite watery, and varying in colour, from a dark-brown to a bright-orange, or from a dirty yellow to a pale straw. They are in some cases inodorous, or nearly so; in others they have a peculiar sickening odour, and in others again they are more or less fetid. Blood is rarely present in the stools, and the patients suffer from fever only in those cases in which there is extensive disease of the mesenteric gland—when they occasionally present a well-marked fever of a hectic character. The abdomen is generally flat, inelastic, and somewhat retracted; occasionally, however, tympanites is a troublesome symptom. The skin is usually dry, shrivelled, and desquamating, and of a dirty brown or sallow hue.

The treatment of chronic dysentery differs but little from that proper in cases of chronic diarrhoea. During the continuance of the disease, and for some period after it has been removed, the patient is to be strictly confined to a mild, unirritating diet, taken in very moderate quantity, and, for drink, to some bland mucilaginous fluid, as toast or rice water—and even this only in such portions as are necessary to allay his thirst. He should be shielded from the influence of cold and damp, and all sudden atmospherical changes, by flannel next his skin and appropriate clothing, and by a careful attention to the temperature of the chamber he occupies.

Leeches or cups to the abdomen will frequently be found of service—but their use will require judgment, and a close scrutiny of each case in order that any latent or chronic inflammation may be at once detected. Whenever indurations within the abdomen can be felt, and they are attended by morbid sensibility on pressure, or the least degree of febrile excitement, the application of leeches or cups should not be neglected. When the indurations are without sensibility, or after this has been removed, blisters are generally very important remedies. The daily use of the warm bath and warm fomentations to the abdomen assiduously employed, are means from which, in cases of chronic dysentery, the most decided advantage will invariably result. Small doses of ipecacuanha combined with extract of hyosciamus and the blue mass, and repeated every three hours, with a dose of Dover's powder at night, will generally be found to aid very effectually in restoring the capillary circulation to its

healthy state, in quieting the morbid irritability of the mucous membrane of the intestines, in moderating the profuse morbid discharges, and in rendering the stools less frequent and of a more natural character. The balsam of copaiba and turpentine will often be found of equal advantage in many cases of the present affection, as in chronic diarrhœa. In regard to the period proper for the employment of astringents, and the articles of this kind that are best adapted to the disease, the same remarks may be repeated as were made in our note on chronic diarrhœa. — C.]

The fluxes of which I have hitherto spoken have all consisted in an immoderate discharge of some of the usual contents or secretions of the alimentary tube, in an altered and unnatural state. But matters are sometimes voided from the bowels totally unlike any of the healthy discharges. *Adeps* is not an intestinal excretion; yet it is sometimes passed, in great abundance, by stool. Many unquestionable instances of this are on record, both in ancient and in modern literature. I have not seen more than one, and therefore have but little to say upon the subject; yet I must not pass it over altogether. A certain quantity, sometimes it has been a *large* quantity, of oil, of liquid fat, has been poured forth, in a sort of diarrhœa. Sauvages was aware of the disorder, and calls it, in his *Nosology*, *diarrhœa adiposa*. In a paper in the *Medico-Chirurgical Transactions* upon this affection, Dr. Elliotson refers to an example of it described by Tulpus, in which a woman discharged every day, for fourteen months, a considerable quantity of yellow fat, that lay upon the feces like melted butter. When voided into a vessel of water it floated, like oil, upon the surface; and when cold it assumed the consistence and appearance of fat. Like fat, it was very inflammable, and burned with a bright flame. With all this there was no kind of distress, nor any wasting of the body; and the patient was in excellent health sixteen years afterwards. Dr. Elliotson had a case of this kind under his own care. The man had also diabetes and phthisis. The symptoms were precisely the same as those described by Tulpus. Dr. Prout and Dr. Faraday analyzed portions of the adipous matter, and they pronounced it to be genuine fat. Mr. Lloyd, of St. Bartholomew's Hospital, has given us the details of a case in which the evacuation of grease was associated with jaundice. The excretion looked like melted fat, but when cool had the consistence of butter. It swam on the surface of water, melted at a moderate heat, and burned readily. In this instance the head of the pancreas, and the duodenum, were involved in a mass of scirrhus disease. And this is a very curious fact: for Dr. Bright also states that in three different persons, each of whom he had known to pass fat from the bowels during life, and whose bodies he had the opportunity of examining after death, he found scirrhus disease of the pancreas, and fungous disorganization of the duodenum.

The remarkable coincidence, occurring so often, of cancerous disease of the pancreas and duodenum, with these fatty profuvia, is the more memorable, because it falls in with the theory (propounded by Dr. Claude Bernhard, and fortified, as he thinks, by the results of experiments on animals) that one important purpose of the pancreatic fluid is to promote the absorption of fatty matters, by forming with them a permanent emulsion, capable of entering the lacteals. This theory, however, is far from being generally accepted as a proven theory: and even if it be true, it will scarcely account for the *large quantity* of fat which has been excreted in some of the recorded cases.

The single case of which I have had any cognizance is that of a tradesman now living in my neighbourhood, who from October, 1843, to August, 1844, suffered frequent attacks of this kind. First, he had severe pain in the epigastrium and right hypochondrium, with little or no fever, nor acceleration of pulse. Occasionally, when the pain was extreme, it was accompanied by nausea and vomiting. After the pain came jaundice and white stools; and, lastly, a discharge from the bowels of a quantity of white fragments, looking exactly like coarse chewings of walnuts. Sometimes much larger lumps of the same sort were voided. These lumps and fragments were found to be composed of adeps. They floated, many of them at least, and for a while, upon the surface of water. They felt greasy; and burned, like fat, with a flame.

After this series of symptoms the patient would recover perfectly, save that the attacks reduced his strength somewhat. They occurred about once a fortnight, so that he had about a score of them in ten months.

The symptoms resembled those which declare the passage of gall-stones; and upon one occasion two concretions were detected among the fragments of fat. One of these was yellowish; the other black, like a cinder. Once, also, a considerable portion of what seemed membrane came away. My neighbour, Mr. Francis E. Hicks (to whose kindness I am indebted for the opportunity of seeing this patient), is of opinion that a cyst, which secreted the fat, existed in the liver, and discharged its contents periodically; and that the membranous fragment was a part of this cyst.

Projecting a little into the epigastric notch, I could feel what appeared to be the edge of the left lobe of the liver. This spot was slightly tender, and dull under percussion.

I had previously been consulted by the same patient for a chronic and obstinate cough, which after harassing him for three or four years, ceased suddenly, upon the occurrence of the first of these strange seizures. On that occasion he voided at least a quart of the fat. Since August, 1844, he has experienced only two or three slight threatenings of an attack; but lo, his old cough has returned.

Mr. Hicks tells me that a female relative of his own laboured for four or five years under a perpetual dry cough, which was most distressing to herself, and very irksome to the hearers of it. A pint of fatty matter was then passed by stool, and the cough ceased at once, entirely and permanently. This happened more than twenty years ago.

All that we know of the disease seems to amount to this: that it is not a *common* complaint; and that it is not necessarily a *fatal* complaint. Persons who have passed great quantities of fat in that way have lived in good health for many years afterwards. Yet though not *necessarily* fatal, it has frequently been found associated with incurable malignant disease in the duodenum and pancreas. Dr. Prout informs us also that in cases in which a similar oily fluid has been passed through the *urethra*, the *kidneys* have been found in a state of organic malignant disease.

With respect to the treatment in such cases, all the hints I can give you are such as are furnished by the two following facts:—

Mr. Howship, in his book on morbid anatomy, mentions the instance of a lady who was affected with this diarrhœa adiposa, and parted with vast quantities of fat; and who was cured upon the principle of *similia similibus curantur*, for she recovered after swallowing a pint of sweet oil. And Dr. Elliotson, acting on this hint, gave *his* patient, who was labouring at the same time under diabetes, a quarter of a pint of olive oil; and the voiding of fat greatly diminished from that time, and soon ceased entirely.

Whether these were really cures, or whether they were coincidences, is a question which we want larger experience to help us to determine.

When I was speaking of the causes of enteritis, I adverted to the presence of *foreign substances*, as they are called, in the bowels, and to *intestinal concretions*.

There are some points connected with these subjects which I had not then leisure to pursue, but of which you ought not to be ignorant.

Intestinal concretions are very common in some of the lower animals—in horses and oxen especially. Most of you have seen, I dare say, immense intestinal calculi of this kind, and great numbers of them, in the Museum of the College of Surgeons. The old remedies called *bezoars* were of the same nature.

They occur also, these intestinal calculi, in the human entrails, and in various parts of them: chiefly, however, in the cæcum and large intestines, but sometimes in the stomach: indeed, very large ones have occasionally been met with in the latter organ. Bonetus describes one which weighed nine ounces, and was as big as a hen's egg. Generally they are few in number in the same person; one only, perhaps, exists; or there may be two, or three. Yet, as many as thirty have been found together in the stomach in one case: and in another case nine. One of the Monros of Edinburgh (Monro primus) detected twelve in the colon of a boy, during life, by the touch. Monro secundus took a concretion that weighed four pounds from the colon of a woman. They have been known to measure as much as eight inches in circumference. In the twenty-fourth volume of the *Edinburgh Medical and Surgical Journal* is an account of one long one, or, perhaps, of three that had become united together, weighing twelve ounces. Dr. Turner, of Keith, has published the case of a man,

named Gordon, who, in May, 1841, passed fourteen large intestinal concretions, and recovered completely. At the close however of the year 1843, he began again to suffer, as he had previously suffered, from costiveness alternating with diarrhoea, and from pain and distension of the abdomen. In September, 1847, he one day voided three concretions, each as big as a hen's egg; and on the two following days, fifteen more, varying in size from that of a partridge's egg to that of a filbert. So that in all there came from this patient's bowels thirty-two of these hard bodies.

Now what are these substances, and how do they get there? What is the pathology of the malady? Why, they seem to be formed, in many instances, by the deposit of saline particles, intermixed with animal matter, upon and around some accidental nucleus which has entered the alimentary canal, and there stopped. A gall-stone may form the nucleus: the centre of the calculus has several times been found to consist of pure cholesterine. Those matters over which the gastric juice has no power, and which pass the pylorus unchanged—such as the stones of fruit, husks of grain, many unbroken seeds, portions of bone, and the like. Other of these intestinal concretions are evidently composed of a mass of short fibres, matted, or interwoven together, after the manner of *felt*. These calculi have a somewhat soft and velvety feel, yet are too hard to be much compressed. Sometimes they involve a nucleus, and sometimes they do not. Their composition has been discovered in rather a curious manner. Mr. Clift, who, as you know, had long the main charge of the Hunterian Museum, fancied, after attentively examining some of the specimens there collected, that they might be formed somehow of the beards of oats; and the late Dr. Wollaston, at Mr. Clift's suggestion, I believe, undertook to analyse them somewhat more rigidly; and he found that Mr. Clift's conjecture was well-founded. If you have ever looked closely into the structure of an oat which has been separated from its husk, you may have noticed that one end of it is formed somewhat like a tiny brush; made up of very minute needles or beards. Dr. Wollaston found that these ends were identical in their shape and composition with the fibres of the intestinal concretions.

The accuracy of the result of this analysis is singularly confirmed by the fact that this particular kind of intestinal calculus is almost peculiar to the bowels of Scottish people; among whom, as you know, oats form a very common article of diet, in the shape of oat-meal. The man Gordon, whose case is related by Dr. Turner, lived chiefly upon this meal. Even after his first deliverance, neglecting the advice which had been given him, he recurred to his old habits of diet, and two-thirds at least of his solid food consisted of oat-meal. Sections of the concretions presented the appearance of concentric layers, arranged round a thin shell of phosphate of lime. They were mainly composed of hairs of the *caryopsis*, and fragments of the envelopes of the oat: and they were studded internally with minute crystals.

Concretions of the same species have also been found in the intestines of Lancashire persons; and they also use oat-meal a good deal as food. Mr. Children gives an account of some in the Philosophical Transactions for 1822. The fibres were cemented together by mucus; and the concretions contained albumen also, phosphates of lime and soda, and common salt.

I mentioned formerly the danger which attends the incautious or excessive use of magnesia, whether for stomach complaints or for urinary disorders. When this substance is taken habitually, and when due care is not used to ensure its habitual expulsion from the intestines, it is liable to accumulate and congregate there, especially in the cæcum and colon. Large masses of this kind have been met with, composed almost entirely of carbonate of magnesia.

And the habitual use of any other indigestible substance may have the same ill consequence. The seeds of figs; unbruised mustard seeds, which (as I mentioned before) are taken daily by some persons; the woody knots found in certain pears; all these have been known to form the material of concretions, or of hard injurious masses in the bowels.

Now concretions of this kind come at length to produce symptoms by the pressure and distension they occasion, by the ulceration to which they sometimes give rise, and, above all, by the obstacle they oppose to the passage of the contents of the intestines. They generally cut the patient off by exciting inflammation.

But they may exist for a long time without producing any definite symptoms, or

any serious injury. And when symptoms do arise, or when we ascertain that such concretions have formed, we are often at a loss for a remedy. From the *colon* we may hope at last to dislodge them: by mechanical interference when they are near the outlet: by frequent injections of warm water, or soap and water, whereby they may be softened or broken down, and washed out, when they are beyond the reach of the finger, or of instruments passed into the rectum. In Gordon's case no means were employed till a late period of his disorder. The concretions were voided, with immediate relief from severe suffering, after the daily injection of emollient enemata, and the free use of opiates, which seem to have had the effect of relaxing muscular spasms of the irritated intestine. The patient again regained a state of robust health.

We have an illustration of the patience of the alimentary canal under the presence of these masses, in what often happens when foreign bodies of some magnitude are swallowed, and lodge in the tube.

In one of the earlier volumes of the *Medico-Chirurgical Transactions* you may read the history of a celebrated knife-eater. A sailor, in a drunken bravado, swallowed a clasp-knife. This was followed by no immediate bad consequences, and he used to brag of the feat he had performed. And afterwards, either to satisfy the scruples of those who did not believe his assertions, or else for the sake of rewards which some people were thoughtless or cruel enough to offer, or else to win wagers, he stupidly repeated his folly, till he had swallowed (I think) thirteen knives of various kinds and sizes. They killed him at last, and their remains were found in various parts of the alimentary tract. But he had no serious symptoms for some time.

Mr. Wakefield has given us an account of a culprit, confined in the Cold Bath Fields Prison, who had swallowed seven half-crowns before his incarceration. One day out they all clattered into the pan of his night-chair.

I saw a prisoner myself, some time ago, in the Penitentiary at Milbank, who after some sickness, and tenderness of the belly, voided a half-crown from the rectum. This was in November, 1839. He had swallowed the piece of money two years and a half before — viz., in March, 1837; and, until within a week of his passing it, he had enjoyed excellent health.

Before I proceed to any other of the viscera of the abdomen, I may as well take such notice as the nature and limits of these lectures requires and admit, of the subject of *worms*; in which subject the intestinal canal is more concerned than any other part of the body. It seems a strange, as it is a somewhat humiliating fact, that the human body should furnish food and a habitation for many of the inferior creatures; not only after death, but while it is yet alive. The parasitic animals which thus prey upon man have been much studied from time to time, and especially of late, in their relations to natural history: and some of the facts that have been ascertained respecting them you ought to be acquainted with. But I shall pursue the subject no further in this place than concerns us as pathologists and physicians. Its natural history will, no doubt, be fully taught you by the professor of comparative anatomy.

First, then, it is a notorious fact that numerous parasites do crawl over our surface, burrow beneath our skin, nestle in our entrails, and riot, and propagate their kind, in every corner of our frame: producing oftentimes such molestation and disturbance as require the interference of medicine. Nearly a score of animals that have their dwelling-place in the interior of the human body have been already discovered and described: and scarcely a tissue or an organ but is occasionally profaned by their inroads. Each, also, has its special or its favourite domicile. One species of *strongyle* chooses the heart for its place of abode, another inhabits the arteries, a third the kidney. Myriads of minute worms lie coiled up in the voluntary muscles, or in the areolar tissue that connects the fleshy fibres. The *guinea-worm* and the *chigoe* bore through the skin, and reside in the subjacent reticular membrane. *Hydatids* infest various parts of the body, but especially the liver and the brain. A little *fluke*, in general appearance much like a miniature flounder, lives, steeped in gall, in the biliary vessels. If you squeeze from the skin of your nose what is vulgarly called a maggot—the contents, namely, of one of the hair-follicles—it is ten to one that you find in that small sebaceous cylinder, several animalcules, extremely minute, yet ex-

hibiting under the microscope a curious and complicated structure. Even the eye has its living inmates. But it is, I repeat, in the alimentary tube that we are most apt to be plagued with these vermin.

Independently of minute scientific divisions into genera and species, there are some broad lines of distinction between these creatures. Thus, some kinds of worms occupy, as I have said, the interior of our bodies; these are called accordingly *entozoa*: some dwell externally, and are named *ectoza*; or, more properly perhaps, *epizoa*.

There are five sorts of intestinal worms, sufficiently common to make it likely that you will meet with some or most of them in your future practice. I shall, on that account, direct your attention first of all to them.

1. A frequent tenant of the human intestines is the round worm, so like in shape, size, and general appearance to the common earth-worm. It is from this species, no doubt, that the whole class are called *worms*. This round worm is often erroneously termed a lumbricus. It is a species of *ascaris*, and it has been named by naturalists *ascaris lumbricoides*—the *ascaris* that is like a lumbricus. Oxen, and hogs, are subject to this entozoon.

2. The *ascaris vermicularis*; or the *oxyuris vermicularis*. These animals resemble slender maggots rather than worms. They are often called simply *ascarides*; or, in the vernacular, *thread-worms*; and to the naked eye they look very like bits of white thread.

3. The *tricocephalus dispar*; also a small worm, but longer than the last; its vulgar denomination is accordingly the *long thread-worm*.

4 and 5. Two species of *tænia*; long, flat, articulated animals, resembling pieces of tape. The *tænia solium*, or common tape-worm of this country; and the *tænia lata*, or broad tape-worm. Many of the inferior animals are infested each with its own peculiar species of tape-worm. From their band-like appearance, the *tænia* are also styled *cestoid* worms.

Of all these I proceed to mention a few more particulars.

The *ascaris lumbricoides*, or round worm, is, I say, very like the common earth-worm, and used to be thought identical with it. It runs from five or six inches to about a foot in length, and it is of a reddish brown colour, with a tinge of yellow. The female worm (for they are of both sexes) is much more common than the male, which is smaller also, and may be distinguished by a curved state of its tail, and by the genital organs. Sometimes young ones are met with, about an inch and a half long.

I shall not go into any minute description of the anatomy of these worms. You cannot mistake them, except for earth-worms; and the points of distinction between the two, when known, are easily perceived. The earth-worm, then, is redder than the intestinal worm, and less pointed at its two ends. The mouths of the two differ much. That of the earth-worm is a short longitudinal fissure, or slit, placed on the under surface of its small rounded head. In the *ascaris lumbricoides*, the mouth is situated at the extremity of the worm, is of triangular shape, and is surrounded by three tubercles.

It is curious that similar differences, only reversed, exist in respect to the other aperture of the alimentary canal, the anus. In the earth-worm this is terminal, at the very end of the cylinder: in the *ascaris* it is a transverse slit *near* the extremity, and on the under surface of the animal.

Again, the earth-worm has rows of little projections, like bristles, upon its under surface; feet they may be called, for they appear to serve the purpose of locomotion. In the parasite there is nothing resembling this.

By attending to these plain marks, you may avoid being deceived by impostors, who pretend that they are afflicted with worms, and to prove their case bring you an earth-worm or two in a bottle.

The *habitat* of these worms is in the small intestines. They may, and do, pass upwards into the stomach, or downwards into the large bowel: in either case they are generally soon voided. Sometimes they are vomited up: but they have been known to *creep* into the œsophagus, and thence into the nostrils. Andral states that he saw a case in which a child was strangled by one of these worms, which had turned back

and become entangled in the larynx. They have been found also in the excretory ducts of the liver. This Andral has witnessed; as has also Dr. Baron in this country.

FIG. 112.



Ascaris lumbricoides: *a*. Anterior extremity; *b*. Posterior; *d*. Vulva; *e e*. Longitudinal lines.

FIG. 113.

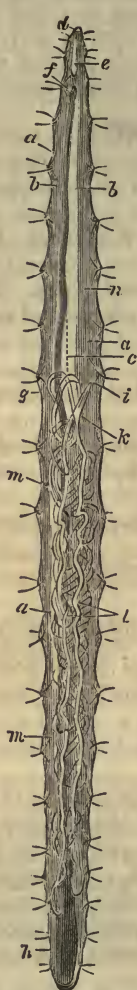


FIG. 113.*

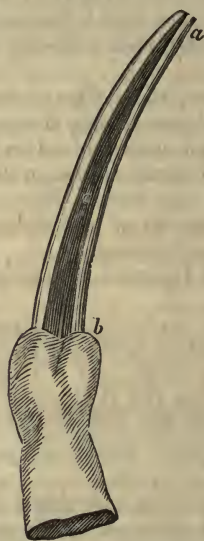


FIG. 115.



Head and mouth of the ascaris; *a*, *b b*. Tubercles; *c*. Oval aperture.

FIG. 114.



Penis of the *ascaris lumbricoides* magnified; *a*. Extremity; *b*. Base.

FIG. 116.



Ascaris vermicularis; *a*, head; *b* tail; *, natural size

FIG. 113.—Organs of the female ascaris: *a*. External tunic; *b b*. Muscular fibres; *d*. Mouth; *c*. Esophagus; *f*. Alimentary tube; *k l m n*. Generative organs.

FIG. 113.*—Organs of the male ascaris: *d*. Mouth; *e*. Esophagus; *h h*. Generative organs; *f*. Intestine; *g*. Penis.

It was formerly thought that these animals were capable of perforating the coat of the intestine: but that opinion is now generally exploded. They do not appear to

possess the means, if they have the inclination, to bore through. What gave rise to this notion was the circumstance of their sometimes passing out of the bowel, through ulcerated or other openings, into the peritoneal sac; or into the vagina or bladder; or out of the body through hernial apertures.

The number of these worms existing at the same time in the same person is very variable. The late Dr. Hooper mentions a girl, eight years old, who voided upwards of 200 in the course of one week. An instance is recorded of a soldier who passed 367 in six days. Another patient got rid of 460 in a fortnight.

Fifty or sixty have been found in the same dead body. They often lie in packets. The corresponding portion of mucous membrane has in some cases been red, in others quite natural. Sometimes two worms are met with; sometimes one only. So that we cannot infer with certainty that because one such worm has been voided, more remain behind; although that is always probable.

This worm is more common in the early periods of life than afterwards.

The other species of ascaris, the ascaris vermicularis or thread-worm, resembles the former in some respects, but differs from it remarkably in size. Here also the female is longer and larger than the male; the one being perhaps half an inch in length, the other scarcely two lines, and very slender.

The thread worms live principally in the rectum, and sometimes are collected there in vast numbers; thousands: and they pass out, or are ejected, matted together with mucus in the shape of balls, or entangled in portions of excrement. Sometimes they emerge of their own accord, and crawl about the neighbourhood, getting into the vagina in females, and even into the urethra, and causing intolerable irritation, itching, and distress.

They are seen, when recently expelled, to be very lively; moving their anterior extremity briskly and continually. To this restlessness and activity the animal owes its name, which is derived from the Greek word *ασκαρίζειν*, to leap. The Germans call it *springwurm*.

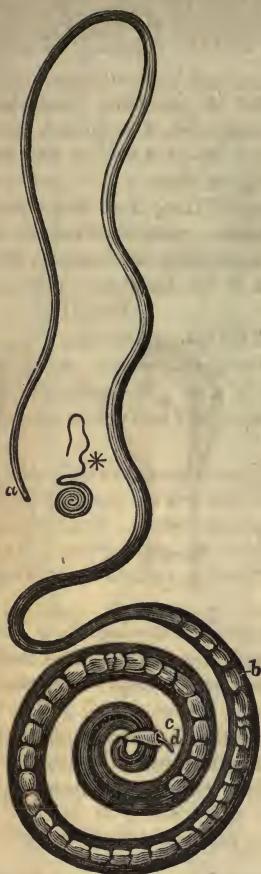
This worm also belongs chiefly to infancy and childhood. It does sometimes infest adults; but generally as the patient grows older the animals cease to trouble him, whether curative means are employed or not. Bremser, however, knew a person eighty years old, who was nearly killed by them.

The third kind of these round worms is the *long* thread-worm; the *tricocephalus* dispar. It is from an inch and a half to two inches in length. One extremity, that to which the head belongs, is extremely fine and small; and then suddenly bulges out a thicker body. The thinner portion is about twice as long as the thicker. Its name is derived from this variation of size. *τριξ*, a hair, and *κεφαλη*, the head; the portion to which the head is appended being as fine as a hair. At one time the head was mistaken for the tail, and then the animal was called *tricuris*, from *τριξ*, and *ουρα*, the tail. The thicker or body part is rolled up in a spiral form, especially in the male, the female being straighter. This worm is of a white colour, unless tinged by its food. It also affects the large intestine as its place of abode; but the opposite end of that gut, the cæcum, is its favourite spot. It is sometimes met with in great numbers, each attached to the mucous membrane by its head; the body hanging loose.

Although generally overlooked, it is said to be extremely common, and to occur in most bodies. I have seldom seen it; but then I have never hunted for it. It infests the dog, the fox, the monkey, and other mammalia.

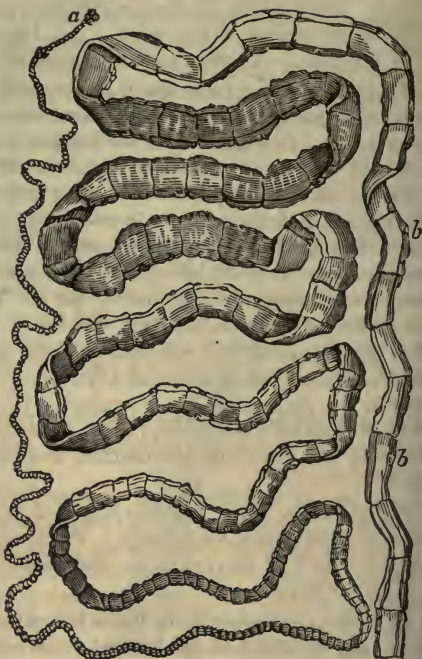
This species of entozoon attracted a good deal of attention nearly a century ago; it being then first observed in Germany during the prevalence of an epidemic fever, which was characterized by profuse mucous diarrhœa. Rœderer and Wagler have given an excellent account of this disorder, under the title of *morbus mucosus*. It was thought to have been excited by these worms, which were found in abundance in the cæca of the dead. This opinion must have been erroneous, for the animals had been noticed in other places, long before; and they produce, in general, no appreciable inconvenience.

FIG. 117.



Trichocephalus dispar—*Trichuris*:
 *a. The head; b. Intestines; c.
 Sheath; d. penis; *, natural size.

FIG. 118.



Tænia Solium: *a. Head; bb. Lateral orifices.

The two *tæniæ* are more formidable beasts. With a strong general resemblance between them, there are marked particular distinctions.

The *tænia solium*, or common tape-worm, has a minute hemispherical head, which is furnished anteriorly with a double circle of little hooks, and behind these lie four suction disks, whereby it adheres to the inner surface of the intestine. Its body is long and flat, of a whitish colour, composed of many pieces curiously articulated together. The articulated pieces are quadrilateral. Very short, small, and indistinctly marked in the creature's neck, they become gradually larger and square as the distance from its head increases, and at length are longitudinally oblong. So that the worm is narrow and thin at its anterior extremity; one-third or one quarter of a line perhaps in breadth: while at its broadest part it may be from three to six lines wide. The young *tæniæ* seem to be merely wrinkled, but they also are really articulated. The segments of the animal, or the *joints* as they are called, have foramina on their margins, leading to ovaries within. The foramina, which are very conspicuous, are placed alternately on the one side of the animal and on the other: on the right edge of one joint, on the left of that next to it. This arrangement is, however, subject to occasional irregularities. Each joint is let in, as it were, to that immediately in front of it; and the connexion between them is not very firm. It is less firm in proportion as the animal is older, and as we approach its posterior extremity: so that the segments are apt to come away, by stool, separately. They have somewhat the appearance of the seeds of cucumbers or gourds; and the parasites, for that reason, a

sometimes called *cucurbitine* worms. Blumenbach and others have supposed that each articulated piece was a distinct worm; but that is not the case. The head of the animal, and the way in which it grows, and multiplies its kind, forbid this belief.

Within each joint—within at least each of the larger and hinder joints—is contained a complicated male and female apparatus, capable of producing thousands of fertile ova: and the spontaneous separation of these riper segments appears to be a natural provision for disseminating the minute eggs. Meanwhile, as the animal shortens by thus shedding its hindermost joints, some of those which are anterior divide into two by a transverse fissure, which two, after attaining a certain size again divide, and, in this way new joints are formed, and recede gradually from the head. But at a certain distance from the head, the divisions and subdivisions cease, and the whole nutritive power is expended in the development of the organs of generation: and at length ova begin to fill the uteri of the joints. Such is the process (as I learn from Professor Owen's lectures) which has been actually observed by Dr. Eschricht of Copenhagen, in a species of tape-worm (the *Bothriocephalus punctatus*) extremely common in a sea-fish called *Cottus Scorpius*; and it may be presumed that the increase and generation of the human tape-worm proceed in a similar manner.

You will observe that this mode of growth and of multiplication is closely analogous with that of many seed-bearing plants.

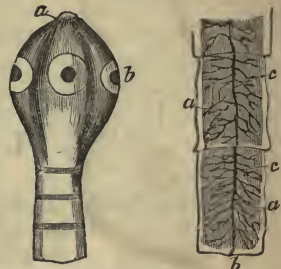
Specimens of this worm are preserved, upwards of twenty feet in length. Much exaggeration seems to have existed formerly about its size. It has been said to measure 150, and even 300 feet. In all probability separate portions of several worms have been estimated as forming parts of one and the same worm. There is one case well authenticated (it is cited by Bremser from Robin) in which a tape-worm was found to extend from the pylorus to within seven inches of the anus; adhering firmly to the mucous membrane all the way. The animal has the power of motion. Its movements are felt by the patients, within them. When recently expelled, and placed in tepid water, it may be seen to shorten itself; nay, portions protruding many feet from the anus have been known to draw themselves back again.

This kind of worm is more frequent in adults than in children: yet it is sometimes met with even in the foetus. It has been badly named *ver solitaire*, for it is not always single. It is not only found in company with different worms, but also with others of its own species. Its natural place of abode is the small intestines: but it extends sometimes into the large, and sometimes into the stomach. Vandoverer declares that after an emetic one of his patients vomited forty Dutch ells of the worm, and might have got rid of more "if he had not been afraid of puking out all his guts, and for that reason bit the worm off."

The *tænia lata*, or broad tape-worm, has often been confounded with the *tænia solium*: yet there are striking differences between them: respecting which, for all practical purposes, it is enough to say that the heads (as viewed through a microscope) are very dissimilar, the head of the *tænia lata* having neither a coronet of hooks, nor a circle of suckers; that its joints are shorter and

roader, and adhere together in a different manner; and that the pores leading to the viducts are situate, not on the edge of each joint, but in the centre of its flat

FIG. 119.



Tænia solium. Head and joints.

FIG. 120.



Tænia lata. a. Marks the generative orifices in both. from Owen's lectures.

FIG. 121.



Tænia lata.

surface. This variety is not so easily broken across as the former; and therefore its segments are less liable to be voided in a separate form. It is probably shorter also than the *tænia solium*. Fifteen feet have been supposed its average length. Marvellous stories, however, are told on this head. Boerhaave declares that he effected the expulsion of one, from the bowels of a Russian, which was 300 ells long.

The geographical distribution of these two species of *tænia* forms a curious part of their history, and throws light upon the long controverted question of their origin. In England, Holland, Germany, and Denmark, the *tænia solium* is common, and the *tænia lata* very rare. In Russia, Poland, and Switzerland, it is just the reverse; the *tænia lata* prevails, the *tænia solium* is seldom seen; while in the French provinces adjoining Switzerland the one species is nearly as frequent as the other.

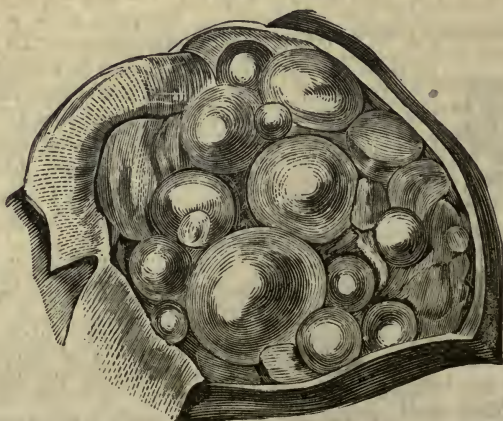
LECTURE LXXIV.

Entozoa continued. Hydatids. Trichina Spiralis. The Guinea-Worm. Strongylus Gigas. Origin of Entozoa. Question of Spontaneous Generation. General symptoms of the presence of Intestinal Worms. Particular symptoms, and remedies, of the common Round Worm, of Thread-Worms, of Tape-Worms.

AT our last meeting I gave a summary description, sufficient, however, for our purposes as medical practitioners, of the five kinds of *vermes* which are the most common parasites of the human intestinal canal. There are yet a few more of these entozoa which are curious and interesting enough to deserve a brief notice.

Hydatids—animals like bags or bladders of water—are of no unfrequent occurrence in several of the internal organs of the body. They are also called *acephalocysts*, headless bags. In size they vary from the bigness of a pea, to that of a swan's egg. They look like, or rather they are, spherical membranous bladders, filled with a thin colourless liquid which holds in solution a large quantity of common salt. Floating in this liquid, and therefore enclosed within what must be considered the primary

Fig. 122.



Acephalocyst.

hydatid, there are usually found smaller ones, of various sizes, sometimes few in number, sometimes in countless multitudes. This seems a consequence of the pecu-

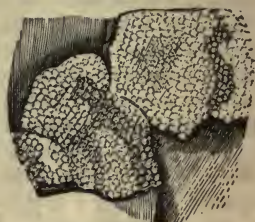
liar mode of propagation of these animals, which is by what is called *gemmation*. The wall of the cyst is laminated, and the young hydatids bud from between its layers. In the species that infests the human frame they are born into the cavity of the parent; in some other species they are detached externally. We find, then, a parent bag, containing other smaller bags, which again are pregnant, as it were, with their own offspring, the grand-children of the primary cyst: and so on, somewhat after the manner of a nest of pill-boxes. The primary cyst itself lies in close contact with, but does not adhere to, an external sac, formed apparently by the pressure of the enlarging hydatid upon the texture of the organ in which it is imbedded. These hydatid tumours are more common in the liver than in any other single organ. I lately mentioned the case of a woman, Harriet Baldwin, who died in the Middlesex Hospital, and whose liver contained thousands of these globular bodies. The enlarged gland had completely sealed up, by its pressure, a portion of the inferior cava. They are found also in the lungs, in the spleen, in the mesentery; more seldom in the brain, and in the kidney; and occasionally in other parts of the body. I have called these bags of fluid, *animals*; but perhaps they should rather be regarded as the dwelling-places of much smaller creatures. It was formerly conjectured that they were not parasites nor distinct animals in any sense; but merely certain of those primitive nucleated *cells*, from which the microscope asserts that all the varied tissues of the body are originally formed: cells rendered gigantic and monstrous by some erring or morbid action of the vital forces. Except in size, the cell and the hydatid were held to be alike; alike in shape, alike in the mode of growth and multiplication. But this view of the matter is inconsistent with the result of more recent and more extended observation.

Within several of the transparent hydatids which were taken from the liver of the woman Baldwin, a number of small, opaque, white grains, were visible. These were examined by Mr. Tomes and myself, under the lens of his powerful microscope. They were plainly minute animals: baglike, with an orifice or mouth which, in some instances, protruded a little from the bag, in others was evidently contracted and drawn inwards. Around this orifice was arranged a circle of small, flat, hook-like rays, somewhat resembling a vandyke collar. Many of these rays, or spines, as they have been called, were detached, and lying loose in the surrounding liquid. Being very ill-informed in this department of natural history, I paid less attention to these creatures than they deserved; fancying indeed that they might be common and well-known, or rather that they were juvenile hydatids. I first became aware of my mistake, through perusing a paper, by Mr. Curling, in the twenty-third volume of the *Medico-Chirurgical Transactions*; where he more minutely describes precisely similar phenomena. The subject has since attracted the attention of many observers, and it appears certain that in almost every hydatid are included more or fewer of these animalcules; which from the spines encircling their heads have been named *echinococci*—hedgehog mites. They have been accounted parasites of the second order, entozoa of an entozoon; but from their general presence it seems more probable that the acephalocysts are mere nests or habitations, formed somehow by, or for, the small indwellers. Professor Owen would have them called *echinococco-cysts*.

Minute in their origin, hydatids may enlarge and multiply till the tumour formed by them attains an enormous size, and at length destroys life by its bulk and pressure; or in other ways which I may have occasion to point out in future. Of course the direct consequences of the pressure will depend much upon the parts occupied by the hydatids. You may readily imagine what kinds of symptoms are likely to ensue when they are lodged within the abdomen; within the less yielding thorax; within the unyielding skull.

We can seldom be sure that hydatids exist within the body, until we see them; nor, if we knew of their presence, could we propose any rational method of cure. It has been fancied that a galvanic current, or an electric shock, passed through the organ containing these creatures, might kill them, and so at least prevent their increase: or that they might be poisoned by alcoholic potations, or by drugs that are

FIG. 123.



Young acephalocysts.

not seriously prejudicial to man, such as mercury, iodine, turpentine. But these, I fear, are mere dreams of our baffled art. They sometimes open a way for themselves to the surface, and escape through an ulcerated outlet: and sometimes they are let out, to the surprise perhaps of the operator, who only knew that he was dealing with an abscess which required puncturing. Mr. Arnott put a lancet into a fluctuating tumour in the epigastrium of one of my patients; very offensive pus issued, with the shrivelled skins of sundry defunct hydatids. The cyst was situated, I believe, in the liver. Although the orifice was slow to heal, the patient ultimately got well. Sometimes the whole colony perishes while yet hid in its dwelling-cave, all the enclosed hydatids losing their vitality, and shrinking up as their fluids are absorbed. It may be that they increase in number and in size till the crowding and pressure prove fatal to them. Their former domicil now becomes their tomb; and effectually precludes any contamination of the fluids of the body, or irritation of surrounding textures, by their remains. This may be deemed a sort of natural cure of such a malady. In the year 1827 I accidentally detected in the liver of a young nobleman a large tumour which, from its size, shape, position, and smoothness, I concluded was produced by a hydatid cyst. He was not even aware of the tumour, and experienced apparently no inconvenience from its presence: it might have been there for years. After some time the tumour became somewhat less prominent. The patient died, of another disease, in 1849; and an old cyst was found containing the dry and withered hulls of numerous extinct hydatids.

There are single cyst-like bodies, with short retractile necks, bearing the generic name of *cysticercus*. One species of this kind, the *cystercus cellulosa* (an adjectival epithet agreeing I suppose with *tela*, understood), inhabits the interfascicular areolar tissue of the muscles. It is rare in the human subject, but frequent in the pig: giving rise to that condition of the muscles which is familiarly known as mealy pork. This is one of the internal parasites with which the organ of vision is liable to be infested. A most remarkable instance occurred a few years ago, in Glasgow. In the eye of a child, who had suffered repeated attacks of ophthalmia, Mr. Logan discovered one day, to his extreme astonishment, a semi-transparent body, about two lines in diameter, floating unattached in the anterior chamber. It seemed almost perfectly spherical, except that from its lower edge there proceeded a slender process, of a white colour, with a slightly bulbous extremity, which appeared to be heavier than the globular part, for it was always turned downwards. This head, or neck, was seen to project or elongate itself from time to time; and occasionally it was drawn up and completely hidden in the cystic portion. When the patient sat still, in a moderate light, the animal covered the two lower thirds of the pupil. "Watching it carefully (says the gentleman who has recorded the case), its cystic portion was seen to become more or less spherical, and then to assume a flattened form; while its head I saw at one moment thrust suddenly down to the bottom of the anterior chamber, and at the next drawn up so completely as to be scarcely visible." The child's head was now turned gently back, and instantly the parasite revolved through the aqueous humour, so that its head fell to the upper edge of the cornea, now the more depending part.

FIG. 124.



FIG. 125.



FIG. 126.

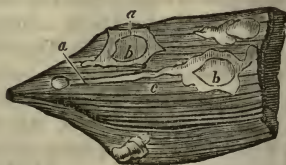


FIG. 124.—*Cysticercus* — natural form of the animal: *a*. The head; *b*. The neck, and *c*. The dilated vesicular tail.

FIG. 125 shows the head in a magnified state: *d*. The proboscis; *e*, *e*, *e*. The suckorial disks.

FIG. 126. — A portion of human muscle with the *cysticercus* inclosed: *a*, *a*. The cyst cut open; *b*, *b*. The parasite; *c*. The muscle.

Upon the child's again leaning forwards, it settled, like a little balloon, in its former position; preventing the patient from seeing objects directly before her.

The animal was carefully watched for three weeks; and no other change was noticed than a slight increase in the bulk of its cystic portion. In six weeks it had evidently grown bigger, the eye became injected, and the iris less free in its movements; and pain ensued. Extraction of the worm was then attempted; but the patient was unruly; the lens was forced out, and the animal ruptured and expelled in shreds: the iris became entangled in the wound of the cornea, and vision in that eye was spoiled.

This cyst-worm has been met with in the brain also, in the heart, and in some other muscular parts. It is generally about half an inch in length.

There is a singular microscopic parasite, the *trichina spiralis*, dwelling in myriads, sometimes, in the muscles of the living human body. It was first described, I believe, by Mr. Hilton, of Guy's Hospital, and afterwards more fully by Professor Owen in 1835. Mr. Wormald, the Demonstrator of Anatomy at St. Bartholomew's Hospital, sent to that gentleman a portion of human muscle, which presented a singular speckled appearance, as if it were mouldy. Mr. Owen found that each speck was a shuttle-shaped cyst, containing a very minute cylindrical worm, coiled up in two, or two and a half spiral turns. The worm measures, when unrolled, no more than $\frac{1}{30}$ th of an inch in length, and $\frac{1}{700}$ th of an inch in diameter; and of course requires, for a satisfactory examination, to be seen through a microscope. The longer axis of the containing cyst lies between, and parallel to, the fibres of the muscle. Fourteen similar instances have since come to Mr. Owen's knowledge.

FIG. 127.

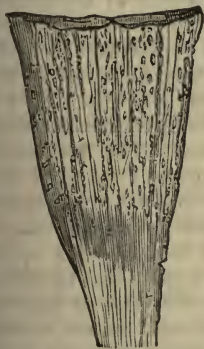


FIG. 128.



FIG. 129.

FIG. 127.—Cysts of the spiral trichina *in situ*, natural size.

FIG. 128.—Separate cyst, containing the trichina magnified.

FIG. 129.—The animal magnified: a. Head; b. Tail; c. Body.

This is a very strange and puzzling kind of parasite. One would imagine that the presence of innumerable living beings, in or between the muscular fibres, would be likely to give rise to symptoms. We might expect pain, or muscular debility, or embarrassed movements; yet no indication of the presence of these worms seems to have been afforded in those instances in which the condition of the subject in whom they were found was known, during life. The principal points that have hitherto been made out appear to be the following:—

1. The muscles thus beset with parasites are the voluntary muscles: and those which lie superficially are fuller of the worm than the deeper seated. The pectoralis major, latissimus dorsi, and other large flat muscles, usually present them in great abundance. They have been detected in the muscles of the eye; and even in those belonging to the little bones of the ear, of the action of which we are wholly unconscious. They occur also in the diaphragm, in the muscles of the tongue and of the larynx, in those of the soft palate, in the constrictors of the pharynx, in the levator ani, in the external sphincter ani, and in the muscles of the urethra. They have not yet been seen in the muscular tunic of the stomach and intestines, in the detrusor urinae, or in the heart. Mr. Owen makes this interesting remark—that all the muscles infested by the trichina are characterized by the striated appearance of their ul-

imate fasciculi: whereas the muscles of organic life, which the animal does not inhabit, have, with the exception of the heart, smooth fibres, not grouped into fasciculi, but united reticularly.

FIG. 130.



Dracunculus: a.
Head; b. Caudal
extremity.

2. It appears, also, from what has been hitherto observed of these entozoa, that their presence in the body is unconnected with age, sex, or any particular form of disease. They have been concomitant with cancer of the penis; tubercles of the lungs; exhaustion of the vital powers by extensive ulceration of the leg; fever, combined with pulmonary phthisis; aneurism of the aorta; sudden depression or collapse after a comminuted fracture of the humerus; diarrhoea. They have also been met with in the muscles of a man who, while in the apparent enjoyment of robust health, was killed by a fracture of the skull.

The *Filaria Medinensis*—*Dracunculus*—or *Guinea-worm*—has its residence in the subcutaneous areolar tissue. It is a long, slender, round, uniform animal, like a fiddle-string, or a piece of bobbin; as you may see in the specimens before you. Its length varies from five or six inches to twice as many feet. Men's lower limbs, their feet and legs, are the parts most commonly possessed by this worm; but it occurs also in the scrotum, in the parietes of the belly, in the arms, beneath the conjunctiva of the eye, and in almost every superficial situation. It is sometimes solitary; but several may coexist or succeed each other in the same individual; nine or ten perhaps. A Dr. Marrudri, a friend of the celebrated Clot Bey, had suffered from twenty-eight of them in succession.

This entozoon is endemic in the hot intertropical regions; in Asia and Africa; upon the coast of *Guinea*, whence its trivial name. It sometimes abounds after the manner of an epidemic. Sir James M'Grigor tells us that the 86th and the 88th regiments, stationed at Bombay, were much plagued by this pest. The 86th was free from it upon entering the fort, in September, 1799; and so continued till the setting in of the monsoon in 1800. In the course of the monsoon nearly 300 of the men were attacked. The 88th regiment relieved the 86th. No case of Guinea-worm appeared among them for nearly a month after their coming into the barracks at Bombay, in October, 1800. In the latter end of November, they embarked for the Egyptian expedition; and in the course of the voyage, in one ship alone, 199 men out of 360 were crippled and laid up with this loathsome disease. Of 181 instances, of which Sir James M'Grigor gives a tabular account, the feet were the parts affected in 124. This fact is illustrative of the mode in which the animal becomes a parasite. It is believed to be one phase of a very minute worm (about $\frac{1}{60}$ th of an inch long, and exceedingly slender) which, abounding at the bottom of tanks, and on low muddy shores, after the periodical rains, in the regions where the dracunculus is endemic, is there called the *tank-worm*. These tank-worms attach themselves to whatever naked parts of the human body they may change to come in contact with; and bore their way into and through the skin. Accordingly, the Guinea-worm is frequently met with in the shoulders and backs of water-carriers. In its new habitat the intruder grows immensely, and becomes the nurse or parent of a numerous offspring. In its full

grown state it is found to be stuffed internally with a countless progeny of little filariæ. If this stage of parasitic life be essential to its fertility, other animals besides man must, in all probability, furnish the requisite temporary residence.

These worms remain for a considerable time in the areolar tissue without producing inconvenience, and therefore without betraying their presence. Hence they are sometimes brought over to this country. The great navigator, Dampier, had no symptom of a Guinea-worm which he carried about with him, until about half a year after he

left the place in which he contracted it. The period of latency is commensurate with that of growth and incubation, which are probably complete when the young filariæ are ready to disperse and to enter upon their independent life. This period seems to lie between six and twelve months.

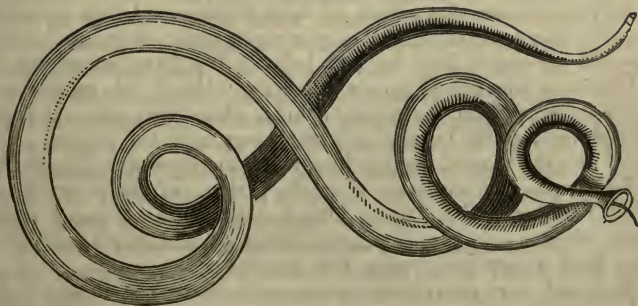
The symptoms which do at length arise are the following:—itching of the part affected; a sensation as if there were something creeping under the skin; sometimes a cord-like ridge can be felt in the track of the worm; at length a vesicle, or a pustule, or a little boil forms, from which, when it breaks, the head of the animal protrudes. This process is often attended with fever; and in certain parts of the body the local suffering is considerable: the areolar tissue sloughs; and sometimes dangerous hæmorrhage occurs.

The only treatment which these cases appear to admit of, is the gradual and careful extraction of the worm. Lest that part of it which already protrudes should recede, or be broken, it is gently wound, day by day, round a small stick, or a little roll of adhesive plaster; pains being taken not to pull upon it so much as to risk its being torn asunder. The roll is protected by a bandage. Whenever, by accident, the animal is broken, very serious consequences are said to ensue; violent inflammation of the part, abscesses and sinuses, and high irritative fever. This mischief is ascribed by some to the presence of dead animal matter, by others, of young filariæ, in the subcutaneous tissue. The extraction is tedious work. Where, indeed, the areolar tissue is very loose, as in the scrotum, the worm is occasionally drawn out at the first attempt: but the process sometimes occupies weeks; and its average period appears to be not less than ten days. When the course of the animal is quite superficial and obvious, the natives are accustomed to make an incision in the skin, at about the middle point, and to pull the worm through from both ends. When once it is out, the parts presently heal. All other medication (and much has been tried) has been found useless; except, perhaps, the administration of assafoetida; and that not as a means of cure, but of prevention. It is said that the Brahmins in India, who are in the constant habit of using this drug, are exempt from attacks of the dracunculus. Cleanliness was also found, in the army, to be a considerable protection. Oiling the uncovered parts of the body has been suggested as a possible defence against the tank-worm.

There is a species of filaria peculiar to the eye; and another to the bronchial glands: but these are extremely rare.

The urinary organs have their parasites also: of which I shall specify but one, and that chiefly on account of its strange lurking-place, and remarkable size. I allude to a species of strongle which sometimes occupies the human kidney, and which is no uncommon tenant of the same organ in various animals; the horse, the bull, the dog, the wolf, the polecat, and the otter. In the human subject its length varies between

FIG. 131.



Strongle of Urinary Bladder.

five inches and a yard, and it is sometimes half an inch in diameter. There is a specimen nearly of that size in the Hunterian Museum. It may well be called the *giant*

strongle, *strongulus gigas*. Fancy a creature as big as a snake coiled up in one's kidney. It gives rise to no distinctive symptoms, although, as you may suppose, it causes much renal distress; hæmaturia, retention of urine, and great suffering in its passage out of the body, either through the natural urinary channels; or by abscess and ulceration through the back.

With respect to some, at least, of the parasitic animals that I have been describing, those I mean which are found shut up in closed chambers, our first feeling is that of wonder how they came there. Into all parts, from which a road is open to the external surface, we can conceive that living creatures may enter, or their eggs be carried. But how can either animals or ova find their way unperceived, into the substance of the liver and of the voluntary muscles, into the eye, into the brain? The whole matter is obscure, yet interesting.

It was the opinion of Linnæus, and of other natural philosophers of his time, that the *intestinal* worms were really terrestrial or aquatic animals which had been accidentally swallowed, either while young and small, or in the antecedent state of ova. It was even pretended that these animals had been recognized and detected out of the body, in stagnant waters. But later inquirers, especially Bremser and Rudolphi, have completely disproved this notion. After dedicating twelve years of his life to the observation and study of entozoa, Bremser was satisfied that no creatures identical in structure with the intestinal worms are ever met with out of the body, except such as have come from the intestines of man, or of some other animal; and conversely, that no terrestrial or aquatic worms are ever found living within the bodies of men or of animals, unless they have been directly and plainly received from without.

But, then, is it not possible that, as Boerhaave supposed, aquatic or terrestrial reptiles, casually entering the body from without, being placed under entirely new and unnatural conditions, may have attained a monstrous growth, and undergone metamorphoses, such as we know that some of the lower animals, by change of circumstances, do undergo; as the tadpole becomes a frog, the maggot a butterfly?

That animals, or the ova of animals, having their proper life out of the human body, may by accident enter it, and having entered may grow, or hatch, is not to be doubted. But that the true intestinal entozoa have ever such an *accidental* origin is rendered very unlikely by the facts that certain species of worms infest certain species of animals only: that in the same animal different species of worms occupy (as we have seen) special parts of the alimentary canal; have each its peculiar habitat: that worms and animals of external origin mostly die as soon as they are received into the digestive organs: while the true intestinal worms perish whenever they are delivered therefrom. Again, the circumstances that these worms not only live but breed within the human bowels, and that they are met with even in the intestines of the unborn fœtus, are very adverse to this theory of an accidental *error loci*.

But to give up the notion of a fortuitous entrance, may not intestinal worms spring from specific germs or ova introduced from without, not casually, but in accordance with a natural law: germs or ova which find in the interior of certain other living bodies the only conditions that admit of their development, the only soil in which they are capable of germinating, the only nest in which they can be hatched? I confess that such has always been my own belief. We have something like this at least in that common affection of horses called the "bots." A species of æstrus or gadfly deposits its eggs upon the animal's hide, where they cause, I presume, some irritation, which induces the horse to lick that part with his tongue. The eggs are thus conveyed into the mouth, whence they reach the stomach. There they are converted into larvæ, and affix themselves to the parietes of the stomach. At length when they are ready to undergo their final metamorphosis, they are detached from the interior of the stomach, pass along with the food and fæces through the intestines, and are ejected from the rectum with the dung.

Why, it is asked, may not similar phenomena take place in the human body? There can be no doubt that we every day swallow, inadvertently, numerous ova, of various kinds. It is supposable enough that sometimes the digestive organs may, and sometimes they may not, have the power of decomposing or expelling these ova.

It is quite certain that what are generally called *spurious* worms may have that mode of origin in the body. Thus Dr. Elliotson states that he had once a patient, an

infant, who discharged from the bowels a dozen live larvæ, or maggots, of the common fly. The child had eaten part of a *high* pheasant some months before. There was, in that case, this instructive circumstance. The infant had been suffering under a chronic cough, but as soon as those larvæ were got rid of, the cough ceased. Dr. Elliotson says that he saw them in the napkin, moving about in the fæcal matter, just as they might have done if they had never been in the child's body. The same physician tells us that he has twice known, in two different patients of his, a living caterpillar to be voided from the intestines. One of the patients was a woman who had been in the habit of eating cabbage stalks while she was washing them for the pot. The moth lays its eggs on cabbages, and she no doubt had swallowed some of them, and one had hatched within her. In the ninth volume of Dr. Duncan's *Medical Commentaries* is a precisely similar case. A boy, after a dose or two of calomel and jalap, emitted from the rectum very many caterpillars, all alive, and full of activity. He had been in the habit, when in the garden, of eating young cabbage leaves. Till this habit began he had enjoyed good health. While the animals were within his bowels he suffered severely; had locked jaw; and fell into a state resembling coma. Upon their expulsion he recovered perfectly. Centipedes have in like manner been vomited, and voided from the bowels. But the most wonderful instance of this kind that ever was heard of, is related by Dr. Pickells in the *Transactions of the King and Queen's College of Physicians in Ireland*. A young woman of melancholic disposition and chlorotic appearance, had been in the daily habit, from some superstitious motive, of drinking water mixed with clay taken from the graves of two priests who lived and died in the odour of sanctity. In this way she probably inhibited the ova of the insects which subsequently issued from her body. In the course of about three years and a quarter, she discharged, partly by vomiting, but chiefly *per anum*, upwards of 2000 beetles, and their larvæ, most of them alive. Dr. Pickells counted more than 1300. Larvæ, and pupæ, and perfect insects, all came forth simultaneously. Some of them ran off, as soon as they were vomited, into holes in the floor; and two large winged insects were so lively and vigorous, as immediately to fly away. These strange births were preceded and attended by a complicated and distressing train of symptoms; a gnawing, and sense of something creeping at the pit of the stomach, vomiting of blood, amenorrhœa, hysterical convulsions, headache, retention of urine, and sometimes a degree of mental derangement. She was at length freed from this disgusting malady by large doses of oil of turpentine.

These spurious worms differ from the true intestinal parasites in this—that the human alimentary canal is not their *only*, but their accidental and unusual nidus. Nevertheless, their occasional presence, alive, in that place, adds to the probability that some of the entozoa may be originally ectozoa.

The main difficulty, however, respects those animals which occupy shut cavities within us, or are embedded in our solid organs: and this difficulty forms one avowed ground of the theory of *equivocal generation*: which means the spontaneous production of living creatures, independently of any germ, or egg, or parent. The vulgar suppose that dirt engenders fleas; that maggots result from the putrefaction of flesh; that eels arise of themselves in, and out of, mud; that insects are bred from the dung upon which they congregate and feed. In other words, they infer the spontaneous origin of those creatures, of which they cannot or do not trace the procreation by pre-existing parents: and philosophers and men of science have done the same. They will not believe that which they cannot see. Now this doctrine of equivocal generation shocks, I confess, my mind, and offends my reason. If well founded, it strikes at the root of that great argument of Natural Theology, which deduces the existence of a First Intelligent Cause, from the marks of adaptation, design, and contrivance, so manifest throughout the visible universe. Observe the demand which this doctrine makes upon our faith. In defiance of all experience and analogy with respect to creatures which our finite senses are competent to examine, it calls upon us to believe that living beings of complex and intricate, yet definite and harmonious structure; provided with a digestive apparatus, with instruments of locomotion, with generative organs; of various species; in many instances of separate yet answering sexes; that not one or two of these beings, nor a pair or two, but beings and pairs innumerable, are daily formed by the casual concourse of "organic molecules." The obscurity that hangs over the origin of some of the entozoa is not indeed the only, nor I think the

chief ground upon which the notion of spontaneous generation rests. You are probably aware that minute animalcules, so minute that most of them cannot be seen without a microscope, soon become abundant in water wherein vegetable or animal matters have been dissolved by infusion. Such animalcules are therefore called *Infusoria*. How do they come there? There are two suppositions open to us. One is, that they are formed by the fortuitous union of organic atoms contained in the infusion. The other is, that they proceed from ova or germs existing in the liquid, or floating always in the atmosphere, and ready to quicken whenever they light upon their proper element. That the ova of animals which are themselves visible only by the aid of a microscope, should be absolutely invisible to us, is not surprising. We may conclude that the latter supposition is the most true, if we can show that when these ova or germs are excluded, all the other conditions of the production of infusoria being present, no animalcules appear. Now Spallanzani long ago found, by careful trials, that no animalcules were discoverable when the access of air to the infusion was completely prevented. But it has been objected to his experiments, that the presence of atmospheric air may be one of the essential conditions which the requisite combination of the organic molecules demands. Air, solar light and heat, and organic matters in solution being given — does animal (or even vegetable) life ever result? That is the question. The experimentum crucis has been made, and has answered “No,” as I learned from one of Professor Owen’s admirable introductory lectures; by whom, I am glad to find, this uncomfortable doctrine of equivocal generation is strongly discountenanced. The experiment to which I refer was conducted by M. Schulze, of Berlin. I will read you his own account of it, as I find it recorded in the *Edinburgh New Philosophical Journal*.

“The difficulty to overcome consisted in the necessity of being assured, first, that at the beginning of the experiment there was no animal or germ capable of development in the infusion; and secondly, that the admitted air contained nothing of the kind. For this purpose I filled a glass flask half full of distilled water, in which I mixed various animal and vegetable substances; I then closed it with a good cork, through which I passed two glass tubes bent at right angles, the whole being airtight. It was next placed in a sand bath, and heated until the water boiled violently, and thus all parts had reached a temperature of 212°. While the watery vapour was escaping by the glass tubes, I fastened at each end an apparatus which chemists employ for collecting carbonic acid; that to the left was filled with concentrated sulphuric acid, and the other with a solution of potash. By means of the boiling heat, everything living, and all germs in the flasks or in the tubes, were destroyed; and all access was cut off by the sulphuric acid on the one side, and by the potash on the other. I placed this easily moved apparatus before my window, where it was exposed to the action of light, and also (as I performed my experiment during the summer) to that of heat. At the same time I placed near it an open vessel, with the same substances that had been introduced into the flask, and also after having subjected them to a boiling temperature. In order now to renew constantly the air within the flask, I sucked with my mouth, several times a day, the open end of the apparatus filled with solution of potash; by which process the air entered my mouth from the flask through the caustic liquid, and the atmospheric air from without entered the flask through the sulphuric acid. The air was, of course, not at all altered in its composition by passing through the sulphuric acid in the flask, but if sufficient time was allowed for the passage, all the portions of living matter, or of matter capable of becoming animated, were taken up by the sulphuric acid and destroyed. From the 28th of May till the beginning of August, I continued uninterruptedly the renewal of the air in the flask, without being able, by the aid of the microscope, to perceive any living animal or vegetable substance, although, during the whole of the time, I made my observations almost daily on the edge of the liquid: and when at last I separated the different parts of the apparatus, I could not find in the whole liquid the slightest trace of infusoria, of confervæ, or of mould. But all three presented themselves in great abundance a few days after I had left the flask standing open. The vessel which I placed near the apparatus contained, on the following day, vibriones and monads, to which were soon added larger polygastric infusoria, and afterwards rotatoria.”

This experiment confirms the belief which various other facts had suggested—that

the different kinds of entozoa are none of them parentless animals, and that they somehow find their way into the body they inhabit, from without. The lowest of the infusoria are of fixed and determinate species; and Ehrenberg states that even the minutest monads possess a complicated organization. It may be asked, concerning both them and the entozoa, why, if they ever arise spontaneously, should they be furnished with a generative apparatus? Again, some of the entozoa abound in certain places, and strangers coming to those places contract them there. The dracunculus was thought by the soldiers in India to be communicable from person to person, as the itch insect, and the chigoe, to both of which it has some analogy, certainly are. The infant filaria probably creeps in through the skin without causing any noticeable pain. Even that monster among the entozoa, the tape-worm, invades the bodies of those persons who visit the countries to which it belongs. I told you before that, when tape-worm occurs in Germany, it is always the *tænia solium*; when in Switzerland, almost always the *tænia lata*. Now the celebrated Soemmering was afflicted with one of these beasts; and he was by birth a German: yet the worm that he voided was of the foreign species, the *tænia lata*. He had resided, however, for some time in Switzerland: and there, no doubt, he somehow picked up the ovum, or the larva, of the parasitic animal. Mr. Abernethy once told me the following curious story:—A shepherd had to drive a flock of healthy sheep to a distant part of the country. The journey occupied two or three days. On the road one of the animals broke its leg, and was carried the rest of the way on horseback. All the flock, except this hurt individual, were turned for one night into a marshy pasture. The broken limb was set, and the patient got well; and was the only one of the whole flock that did not subsequently become affected with the rot; the only one that escaped having flukes in its liver. Is it not almost certain that the germs of these parasites were swallowed with the herbage cropped by the sheep in the damp meadow? Professor Owen suggests that “the young flukes may pass instinctively from the duodenum through the ductus choledochus to the gall-bladder.” The germs of the entozoa which dwell in closed chambers, and within the solid viscera of the body, are probably carried thither by the blood.

The wonderful tenacity of life possessed by some (at least) of these minute creatures, increases the chance of their reaching at last their appropriate habitat, and adds proportionally to the presumption against their spontaneous origin. Spallanzani kept certain infusorial animalcules four years in a state of complete desiccation and apparent death; but they presently recovered life and motion upon being then moistened. He dried and moistened alternately the same animalcules twelve times, with similar results; except that the number of revivers was each time less and less, and after the sixteenth moistening there was no resurrection. The *Vibrio Tritici* (a minute worm which is a parasite of wheat) having been dried by Mr. Bauer, resumed its activity, when remoistened, after the lapse of from four to seven years. Another small parasitic worm has been seen to exhibit strong contortions—evident vital movements—after having been subjected for above an hour, together with the cod-fish which it inhabited, to the temperature of boiling water. On the other hand it is stated by Rudolfi, that entozoa which infest the herrings annually sent to Berlin, hard frozen and packed in ice, do, when thawed, manifest unequivocal signs of restored vitality. “If,” (says Professor Owen, from whom I take these facts,) “the fully developed and mature entozoa can resist such powerful extraneous causes of destruction, how much more must the ova possess the faculty of enduring such without losing their latent life.”

The difficulty belonging to this subject was materially lessened by the curious modern discovery, first announced by M. Steenstrup, of *alternating generations*. “The young of most of the entozoa undergo metamorphoses.” Certain entozoa, the parasites of certain animals, have been ascertained to present merely a transition state of other entozoa, the parasites of certain other animals. In the last edition of his Lectures, Professor Owen stated the probability that the whole of the *cystic* family of entozoa were nothing else than the larvæ of the whole *cestoid* family: a doctrine which may now be said to be fully established. The doctrine was suggested by definite resemblances and relations subsisting between the two sets of entozoa on the one hand, and between the two kinds of animals respectively infested by them on the other. The first, or lower sort of these entozoa, are so far imperfect, that they are sex

less, have no generative organs. The second, or higher, possess, as I have shown you, an apparatus for most abundant reproduction. Parts of the first have a close rudimental likeness to corresponding parts of the second: their heads, especially, exhibit a striking conformity of structure. This is the sort of relation which is observed between the *entozoa*. That which exists between the two *infested animals*, is the relation of natural prey, and devourer. To take an example by way of illustration. The common domestic mouse and rat are subject to a species of cyst-worm, the *cysticercus fasciolaris*. The cat, which preys on these animals, is subject to a species of tape-worm, the *tænia crassicollis*. Now this *tænia*, Professor Owen informs us, is remarkable, among *tænia*, for the disproportionate size of its head, its short and thick neck, the position of its four suckorial disks, and the shape and number of the hooklets of its proboscis: and all these peculiarities are repeated in the *cysticercus* of the mouse and rat, which cyst-worm he regarded accordingly as the larval form of the tape-worm of the cat. Nor is this a solitary instance. "All the *cysticerci* manifest their affinity with the *cestoidea* by the organization of their heads." I may add that their necks are divided into segments resembling the joints of the *tæniæ*. They seem, in fact, part hydatid, part tape-worm.

This doctrine has since been confirmed by the experimental researches of several eminent German naturalists. Küchenmeister was the first, I believe, to feed dogs and cats upon flesh that contained living cyst-worms. After a while he found corresponding tape-worms in the intestines of those dogs and cats. But he was not content with practising upon these *corpora vilia*. He conceived and executed the horrid and revolting project of sowing (as it were) cyst-worms in the human bowels. He contrived to mix *cysticerci*, collected from pigs and rabbits, with the food of a criminal under sentence of death for murder, passing them off as grains of rice in warm rice soup, as bits of paste in vermicelli gravy, and as small lumps of fat in black-puddings. These were swallowed by the unconscious and wretched man at various periods from seventy-two to twelve hours before his decapitation. Forty-eight hours after it, ten young tape-worms were seen attached by their hooks and suckers to his small intestines.

Similar experiments upon the lower animals were carefully repeated, with similar results, by Professor Siebold. The transformation of the imperfect cyst or bladder-worm into the perfect tape-worm was thus satisfactorily demonstrated. To clench the proof, the converse experiment has since been made. Joints full of ripe ova, from the tape-worm of a dog, were given with their food, by M. Haubner, of Dresden, to certain lambs. In about a fortnight all these lambs (and no others in that flock) became affected with what is called "the staggers;" and when they were killed, at different intervals, cyst-worms, in various stages of growth, were discovered in their brains, and in other parts of their bodies. Again, mice were induced by Professor Leuckart, of Giessen, to eat mature joints of the *tænia crassicollis* of the cat; and the livers of those mice were found thereafter to be studded with the *cysticercus fasciolaris*. Swine also, fed with the eggs of the *tænia solium*, and killed some weeks afterwards, were occupied throughout their whole bodies with the *cysticercus cellulosæ*.

Whether the several cyst and tape-worms are distinct in species, or mere varieties determined by the species of animal which they infest and feed on, has not, so far as I am aware, been ascertained. It is however believed that the *cysticercus tenuicollis*, nursed in the ox and other ruminant animals, is transfigured and completed into the *tænia serrata* of the dog; and that the *cysticercus pisiformis* of the rabbit and hare, becomes in the fox, which eats them, the *tænia crassiceps*. Reasons were suggested, in 1852, by Dr. Nelson, for thinking, what can now be scarcely doubted, that the *cysticercus cellulosæ*, hatched in the bodies of measly pigs, is matured in the human body into the *tænia solium*: and if the kind of animal in which the ultimate metamorphosis takes place, determines the specific form of the resulting *tænia*, that of man may sometimes be derived from *cysticerci* which haunt the bodies of sheep and oxen. Pork is eaten raw by many farm-labourers in this country, and with it the cystic larva of the *tænia solium* must be often transplanted into their bowels. The taste for raw or half-cooked meat is not uncommon. It is a reasonable conjecture that the *tænia lata*, which is endemic on the shores at the head of the Gulf of Bothnia, and frequent in the neighbourhood of the Swiss lakes, may be the adult

form of a cystic entozoon from some marine, or fresh-water animal. Professor Siebold believes that certain minute worms which infest a species of *slug*, are the produce of ova from the *tænia* of some bird. The ova are voided with the excrement, and the creatures hatched from them may be presumed to creep upon the body of the first slug that may crawl near them: and then, if they have the additional good luck to be swallowed, with the slug, by the proper bird, they regain a fitting nidus for their further and perfect development. Our red grouse, a bird peculiar, I believe, to the British Islands, are very subject to tape-worms. In some years thousands of them die of this distemper. If any of you can succeed in tracing the birth and nursery habits of these destructive parasites, so as to prevent their final development in the grouse, you will secure the praise and thanks of a great number of English sportsmen. We may take for granted that the little echino-coccus is the cystic stage of some tape-worm; but to what animal the tape-worm specially belongs, is as yet an unsolved mystery.

Of the innumerable eggs produced by the human and by other tape-worms, very few can ever reach the fitting nidus for their first hatching into the cystic state, and fewer still the remoter place of their final mutation into perfect *tæniæ*. Professor Owen remarks that many of these little ova, in great cities especially, must eventually find their way into streams of water, for whose minute inhabitants — or for terrestrial creatures under other circumstances — they probably furnish food: being thus analogous to the seeds of the fruitful *cereal*ia, which “minister far less to the perpetuation of their own species, than to the sustenance of man.”

After all, it is a mortifying rebuke to human vanity, that for the birth, the nurture, and the support of a repulsive, a noxious, and (so far as our finite faculties can perceive) a useless parasitic worm, the subservience should be required of two other creatures of higher grade, and (as we speak) of nobler dignity in the scale of life; one of which is no less a being than that “paragon of animals,” Man.

Upon the whole, we may reject the hypothesis of equivocal generation, and fall back upon the Harveian axiom, taken in its most extended sense, of *omne vivum ex ovo*. If I have digressed somewhat in order to set before you the grounds of my own belief in this matter, the interest and importance of the subject must be my excuse.

What I have further to say will relate exclusively to the intestinal worms of the human body: their predisposing causes; the symptoms they occasion; and the methods of getting rid of them.

Of predisposing causes there is little to be said. I know of none except the endemic or the accidental prevalence of the outward germs of these worms, and the personal habits of those who are liable to their intrusions. The *tænia solium* has been observed to be common among pork-eating communities; the cyst-worm from which it is bred being, as I have already stated, a very frequent parasite of the omnivorous pig. By the same rule, it is said to be very rare among Jews. Wherever raw meat forms a part of the food of the people, as in Abyssinia, there we find the tape-worm prevalent. Even in this country the taste for raw meat appears to be less singular than one might have supposed. Dr. Willshire has lately published several instances, as Dr. Crichton had previously published one, of the coincidence, in the same person, of tape-worm, with the practice of eating raw or under-done flesh.

It more concerns us to inquire into the general symptoms, through which the existence of worms in the alimentary canal may be ascertained, or suspected.

Those symptoms are very multifarious; and, for the most part, very equivocal. I know of none that can be reckoned certain or pathognomonic, except the actual appearance of one or more of the animals, or of portions of them, in the excrements of the body. Yet that they do give rise to a variety of morbid phenomena — which morbid phenomena are, however, liable to be produced by other causes also — there can be no question.

The most common of these are well known to all nurses and old women; such as colicky pains, and swelling of the belly; picking of the nose, in consequence of itching and irritation there; itching of the fundament; a foul breath; grinding of the teeth during sleep; a variable and capricious appetite, sometimes voracious and insatiable, sometimes none at all; and irregular bowels.

Worms now and then occasion strange, and even severe *nervous* symptoms, explainable upon the principle of the reflex office of the spinal cord. We conclude that the symptoms are owing to worms in such cases, because they cease when the creatures are got rid of. Some examples of this I have already noticed. Thus Dr. Elliotson's infant patient lost a chronic cough upon the expulsion of the live larvæ of the common fly. Bremser gives a very similar case. A child of eleven, afflicted with tænia, had a troublesome dry cough. It was observed that the cough was suspended for two months, just after a very large portion of the worm had been brought away by anthelmintic medicines. This kind of coincidence happened, not once only, but three or four times; and at length, when the whole of the worm had come away, the cough was permanently cured. You may read an instructive instance of the same kind, in Dr. Graves' Fortieth Clinical Lecture. I mentioned, some time ago, the frequent association of intestinal worms with epilepsy, which is then of the eccentric form: and I stated that a certain nobleman voided some kind of worm (a tape-worm, I think) from his bowels, and was thenceforward free from epileptic fits, under which he had long laboured. A curious circumstance, illustrating the fact that irritation of the mucous membrane of the alimentary tube may affect distant parts, is quoted by Dr. Joy from Albinus. A soldier received a wound, which led to the formation of an unnatural anus, in front of the abdomen, and in the track of the colon. Through this opening the mucous membrane of the bowel sometimes protruded; and whenever it was out, and exposed to the contact of cool air, the patient began to cough; and continued to do so till the mucous surface was warm again. Partial palsy, amaurosis, aphonia, and other nervous symptoms, occasionally depend upon the presence of worms in the intestines.

[Dr. Schleifer, in the Austrian Weekly Journal of Medicine, relates the case of a child, nine years of age, who became dumb, after suffering in early life from cutaneous eruptions, engorgement of the glands, &c. The loss of hearing was attributed chiefly to a fall, and treated accordingly. The child became emaciated, pale, with a dark leaden appearance of the contour of the eyes. The tongue was white and loaded, the breath offensive, and the abdomen tumid and hard. The muscles of the face were in constant motion, and the patient moaned incessantly. Dr. S. suspected the presence of worms, and treated the patient accordingly. In three weeks, eighty-seven lumbrici were discharged, and, during five weeks, immense quantities of ascarides. At the end of the sixth week, the child had recovered his hearing and speech.]

In the Journal of Medicine and Surgery of Paris, for April, 1844, a case is quoted, from the Gazette Médicales of Dijon, of a young man, nineteen years of age, who was attacked with all the symptoms of acute pleurisy: chill, followed by fever; severe pain in the left side; difficult, jerking respiration; paroxysms of dry cough, which occasioned the patient to scream out, &c.; all of which symptoms promptly disappeared after the discharge of seventy-five lumbrici.—C.]

But let us examine into the symptoms which are more or less proper to particular species of intestinal entozoa; and into the treatment which they severally require.

A variety of symptoms are ascribed to the *ascaris lumbricoides*. Dr. Baillie says that the most characteristic are a tumid belly, emaciated extremities, offensive breath, and a deranged appetite. To these may be added colicky pains of the abdomen. When these animals get out of the small intestines, and ascend into the stomach or œsophagus, they may occasion pain, nausea, vomiting, even convulsions. They have caused death, as I mentioned before, by crawling into the biliary ducts, and into the chink of the glottis. Sometimes, on the other hand, they emerge, unexpectedly, from persons who had received no previous notice of their presence within.

This, the commonest parasitic tenant of the human bowels, is also a troublesome one to eject. A great variety of anthelmintics have been cried up as successful against it; but brisk purgatives and bitter medicines in the intervals, have the best evidence in their favour. These animals seem not to like steel; and my own plan of assailing them is that of purging the patient from time to time by calomel and jalap, and administering three times a day, some preparation of iron; the sulphate, or the muriated tincture. I believe that most of the patent worm remedies consist of mercury, jalap, and scammony, given in strong doses. The fœtid drugs, assafoetida, galbanum, valerian, are often used. Cowhage also, the *dolichos pruriens*, which is

supposed to tease the skin of the parasite no less than that of the human worm; and tin-filings, which are thought to bruise or lacerate the offenders, are favourite medicines with some persons. I have never tried them. The oil of turpentine I have not found so effectual in expelling this species of ascaris, as I shall presently show you that it is against the tape-worm. Croton oil has been much commended, either given by the mouth or rubbed upon the abdomen. Common salt, coloured by cochineal, and exhibited every morning in half-drachm doses, was found by Dr. Rush to be very successful.

The symptoms produced by the *ascaris vermicularis* are itching and irritation about the anus, especially in the evening, and at night. The annoyance is aggravated by the warmth of the bed, and by whatever overheats the body. I would refer you to a paper by Dr. Heberden on this subject, in the first volume of the *Medical Transactions*. The case he describes is the more valuable, as it was related to him by a physician who was all his life plagued by these thread-worms. Generally, however, they infest children; and become fewer, and at length disappear, as childhood passes into youth. When they do accompany life through its several stages, although they are a source of great worry and suffering, they do not appear to shorten the duration of the patient's existence.

To introduce at one end of a twisted tube, several yards long, substances which are intended to act upon animals that live quite at its other end, would be a very round-about course. Whether a purgative effect, or a specific destructive effect, be the object, enemata are preferable to medicines given by the mouth. *Bitters* offend and destroy these little worms. I have relieved many patients from their tormentors by prescribing simply the infusion of quassia as an injection. Tobacco clysters are praised; but the remedy is a hazardous one. Dr. Darwall says of an enema composed of half an ounce of the muriated tincture of iron mixed with half-a-pint of water, "there are few cases so obstinate that this will not suffice to overcome." Lime water, injected into the rectum, forms another efficacious remedy against ascarides.

Thread-worms may be scooped out of the rectum, with the finger. Old women fish for them with a piece of fat meat, or a candle, wherewith the entangled worms are drawn out of the bowel. Perhaps, in troublesome cases, the plan laid down by Martinet is as good as any. He recommends three successive injections: the first merely purgative; the second specific (common salt in solution, cold vinegar and water, lime water, some bitter infusion); the third, oleaginous and soothing. Oil often allays the itching. This teasing symptom may sometimes be quieted by applying a towel, wetted with cold water, to the fundament, while in bed.

With some means of this kind for expelling the worms, appropriate measures should be combined for improving the general health.

I know of no signs by which the presence, in the human intestines, of the *tricocephalus dispar* is revealed; and I am equally ignorant of any remedies for it.

That a *tape-worm* is within, we know when joints of it are voided. Numberless symptoms have been ascribed to this huge internal parasite. The following are probably the most distinctive. Uneasy feelings in the epigastrium, which often abate or are removed by eating; the appetite generally craving, but sometimes bad; itching of the nose, and of the anus; nausea; colic; giddiness; a sour breath. Less frequently loud borborigmi occur; and sometimes convulsions.

Louis has watched and recorded, with his accustomed minuteness, the symptoms of ten cases, in the wards of La Charité. Seven of the patients were males, and three were females. The youngest was a boy of twelve, the son of another of the patients; the oldest was seventy-four. Most of them were in comfortable circumstances, and had been habitually well fed. The greater number of them had for some time been passing fragments of tape-worm, with their stools, in their clothes, and in their beds. In one of the cases the articulations had been twice only detected in the stools, and each time upon the operation of a purgative.

In all the patients but two, the other symptoms commenced when the evacuation of the fragments commenced. This renders it probable that the worms begin to give annoyance when they get into the large intestine. The temporary relief that results from the expulsion of portions of the animal strengthens that supposition. In the

Medico-Chirurgical Journal is mentioned the case of a man who was in the habit of freeing himself from large fragments of tape-worm by introducing a stick into his rectum, and twisting the worm round till it broke.

The chief symptoms observed in Louis' cases were colicky pains of the abdomen; itching of the anus, and of the end of the nose; uneasiness in the epigastrium; and deranged digestion and appetite.

Pain in the abdomen occurred in *all* the instances: but it differed in different cases, both in degree and in kind. It was intermittent; and mostly felt towards the flanks.

There was itching at the margin of the anus in seven of the ten cases; itching of the nose in four. With one exception only, itching was present in one or the other, or in both of these situations.

The appetite was craving in one patient; unaffected in four; variable or bad in all the rest. In all, slight emaciation was observed. In all, the pupil of the eye was of its natural dimensions. This is noticed, because dilatation of the pupil has been set down as one of the symptoms of *tænia*.

Louis thinks that the following combination of symptoms indicates with tolerable certainty the presence of some kind of worm in the intestines. Pain in the belly; colic of various degrees of intensity, unaccompanied by diarrhoea; itching about the anus, and at the end of the nose. If pains in the limbs, lassitude, and nervous symptoms exist also, the diagnosis is strengthened.

We have a very effectual remedy for *tænia*—at any rate for the tape-worm of this country—in oil of turpentine, given in large doses. The anthelmintic virtues of this substance were not generally known till a paper on the subject, by Dr. Fenwick, of Durham, was published, in 1811, in the *Medico-Chirurgical Transactions*. A sailor, plagued by this parasite, had noticed that large fragments of the worm were passed whenever he had swallowed an unusual quantity of raw gin. Thinking that a stronger spirit might have a stronger effect upon his internal enemy, he tried a glass of turpentine, which completely cured him. This practice was then taken up by some unprofessional persons, who gave turpentine, with similar success, in several cases. At last Dr. Fenwick, in conjunction I believe with my friend Dr. Southey, investigated the subject; and when they had satisfied themselves of the value of the remedy, the result of their inquiries was communicated by Dr. Fenwick to the late Dr. Baillie, in a letter which was read before the Medical and Chirurgical Society. But there is nothing new under the sun. Fifty years ago, Mr. Malden, in the *Memoirs of the London Medical Society*, recommended the same remedy, in the same doses, for the same purpose. But his recommendation had been neglected, or forgotten.

The dose of turpentine is from half an ounce to two ounces. It may be given in combination with castor oil; or castor oil may be administered afterwards to assist its purgative effect. It should be taken in the morning, fasting; and no drink should be admitted into the stomach until the medicine begins to operate, lest sickness and vomiting should be provoked. The worm generally is voided, dead, within an hour or two.

The inconveniences of turpentine as a remedy are its nasty taste, the sort of intoxication it is apt to produce, the distressing sickness it excites in some persons, and the strangury it sometimes occasions. This effect of it, however, is less common from large than from small doses of the oil. The bowels should be kept open with castor oil, so long as the urine retains the violet smell, which indicates the presence of the turpentine in the circulation.

Upon the continent a celebrated empyreumatic oil, called Chabert's, is in great repute. It contains turpentine; but is still more nauseous than it. One part of the empyreumatic oil of hartshorn is mixed with three parts of oil of turpentine. After the mixture has stood for three days, three-fourths of it are to be distilled over, in glass vessels, by means of a sand-bath. Chabert was a veterinary surgeon; and had used this remedy with remarkable success upon domestic animals, cows, dogs, and sheep. Bremser, after testing its safety by taking it himself, administered it, he tells us, to no less than 500 individuals troubled with *tænia*. Among these were two children, a year and a half old. He found it both a powerful and a permanent cure. Of the whole number treated there were but four who required a second course of the remedy. The dose is two teaspoonfuls, night and morning, until four or six

ounces have been taken; a purgative being from time to time interposed. If that dose produce any confusion of head, it must be diminished.

Bremser admits that this curative process is tedious, but affirms that, on the other hand, it is safe, and but little inconvenient. When the patient has continued free from any symptoms of tape-worm for three months, he concludes that the cure is absolute. Other practitioners agree with him in attesting the efficacy of this oil; but are not so confident about its perfect safety and convenience.

Another great remedy is the bark of the pomegranate-root. This is at least as old as the time of Celsus. It has long been employed in India. Its value has only been recently appreciated in France; and in this country it is not much used, I believe, even yet. You may read a very instructive account of its effects in a paper communicated to the Medical and Chirurgical Society by Mr. Breton. He boiled two ounces of the fresh bark of the root in a pint and a half of water, till the decoction was reduced to three-quarters of a pint. Two ounces of this, cold, he gave to a patient who had tape-worm, and repeated the dose every half hour until four doses had been swallowed. About an hour after the last dose, an entire *tænia* was voided, alive.

The bark dried in the sun he found still more active. A stout man, forty years old, took a decoction prepared in the same way as the former, only with the dried instead of the fresh bark. Three hours after the first dose a live tape-worm came away, nineteen feet two inches long. The medicine thus prepared seemed to be too strong; the patient was sick, giddy, and trembling for several hours. He, also, had taken four doses.

To two boys, of seven and ten years of age, he began early in the morning to administer one ounce of a decoction made of half the previous strength, every half hour, for six times; and then stopped. In the middle of the day he resumed the medicine in half-ounce doses. Giddiness and faintness soon came on; and, about five o'clock, each of the patients passed a *tænia* of the *solium* kind.

A scruple of the powder was given, mixed with water, every hour, for five successive hours, to a boy of nine. Forty minutes after the last dose a living *tænia* was expelled.

The same quantity was given to a girl, ten years of age; beginning at eight, and repeating the dose hourly till twelve o'clock. At twenty-four minutes past one she voided a living *tænia lata*: and the next morning, at nine o'clock, a dead *tænia solium*. This curious case shows that the two species may exist in the same person at the same time.

Mr. Breton relates other examples; but those which I have cited are quite sufficient to demonstrate the power of this substance. The bark appears to act upon the worm as a poison. In tepid water *tæniæ* will live for several hours. When they are plunged into the aforesaid decoction, they writhe and manifest great uneasiness. Between the first dose of the medicine and the expulsion of the worm the shortest period appears to have been three hours, the longest twenty-five.

Louis' ten cases, before referred to, were all treated by a nostrum called the potion of M. Darbon. It proved successful in all. Eight or ten ounces of it were taken in the morning, before breakfast; and the cure was accomplished by that single dose. It is said to be quite safe, to have no very decided taste, and to cause no further inconvenience than a slight colicky feeling, and uneasiness in the epigastrium, less than is produced by many a purgative: and even these sensations were probably owing to the movements of the worm; for when, after the lapse of four months, the dose was repeated, it was followed by no inconvenience at all. The medicine is not strongly cathartic, and sometimes requires the aid of a lavement. In each of these cases the expelled *tæniæ* were apparently dead. Their heads were detected in the evacuations: in one instance seven heads were seen. Louis says that within a few days at furthest after the discharge of the worms, all the previous symptoms of their presence ceased; and the patients improved in respect of appetite, digestion, complexion, flesh, and strength. They all remained well four months afterwards, and then the potion was again administered; but it brought away no more worms. Some of the patients, who had previously tried other modes of cure, had never enjoyed so long an interval of freedom.

In 1850, a new vermicide drug was introduced from France into this country, consisting of the dried flowers of a tree that abounds in Abyssinia, where they have been

employed as an anthelmintic for more than two centuries. Kousso, in the Amharic language, is the name both of the tape-worm, and of the remedy. This has been tried by several English physicians, but first and most frequently by Professors Budd and Todd, in the King's College Hospital. In every instance, so far as I am informed, a single dose of the medicine has been followed by the expulsion of the worm, or of a great part of it. Its action is attended with but little distress, often with none. Occasionally it has caused nausea; sometimes it has proved gently aperient; and sometimes the aid of a subsequent purgative has been found requisite. It appears to be more uniformly successful than turpentine,—while it is much less offensive to the palate, and less rough in its operation. *Tæniæ* have been dislodged by it where turpentine had failed, or had lost its previous power. A patient of my own, a young military officer, was plagued with tape-worms in India. He had been in the habit of taking turpentine, which at first brought away portions of the animal, but after a while it failed entirely. Upon returning to England he took a dose of the kousso, a quarter of an ounce; and in four hours a worm, twelve feet long, was voided alive. But the symptoms recurred, and he then swallowed a second dose, with a similar result. A third dose, taken a fortnight afterwards, simply purged him. He assured me that, irrespective of its vermifuge property, he found the kousso far less unpleasant and annoying than the turpentine had been. In but few instances has the head of the parasite been noticed in the evacuations; but portions of its narrow part, near the head, often. Neither has the kousso always achieved a radical cure. Both kinds of *tænia* have been expelled by it; and being certainly effectual for the time, and both speedy and safe in its operation, it will no doubt be largely used here now. At first its cost was an impediment. Thirty-five shillings an ounce was the price, and half an ounce the dose. Much smaller quantities have however been found insufficient, and an ounce of the dried flowers may now be obtained for twenty pence. The powdered flowers are steeped in half a pint of lukewarm water for a quarter of an hour, and then the whole of the mess is swallowed, powder and infusion together. Lemonade is recommended to be drunk, both before and after the dose; why, I do not know, unless it be for compensation to the palate. It is well with this, as with every other weapon against tape-worm, to take the enemy at a disadvantage by using the remedy in the morning—an aperient having been premised the day before. The worm is thus less likely to be defended against the access and contact of the drug, by food, or by the intestinal secretions.

Various other remedies have been employed, and employed with more or less success. One of them, the root of the male shield fern, formed the basis of a nostrum, called Madame Nouffer's, which was so highly thought of, that in 1776 the King of France gave that lady some hundred pounds sterling for the secret of its composition. This ancient fern-remedy has had a very fluctuating reputation, in consequence probably of some imperfection in the manner of preparing it. It is, however, a valuable vermifuge, and likely to become the favourite remedy, in this country at least. Bremser thought that it was especially powerful in expelling the *tænia lata*: but against this opinion we may set the experience of Dr. Gull, who in a recent volume of the *Guy's Hospital Reports* gives the results of the employment of an ætherial tincture prepared from the *rhizoma* of the male fern, in 200 cases, among the patients of that hospital. He states it to be a convenient and an effectual remedy, succeeding where other anthelmintic drugs had failed; and out of fifty tape-worms expelled under its use, forty-nine were of the English, and one only of the Swiss species. From a drachm and a half to two drachms of the tincture was the common dose. It caused some nausea, occasionally even vomiting, and then it operated as a purgative. Dr. Willshire confirms Dr. Gull's estimate of this substance, and pronounces it to be less nauseous and irritant than turpentine, more sure than pomegranate, less bulky and less expensive than kousso.

LECTURE LXXV.

Diseases of the Liver. Acute Inflammation. Abscess of the Liver. Causes and Treatment of Acute Hepatitis. Chronic Hepatitis. Jaundice. Its Symptoms, Causes, and Species.

FROM the alimentary canal I go to the other organs directly or indirectly concerned in the digestion and assimilation of our food. And first, let us look at that large gland, the liver, of which the most obvious office is the secretion of bile. Modern science has, however, discovered that it plays an important part in perfecting the process of blood-making. Sugar is formed, and poured into the venous blood between its entrance into the liver through the portal vein, and its exit from the liver through the hepatic vein.

The liver is liable to various forms of disease: but it is not so frequently in fault as many would have us believe. It is often blamed most gratuitously and unjustly.

The researches of Mr. Kiernan, recently published in the *Philosophical Transactions*, have paved the way for a better understanding in future of its pathology. If I did not feel myself too much circumscribed by the remaining limits of this course of lectures, I should be glad to attempt to assist you in construing Mr. Kiernan's somewhat difficult and very valuable paper. But doubtless you are made familiar, in other lecture-rooms, with the anatomy of the organ, as described by him: and I must content myself with noticing, as we go along, one or two points, in respect to its morbid appearances, concerning which, before Mr. Kiernan took the subject up, great mistakes prevailed, even among the most celebrated pathologists.

I shall follow the usual order, and consider first, *inflammation* of the liver, which may be either acute or chronic.

These are, both of them, diseases that are much more common in warm climates than here.

Of well-marked acute hepatitis the symptoms are fever, with pain and a sense of tension in the right hypochondrium, inability to lie on the left side, difficulty of breathing, a dry cough, vomiting, hiccup.

You will not find all these symptoms present in every case: yet they are all worth attending to.

The pain is sometimes sharp and pricking, like a pleuritic stitch: sometimes dull and tensive. In the former case the peritoneal covering of the gland is supposed to be affected; in the latter its parenchyma. The pain sometimes extends to the right clavicle, and to the top of the right shoulder. This circumstance was noticed by Hippocrates; and I have referred to it before as a good example of what are called sympathetic sensations. The existence of this pain makes it probable that the inflammation affects the convex surface of the right lobe of the liver. Occasionally the *left* shoulder is painful: the left lobe of the liver being involved in the disease. The pain in the right side is aggravated, often, by the movements of the diaphragm in respiration; and this explains the embarrassment of the breathing, and the short dry cough. Why the patient cannot well lie on the left side is obvious enough: all the connexions of the inflamed organ are then put upon the stretch by its weight. There are, however, some exceptions to this rule. On the under or concave part of the liver lies the pyloric extremity of the stomach; and that viscus often sympathizes with the hepatic inflammation: nausea and vomiting occur; and hiccup. The thoracic symptoms on the one hand, or the stomach symptoms on the other, may be expected to predominate, according as the convex or the concave surface of the organ is chiefly the seat of the inflammation.

Permanent rigidity of the abdominal parietes, especially on the right side—and, more particularly, rigidity of the right rectus muscle—is a symptom strongly indicative, according to Mr. Twining and other surgeons in India, of deep-seated abscess of the liver. The same symptom was noticed by Dr. Budd in a case of jaundice from closure of the common duct, and in a case where a cancerous ulcer of the stomach had

eaten into the adherent liver: and by Dr. Graves in a case of inflamed gall-bladder. This sympathetic affection is but one instance among many, of that kind of protective instinct whereby a tender part is in some measure shielded against the infliction of pain by pressure.

The situation of the pain, the cough, the short and shallow breathing, used to puzzle the older observers: and they confess their occasional inability to determine whether the inflammation was situated in the lower lobe of the right *lung*, or in the *liver*. But now-a-days we need have no difficulty in making the diagnosis. The ear will tell us, if we employ auscultation and percussion, whether the contents of the chest or of the belly are suffering: and my own experience has taught me that sharp pain, with feverishness, occurring in the debateable ground of the right side, denotes pleuritic inflammation far more often than it denotes hepatic.

Jaundice is an *occasional*, but by no means a *necessary* effect or accompaniment of hepatitis; whether acute or chronic: and, therefore, what I have to say of that symptom I shall give under a separate head.

Acute hepatitis may terminate in resolution; or it may terminate in diffused suppuration; or, what is more usual, in the formation of a circumscribed abscess, or of abscesses, in the liver. In this climate we do not often meet with hepatic abscesses; but they are very common in hot countries: and some of the most interesting events of the disease have relation to the progress of these collections of matter. When they approach the surface of the liver, adhesions generally take place (in virtue of that conservative principle of which we so constantly discern the working) between the diseased organ and the neighbouring parts. If no such adhesion occurred, the matter would at length burst into the cavity of the peritoneum; and this *does* sometimes happen. The peritonitis which is thus, suddenly, set up, is almost always fatal. I referred, however, in a former lecture to one instance in which it was recovered from. Sometimes the adhesion is effected between the liver and the parietes of the abdomen, and the abscess points *externally*, and may be opened by the surgeon. Such a case occurred in my neighbourhood last year. Sometimes the liver glues itself to the stomach, or to the intestines; and then the abscess breaks into the alimentary tube, the matter is evacuated by vomiting, or by stool: and all goes on tolerably well again. In other instances the agglutination is to the diaphragm, which is perforated, and the pus makes its way into the sac of the pleura, or through the lung to the bronchi, and so out by the mouth. I have seen three or four examples of this myself. In one the abscess originated in the formation and suppuration of a hydatid tumour in the liver; and the patient (a woman, she was under Dr. Macmichael's care in the Middlesex Hospital) spat up quantities of yellow fluid, consisting partly of pus, and partly of pure bile. The examination of the dead body ultimately demonstrated the nature of that case. In an instance of a similar kind, which has fallen under my cognizance of late, even while these pages were in the printer's hands (April, 1848), a more fortunate result may fairly be anticipated, although hydatids from the liver have made their way both into the alimentary canal and into the thorax. A surgeon from the country, aged 38, came to my house, and told me the following curious history:

For eight or ten years, at intervals varying from ten to fourteen months in duration, he suffered a series of attacks precisely resembling such as are commonly produced by the passage of a biliary concretion through the excretory ducts of the liver. In May, 1847, just after one of these attacks, while searching for a gall-stone, he discovered among the excretions from the bowels, two or three small hydatids. In July he again experienced for four or five days the same train of symptoms, and then vomited a green hydatid, as large as a pigeon's egg, but more elongated. Soon afterwards a short dry cough set in, with pain in a circumscribed spot on the right side of his back, and also at the tip of the right shoulder. These pains became very severe, and in the first week of August he expectorated, with coughing, matters which were yellow in colour, and bitter in taste; and after another week he spat up a hydatid like a grape. Pressure upon the hepatic region posteriorly, always produced an immediate paroxysm of coughing. This state continued for six or seven weeks, and during that period he expectorated some hundreds of hydatids, of which there are specimens preserved in the Museum of the College of Surgeons. At the conclusion of the fourth week he began to cough up from time to time large quantities of bile. The hydatids

ceased to appear towards the end of November; the bile, in the second week of February last. Once he coughed up four ounces of blood.

When I saw this gentleman he still had some cough, and expectorated mucus streaked with blood, and felt some pain in the situation of the liver. He was recovering his lost flesh. His pulse had never been much accelerated: nay, while he was coughing up bile, and voiding none at all by the bowels, it fell to 49 beats in the minute, and remained at that frequency for five or six weeks, until bile again began to be visible in his stools. During the same period the urine was very dark-coloured, and extremely scanty, not exceeding six ounces in twenty-four hours.

Milk and eggs appeared always to cause a great increase in the quantity of bile secreted and discharged. A similar increase was produced within five minutes of his taking any kind of acid. Magnesia as constantly reduced the quantity.

It is a remarkable part of Mr. L.'s history, that throughout all this illness he carried on a large country practice. For weeks he scarcely slept at all. While at the worst, and living upon a very slender diet, he began for the first time in his life to have acne rosacea of the face.

This brief account I took down from his own lips. The case is so full of interest that it deserves to be recorded in more complete detail.

It is plain that in such instances as these, there must be adhesion of the *lung* to the diaphragm also: but cases have occurred in which the matter burst into the cavity of the pleura, and presently destroyed the patient by suffocation.

The event of a third case is also still *sub judice*. A gardener, forty-five years old, was attacked, four weeks before he came under my care in the hospital, with severe pain in the right loin. Just at the edge of the short ribs, and not far from the vertebral column, on the right side, I found a large elastic swelling, very tender, and of a bluish red colour. As the patient's urine was most remarkably loaded with amorphous deposits, my first suspicions turned towards the kidney. It soon, however, became evident that, although the tumour was below the diaphragm, the parts above that muscle were involved in the disease. The right half of the thorax was dull to percussion; no vesicular breathing was audible there, but some scattered crepitation and bronchophony. Four days subsequently to his admission the patient began, during an effort of coughing, to discharge almost in a stream, from his mouth, a considerable quantity of grey, pultaceous fluid, of the consistence of gruel, and horribly foetid. Altogether the amount of matter thus expectorated was estimated at two quarts. The pain then ceased; and the swelling was observed to be less. After three days more, it was deemed proper to puncture the tumour, and matter was let out having precisely the qualities of that which he had ejected through the mouth. The patient remained eleven weeks in the hospital, the expectoration becoming less unnatural, and the discharge from the abscess gradually diminishing. He then chose to depart. Some months afterwards he applied for re-admission, and again staid with us a week or two. His health had much improved during the interval: but the wound in his back was still open, and he still continued to expectorate somewhat; and yellow bile was frequently to be seen, both on the dressings which covered the puncture, and in the vessel into which he spat. I think it probable that this man may eventually recover, at the expense, however, of a permanently damaged lung.

A Kitchen-maid at the Thatched House Tavern, in St. James' Street, received a violent blow at the back part of the right hypochondrium. This was followed by pain there, and by fever; and at length she began to have cough and dyspnoea, and to expectorate. She was taken into the hospital. Here she brought up an abundance of frothy mucus of a bright yellow colour; not at all resembling the rusty sputa of pneumonia, but exactly of the tint which bile would give to it. At the same time large crepitation could be traced from the bottom to the top of the right lung. I fully expected that this woman would die: but by degrees the yellow expectoration ceased, all the auscultatory signs gradually disappeared, and she recovered perfectly.

I thought, at the time, that this also might be a case of perforation of the diaphragm and of the lung, connected with the formation of an abscess in the liver. And the absence of jaundice, to which the colour of the matter expectorated might be imputed, lends likelihood to that supposition still. But I have subsequently met with a case in which similar symptoms presented themselves, without any hepatic mischief. A gentleman, beyond the middle period of life, was attacked with pain in the right

side, and with fever. He coughed also, and spat up some reddish sputa. Auscultation disclosed the existence of pneumonia in the lower lobe of the right lung. After two or three days the patient became deeply jaundiced; his skin presenting an orange-yellow hue. There was no deficiency of bile in the fæces; nor any pain, tenderness, or hardness in the hepatic region. Very tenacious mucus was expectorated, having a deep grass-green colour, with here and there patches of yellow. The inflammation extended to the upper lobes of the lung, and the patient sank. The lower lobe was found to be in a state of grey softening, or diffused suppuration, and it adhered, by a capsule of recent lymph, to all the parts around it. No appreciable change could be discovered in the liver, which contained rather less blood and bile than usual; nor in the gall-bladder and ducts. The secreted mucus of the air-passages had received its very unusual hue from the bile that circulated with the blood.

The sputa, collected in a basin, presented another appearance, such as I had never before seen. Large pyramidal bullæ projected among them, of green colour and crumpled irregular surface; looking like bells of moulded green glass. And when the summits of these large bubbles were broken through, the bubbles did not collapse; but their brittle walls remained firm, as they might have done if really vitreous.

Rigors occurring during the progress of hepatitis should make us suspect that suppuration is taking place: if the pain be thenceforward mitigated, or exchanged for a sense of weight, and if hectic fever set in, we may be tolerably certain that pus has formed.

Of the several courses taken by a hepatic abscess, that towards the surface of the body is the most common; and it is the only one with which we are able to interfere. And the chief thing we have to look to, is not to interfere too soon. The adhesion of the inflamed organ to the wall of the abdomen is the indispensable condition, not of success only, but of safety, where the question arises of puncturing the tumour. Without such adhesion the pus will be transferred from the cyst to the cavity of the peritoneum; or if the abscess be not reached by the scalpel, that cavity will be laid open. Now it is not easy to ascertain whether there be adhesion or not. Certainly the operation ought not to be attempted until the parts above the abscess are very thin, and are verging to a point; and even then, unless there were some distinct purpose, besides that of saving a little time, some urgent distress or danger to relieve, I think such abscesses might be more prudently left to themselves. Dr. Gregory, of Edinburgh, used to mention a case in which, as fluctuation was palpable, and the tumour pointed, it was proposed to open it; but the patient died, somewhat suddenly, before the operation could be performed; and inspection of the dead body showed that no adhesion existed. Mr. Malcolmson has lately published, in the *Medico-Chirurgical Transactions*, two or three instances of a like kind. These are circumstances which teach us to be cautious about recommending the operation. A plan was proposed and practised by the late Dr. Graves for *producing* adhesion, by making an incision over the centre of the tumour, down to within a line or two of the peritoneum. The same safeguard had been previously devised by Dr. Dick, in India, who made, however, the wound in the integuments by caustic, instead of by the knife.

A case is recorded of the bursting of a hepatic abscess into the pericardium; another into the vena cava. Facts of this kind constitute mere medical curiosities, and have no practical bearing.

It is clear, both from the size of the organ, and from its situation in the body, that an abscess in the liver can never be otherwise than gravely hazardous. Yet many recover from them. Much evidently depends upon the manner and direction in which the pus seeks and finds a vent. The most desirable road for its exit is one which it sometimes takes, but which I have not yet mentioned. It occasionally flows out into the duodenum through the excretory ducts of the liver, when these happen to have been laid open by the suppurating process. Next to this we may hope for its discharge through the adhering parietes of the abdomen; next by a breaking of the abscess into the alimentary canal. The escape of the matter through the air-passages is fearfully perilous; and its entrance into the shut serous sacs, or into the great blood-vessels, almost necessarily fatal.

I have spoken of abscess in the liver, as an event of acute inflammation of that organ. Such inflammation is apt to arise, in tropical countries, after exposure to cold. In any climate it may be excited by a blow, or other mechanical injury. But abscess in the liver seems to be far more frequently the result or other remote causes. You will call to mind those collections of matter which form in the liver, as well as elsewhere, in consequence of suppurative phlebitis. There is, moreover, an acknowledged connexion between hepatic suppuration and *dysentery*. In hot climates the two are often found coincident. It has commonly been thought that, in these cases, the affection of the liver happens first, in the order of time; that the hepatic disease, interfering with the freedom of the portal circulation, occasions congestion of the sub-mucous capillary blood-vessels, and so disposes the membrane to take on inflammation under the influence of any slight exciting cause. Dr. Budd, however, has proved, by a careful collation of a large number of authentic instances, that the relation of these two morbid conditions to each other, is exactly the reverse: that the dysentery is the primary disorder, and the hepatic abscess the secondary; the link of connexion between them being the same as between suppurative inflammation of a vein, and the formation of pus in parts more or less remote. The blood, in its return through the veins which are tributaries of the vena portæ, carries with it, if not pus, yet some vitiating ingredient from the inflamed membrane: and this vitiating ingredient provokes inflammation in the capillary vessels of the liver.

It is not, however, upon dysentery only that suppurative inflammation of the liver is apt to supervene; but upon various other morbid conditions also of the extensive mucous surface from which the returning blood is poured into the portal vein; upon injuries, therefore, (including surgical operations,) to the rectum, and the parts adjoining it, and especially upon ulceration of the intestines, of the stomach, and of the gall-bladder or its ducts. Yet not every form of ulceration: for abscess in the liver does not occur with that half-sloughing, half-ulcerating state of the glands of Peyer, which is so common in one species of continued fever; nor with that curious sort of ulceration of the duodenum, originating apparently in the glands of Brunner, which Mr. Curling has shown to be a frequent consequence of external burns; nor with the intestinal ulceration (still primarily glandular) which belongs to phthisis; nor are such abscesses often met with in connexion with simple ulcer of the stomach.

This, to the best of my knowledge, is a new, and certainly it is an important, view of the pathology of hepatic abscess. You will find, I say, the evidence upon which it is founded, fully developed in Dr. Budd's able and comprehensive treatise upon the diseases of the liver.

Acute hepatitis, when it occurs, requires vigorous treatment in the outset. Our object is, if possible, to prevent suppuration. Blood should therefore be freely taken from the system by venæsection, and from the neighbourhood of the inflamed part by leeches. I will not weary you by going over the old ground that we have so often trodden already, nor repeat observations which have been many times made respecting the methods, and the requisite amount, of this great antiphlogistic remedy. Depletion of the portal vessels may also be indirectly obtained by purgatives; especially by such purgatives as produce copious and watery stools. The neutral salts are therefore proper in this disorder. It may be, as some suppose, that they operate beneficially, as counter-irritants, upon the duodenum; but their effect in draining the veins that feed the vena portæ, and thereby relieving the hepatic congestion, is more obvious and more intelligible. These saline medicines should be much diluted; and their action may be quickened, if that be necessary, by adding the infusion of senna.

After blood-letting has been duly performed, and the force of the inflammatory action has been broken, blisters may be applied to the right hypochondrium: and I believe that repeated blistering is more serviceable than a single blister kept open by savine ointment.

Some difference of opinion prevails among medical men in regard to the employment of mercury in the *outset* of acute hepatitis. I cannot pretend to offer you the results of my own observation on this point, but I find that the best authorities, among those who have had to treat the disease in hot climates, are *against* its use at the *very first*, as being stimulating to the liver. I suspect that this is a piece of theory: but at all events, after the first violence of the inflammation has abated, that remedy is not to be omitted, either in the acute, or in the chronic form of the dis-

order, to be mentioned presently: only in the one case it should be so administered as to affect the system as speedily as possible; in the other it is to be introduced with a slowness which bears a proportion to the place of the disease.

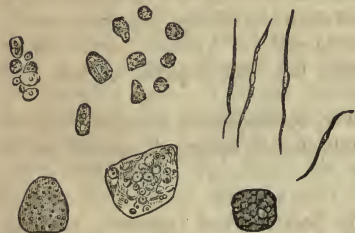
When suppuration has taken place, or is unavoidable, when the patient ceases to complain of pain, but has in its stead a feeling of weight in the hypochondrium, and becomes distinctly hectic, a corresponding change must be made in the treatment. Active depletion is no longer admissible, and mercury must now be avoided: you must sustain the strength by a more nourishing diet, and prescribe some tonic remedies; the sulphate of quina, with sulphuric acid; or the nitro-muriatic acid, which enjoys a considerable repute, greater perhaps than it merits, for the relief of liver complaints.

Acute inflammation of the liver is apt to degenerate into *chronic*. Chronic inflammation may also arise under the circumstances that gave birth to the acute form. Chronic hepatitis, again, is not unfrequently produced by the presence of specific disease in the liver; of carcinoma; of scrofulous tubercles. Melanosis and hydatids are both of them of common occurrence in the same part; and they may give rise to symptoms, or they may not: and when these morbid conditions do declare themselves by external signs, those external signs are very much the same as belong to chronic hepatitis. The precise diagnosis is sometimes exceedingly obscure; the symptoms point distinctly to the liver as the *seat* of the disorder; but as to its *exact nature*, we must often be content with probabilities alone.

The symptoms of chronic hepatitis—or of the chronic forms of disease to which I have alluded, when they show themselves by symptoms—are (I give you them in Cullen's words) “some fulness and some sense of weight in the right hypochondrium; some shooting pains felt at times in that region; some uneasiness or pain felt on pressure in that part; some discomfort from lying upon the left side; perhaps some degree of jaundice; and sometimes a certain amount of fever combining itself with more or fewer of these symptoms.” In short, they are just the symptoms of acute hepatitis occurring in a minor degree.

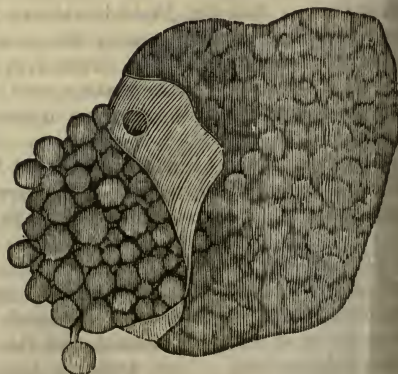
Chronic affections of the liver are sometimes attended with an *increase*, and sometimes with a *diminution*, of its *size*. When it is augmented in bulk, its place and enlargement may be ascertained by palpation and percussion; nay, the magnified gland may sometimes be seen, extending beyond its proper situation in the hypochondrium, and passing far down into the abdomen. I have known the liver reach to the right groin: and when its left lobe is affected, it will sometimes stretch across towards the lower part of the left side of the belly. On the other hand, the liver may shrink into a much smaller space than it naturally occupies. These small livers are usually hard. Interfering more with the portal circulation than many enlarged livers do, they are more frequently attended with dropsy of the peritoneum.

FIG. 132.



Fibres originating from nuclei from fibrous tissue of a cirrhotic liver. Some cells are figured of the natural size, others are very much atrophied.

FIG. 133.



Cirrhosis of the liver. From a specimen in Dr. Gross' collection.

The "hobnail" liver, the *cirrhose* of modern French writers, is nodulated as well as hard. The irregularity of its surface may be so great as to be perceptible to the touch. I fully described this condition of the liver when I was upon the subject of passive ascites, of which it is the most common cause.

When a large round boss can be distinguished, projecting from the surface of the liver, we may speculate upon its being caused by a collection of hydatids; especially if the tumour has arisen without pain, or fever, or any material interference with the general health. When several smaller prominences can be felt, rendering the enlarged liver uneven, and the patient's health is broken, they are probably cancerous; and we search after tokens of cancer, elsewhere. A smooth, globular, painless tumour perceptible by the fingers near the margin of the liver, suggests the likelihood of a distended gall bladder, especially if jaundice concur.

Hydatid tumours may occupy the liver during the greater part of a man's life without causing him much inconvenience or discomfort, and with no perceptible impairment of his health: or, they may keep him in perpetual jeopardy, and at length shorten his existence in one of several ways.

I stated in the last lecture that such tumours sometimes undergo what may be called a natural cure. In the dead body they present themselves apparently shrunken, and containing a dry or pastelike mass, with intermixed fragments of dead and shrivelled accephalocysts. The sac of these old and defunct tumours is often more or less ossified. In this condition they are inert, and comparatively harmless.

But the living tumours may burst, under external violence, or from internal erosion, or perhaps from the increasing pressure of their contents; and their bursting has exactly the same consequences and dangers which I have just pointed out to you as attending the rupture of hepatic abscesses: for the clear liquid contained in the accephalocysts is not less irritant to serous membranes than bile itself. Indeed hydatid tumours may be converted into abscesses, by the occurrence of suppurative inflammation of the inner surface of their proper sac. The experience of Dr. Budd and of M. Cruveilhier concur in teaching that the most frequent cause of such suppurative inflammation is the entrance of bile into the sac through an ulcerated opening in a branch of the hepatic duct imbedded in its walls. Of this mingling of pus and bile and hydatids, and of the perilous passage outwards of these mixed contents of the tumour, through the lungs, and through the stomach and bowels, I have recently given you examples.

Of another possible and fatal consequence of the growth of these tumours you have also had an instance in the case, so often mentioned, of Harriet Baldwin, in whom the pressure of the distended liver completely stopped the return of the blood through the inferior vena cava.

One further point of interest I have to notice respecting these collections of hydatids. Concurrently with one (or more) of them existing in the liver, one or more have frequently been discovered in the lower lobes of the lungs, or in close contact with the spleen, or in some part of the mesentery; and seldom, if ever, in any other place. Now Dr. Budd has shown the great probability that in the majority of such cases the hepatic tumour has been the *parent* of the others: that *secondary* hydatid tumours in the lungs, in the liver itself, or between the folds of the peritoneum, may result from the transplanting, and subsequent growth, of hydatid germs which had found their way, through an ulcerated opening, into the hepatic, or into the portal, vein.

I mentioned some time ago, the "fatty" liver, so frequently found associated with pulmonary consumption. The liver in this state is soft, enlarged, smooth on its surface, and of a buff or tawny colour throughout.

[The *nutmeg* condition of the liver consists, in its best marked instances, of deep red congestion, forming patches and streaks, occupying the central parts of the lobules, and surrounded by patches of a greyish, or dirty white colour. The congested portions are most definitely limited, and the contrast between them and the pale parts is extremely striking. This probably depends on the circumstance that the portions thus devoid of blood are affected with fatty degeneration, a change which, by causing the cells to increase in bulk, occasions compression of the interwoven capillaries. That this is the true cause of the limitation of the congestion, is proved by its exactly

ceasing at the inner margin of the zone of fatty degeneration. The nutmeg appearance may be exhibited, in some degree, by livers which are quite free from fatty degeneration, but it is never so marked as in the latter case. — *Jones and Sieveking: Path. Anat., Amer. Ed., p. 507. — C.*]

Mr. Bowman has lately shown that these changes are owing simply to the unwonted abundance of certain small granules of fat, of which, in the healthy organ, each lobule contains a few only. If in a phthisical patient we find the liver palpably enlarged, and if we can feel no irregularity of its surface or of its edge, we may guess that it is encumbered with this interstitial fat: but there are no symptoms peculiar to the fatty liver. As to its cure, we are quite helpless: and the same may be said of the hobnail liver, as well as of all those forms of disease in which the organ is loaded with specific deposits.

FIG. 134.

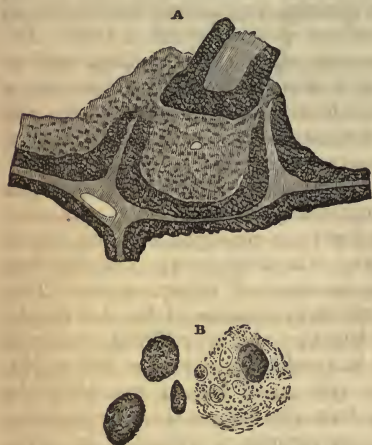


FIG. 135.

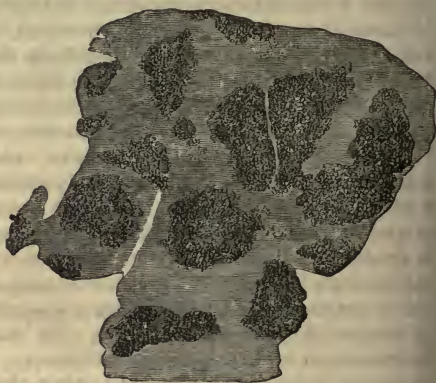


FIG. 134. — (A) Section showing lobules of the liver, bounded by marginal zones in a state of fatty degeneration. The interior of the lobules is deeply congested, and contains much dark yellow pigment in masses.

(B) Cells loaded with pigment, atrophied cells and granular matter from the interior of the lobules.

FIG. 135. — Section of liver in a fatty state, with abundant new-formed fibrous tissue between the lobules.

FIG. 136.



FIG. 137.

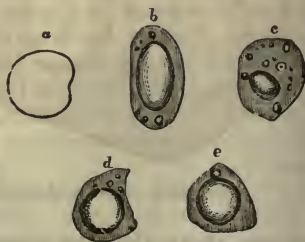


FIG. 136. — Section of liver in an advanced state of fatty degeneration. The cells are much broken up and fused together.

FIG. 137. — (a) Empty envelop of an hepatic cell, from which the oil has escaped.

(b) (c) (d) (e) Hepatic cells containing much oil.

There is, again, the “waxy,” or “lardaceous” liver, of which also I spoke formerly, and which is characterized by the deposition throughout more or less of its substance,

of an albuminous material, rendering it large, and smooth, and of a whitish colour within and without.

This morbid condition, when it can be recognized in its earlier stage, is not, perhaps, quite so hopeless. Both Dr. Budd and Dr. Graves believe that the disease is, sometimes, at least, within the reach of remedial measures: among which, besides careful regulation of the diet and habits of living, the muriate of ammonia, the iodide of potassium, and minute doses of mercury, seem the most promising.

Dark masses of extravasated blood are sometimes found interspersed through this gland, and then, by an absurd perversion of language, the patient is said to have had "apoplexy" of the liver.

The same causes which produce acute hepatitis, acting in a less intense degree will excite chronic inflammation of the same textures. Intemperance also, and particularly the habitual and excessive use of alcoholic liquors, certainly tends to generate hepatic inflammation, especially in its more chronic form. We see this even here, and it is still more strikingly perceptible in warm climates, as you may learn by reading the works of those persons who have had experience of the diseases of India. Dr. William Ferguson, for example, who was for some time chief of the medical staff of the Windward and Leeward islands in the West Indies, observed that there was a regular increase and aggravation of these chronic affections of the liver among the troops after they received their monthly pay, when they drank great quantities of ardent spirits; arrack in the East Indies, and rum in the West.

There has long been supposed to be what is called a *gin-drinker's liver*, in which a section of that gland presents an appearance very closely resembling the section of a nutmeg; and a good deal of useless speculation has been employed as to the nature of the change which has taken place in such cases. The nutmeg aspect of the liver is produced by the congestion of blood, and the retention of biliary matter. This combination is, indeed, very likely to arise under the daily stimulus of distilled spirits, but it arises under various other circumstances besides; and therefore it is no sure indication of intemperate habits. Of this we had sufficient evidence before Mr. Kiernan's observations were published. Again and again have I met with the nutmeggy liver, strongly marked, when there was reason to believe that the possessor of it had never transgressed the strictest rules of temperance in drinking: in young persons, for example, of both sexes, who certainly never had been dram-drinkers. Disease of the heart is a very obvious, and a very common cause of hepatic congestion. The true gin-drinker's liver is that which I have already spoken of as the hob-nail liver.

FIG. 138.

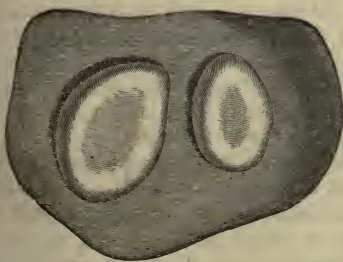


FIG. 138. — Scirrhus of the liver.

FIG. 139.



FIG. 139. — Gin-drinker's liver.

You are aware that the congestion occurs under two forms, according as the branches of the hepatic vein, or those of the vena portæ are gorged; and that the former of these two conditions is very much more common than the latter. If both these sets of vessels are full, the liver is universally red. If the hepatic vein alone be the seat of the congestion, then in the centre of each lobule we see a red speck, surrounded by yellowish matter; the specks are isolated, the yellow colour is arranged in a sort of network. Whereas, if the portal system be greatly engorged, the red streaks will be continuous, and the yellow portions hemmed in by them, and isolated. I show you these distinctions in some specimens prepared by Mr. Kiernan himself.

I should have stated, when speaking of the signs of chronic hepatitis, what is sin-

gularly true of chronic liver affections in general, that they are apt to be attended with much languor and lassitude, and a remarkable depression of spirits; and with that sort of dread, and apprehension of impending evil, which I mentioned as being a striking feature of hypochondriasis: the very derivation of which term marks its frequent connexion with hepatic disorder. There is sallowness of the complexion also; and sometimes emaciation.

The same remedies are adapted to the chronic, as are proper for the acute inflammation of the liver: the comparative mode of their exhibition, however, differs somewhat.

General blood-letting is not often necessary or advisable, except when more violent aggravations than usual of the inflammatory symptoms supervene. Topical bleeding, and blistering, are more expedient.

But the two main remedies to be tried in this complaint are mercury, and the saline purgatives, given in small doses, and repeated for a long time together. Five grains of blue-pill every night, or every night and morning; and as much of the sulphate of magnesia as will produce one or two watery stools every day, for weeks, perhaps, in succession. Patients are not so well content to bear this discipline when it is administered in boxes and phials, *as physio*; but they have more faith in the natural mineral waters: so that a residence at Cheltenham, or some such place, is exceedingly proper to be recommended in these cases; where the daily use of the waters may keep up a continual drain on the system of the vena portæ; and where relaxation from business, the amusements that are constantly going on, with change of scene and of society, may contribute to dissipate the hypochondriacal feelings which are so apt to render the subjects of chronic hepatic disease supremely wretched.

Moderate exercise, in the open air, on horseback and on foot, should be encouraged. There is no doubt that hepatic as well as gastric derangements are fostered by sedentary habits. Tepid bathing is another expedient from which benefit may be hoped. In many instances it will be proper to make trial of Scott's nitro-muriatic bath.

Iodine has been thought of much use in certain kinds of hepatic disease; in those kinds especially which are connected with enlargement of the viscus. The iodide of potassium, or a mixture of the iodide and of iodine, or some of the combinations of iodine and mercury, may be given in such cases: or the *unguentum iodinii compositum*, or the *unguentum hydrargyri iodidi*, of the Pharmacopœia, may be rubbed night and morning upon the hypochondrium. I have not seen much benefit from these forms of medicine myself in such cases; but they are said, by persons of experience and credit, to have been successful in their hands.

Taraxacum is also a drug which has been much employed in liver complaints since Dr. Pemberton's book on the diseases of the abdomen was published: and when well prepared, and taken for a long time together, I believe it often does much good. The Germans are very fond of giving the *muriate of ammonia* in small and frequent doses. They have the same belief in the virtues of this salt, in various disorders, as most English practitioners have in those of mercury; and what is curious, they attribute to it some specific influence upon the functions of the *liver*.

In the account that I have now given of the principal diseased conditions of the liver, I have not dwelt upon, nor included, all the changes of structure and appearance to which that organ is liable. There are various conditions which disclose themselves by no intelligible symptoms during life, of which the nature has not yet been determined, and of which the cure still remains to be discovered. At this advanced period of the course, and with no time to spare, I do not think it necessary or right to trouble you with the unprofitable discussion of matters that are not strictly practical.

I have mentioned *jaundice* as an occasional symptom both of acute and of chronic inflammation of the liver. But jaundice is spoken of, in general, as constituting, itself, a distinct form of disease. If we consider it in that light, its diagnosis is most easy. We have only to look upon our patient to know what is the matter with him. But jaundice depends upon various and very different morbid conditions; and looking to those conditions as the true objects of diagnosis, we find that the real nature of a given case of jaundice is often involved in very great obscurity.

Let us first consider the constituent features of jaundice, whether it be regarded as

a *disease*, or as a *sign of disease*. They are, yellowness of the skin and of the eyes; whitish or drab-coloured fæces; urine having the colour of saffron, and communicating a bright yellow tinge to white linen.

The characteristic yellow complexion is owing, no doubt, to the presence of bile, or at any rate of the colouring matter of the bile, in the circulating blood. And the deep tint of the urine is evidently derived from the same source. On the other hand, the paleness of the fæces is to be ascribed to the want of bile, which always exists in healthy and natural excrement. This last symptom is not, however, a constant one; there may be jaundice while bile appears in the stools. I shall explain how this is supposed to happen presently.

If you ever doubt, as you possibly may, whether your patient be really jaundiced, or only yellowish from sallowness combined with anæmia, you will look especially to the conjunctiva, and to the urine, both of which betray the yellow tint of jaundice very early and conclusively. The eye readily recognizes bile in the urine; but its presence may be ascertained, in questionable cases, by an easy chemical test. If sulphuric acid be added in sufficient quantity, the urine assumes a dark green, and afterwards a purple colour.

It has been made a question how the bile, or its colouring matter, comes to be visibly present in the blood, or rather in many of the tissues supplied by the blood, and in several of the other fluids of the body. The older and more general opinion seems to have been that the bile, after being formed in the liver, is first detained there, or in the gall-bladder, in consequence of some impediment to its excretion, and then re-absorbed, and carried into the circulation, and so conveyed to the surface, and to the parts in which the change of colour is observed. To numerous instances of icterus this explanation may fairly be applied. That bile is capable of being taken up by the absorbents we know: for when the cystic duct is permanently shut, the bile disappears gradually, but entirely, from the gall-bladder. The existence of some positive mechanical obstacle to the efflux of the secreted fluid is often ascertained; and even when none can be discovered after death from well marked jaundice, it is conceived that either the ducts of the liver might have been temporarily plugged up by inspissated bile, or a sort of biliary sand—or else closed, for a time, by spasm, or by some morbid condition of the duodenum.

But another theory has been advanced on this subject: first I believe in this country, by Darwin. It was afterwards revived by M. Chevreul, who has been followed by Mr. Mayo, and others. These pathologists are of opinion that the bile is formed, not by the liver, but in the blood: that the office of the liver is to strain off or withdraw the bile from the circulation, constantly, as fast as it is formed: just as the perpetual elimination of urea from the blood appears to be one great purpose of the kidneys. They hold, therefore, that jaundice manifests itself whenever the due separation of the bile from the blood is suspended or imperfect. Failing of its natural vent, this peculiar substance accumulates in the blood, seeks other outlets, is deposited in various places, and, in fact, partly escapes through unaccustomed channels. They speak of jaundice as a symptom of *suppression* of bile, while others consider it as a sign of *retention*; using these words, suppression and retention, in the sense in which they are applied to the secretion of urine. They maintain that the proper function of the liver, the abstraction of bile from the blood, may be arrested by alterations of the substance and structure of that gland; or by the obliteration or obstruction of the gall-bladder or ducts, impeding or forbidding the removal of the bile already collected; or by some obscure influence of the nervous system upon these organs. They introduce the last kind of cause with the view of explaining those cases, which certainly occur, in which jaundice is the result of severe bodily pain or strong mental emotion. Nay, on their supposition, we might even suppose that the yellowness is sometimes due to a spontaneous and unwonted *abundance* of the elements of bile in the blood: in which case we need not wonder that jaundice should go along with perfect integrity of the biliary apparatus.

There is little reason to doubt that this also is a well-founded theory in respect to some predicaments of jaundice. Dr. Budd even thinks it probable that, in the majority of all cases, that disorder “results primarily, and solely, from the secretion of bile being suppressed or deficient.” Chemistry, however, has never yet detected bile, or its principles, in the blood. Its pigment has indeed been found to be present

in blood drawn from *jaundiced* persons; but this fact has no bearing upon the question how this colouring matter came to be there. Nevertheless there may be—I quite believe there is—such a disorder as jaundice from suppression of bile. But in actual practice it is often impossible to determine, while the patient is still alive, whether the case be one of suppression, or of retention.

I pass from this digression to a somewhat closer examination of the principal circumstances noticeable in the complaint. Its technical appellation, I should observe, is *icterus*, which is the Greek name for a bird with a yellow plumage, the galbula, or golden thrush; the sight whereof, by a jaundiced person, was death (Pliny tells us) to the bird, and recovery to the patient. Various other terms have been applied to the disorder, most of them having reference, like jaundice itself (from the French *jaune*), to the unnatural colour. *Morbus arquatus*, from its exhibiting some of the bright hues of the rainbow; *aurigo*, from its resembling gold; and we hear the common people say, now-a-days, such a one is as yellow as a guinea. The Latins spoke of it also under the title of *morbus regius*: why they so called it we learn from the following curious passage in Celsus, giving an account of the pleasant regimen, fit for royalty itself, to be adopted by those who labour under the malady. “Per omne vero tempus utendum est exercitatione, fritione: si hiems est, balneo; si æstas, frigidis natationibus; lecto etiam et conclavi cultiore, lusu, joco, ludis, lasciviâ, per quæ mens exhalaretur: ob quæ regius morbus dictus videtur.”

The whiteness of the stools I have mentioned as being a very common but not a constant appearance. It clearly depends upon the absence of bile. Such stools have often a sour and very offensive smell. But sometimes there is bile in the discharged fæces, and at the same time the yellow colour of the skin, and eyes, and urine. This probably depends upon the circumstance that some branches of the hepatic ducts are obstructed while the others are free; and thus the bile that is secreted is, in part, re-absorbed into the blood, and in part carried off into the intestines. In a former lecture I stated that one of the uses of the bile appeared to be that of stimulating the bowels to action: it is the natural purgative. Accordingly in most cases of jaundice, the bowels are costive. But neither is *this* uniform. In some of the worst cases, wherein the jaundice depends upon hepatic disease, which is connected with disease also of the mucous coat of the intestines, there is constant diarrhœa.

In some instances the yellowness of the skin is at first attended with itching, which is occasionally so intolerable as to require the employment of opiates to allay it. In most cases there is no itching at all. The bile never fails to appear in the urine, which is in itself dark, and when collected in considerable quantity in a deep vessel, even *black*; and which tinges any white substance that is dipped into it of a bright yellow. The urine which thus sometimes seems black, may be proved to derive that appearance merely from concentration of the yellowness, by pouring a little of it into a shallow white dish, or by diluting it with water; when the brilliant yellow tint will become manifest. Bilious sweat sometimes occurs, staining the patient's linen yellow. The saliva, in some jaundiced persons, has the same yellow tinge, and a distinctly bitter taste. Yet the secretions furnished by the mucous membranes are in general signally exempt from this change of colour. The tongue and inside of the lips present commonly a remarkable contrast with the face, of a jaundiced person. Dr. Budd observes that the mucus of the stomach and intestines is never stained with bile, except when that secretion has continued to enter the duodenum. I have already mentioned two or three instances that have fallen under my own notice, in which mucus brought up from the lungs was rendered green and yellow by bile: but this is of rare occurrence. It has been said that the milk of women who are nursing is made yellow in this disorder. Dr. Heberden, however, states that he never witnessed this; and he had known a woman with a very deep jaundice upon her, suckle her infant for six weeks together with no apparent bad effects upon its health. One man assured him that his tears were yellow. You are aware of the vulgar notion, suggested, no doubt, by the colour of the conjunctiva, that to a jaundiced eye all things appear yellow. It is an old notion, for we find it expressed by Lucretius:—“*Lurida præterea fiunt quæcunque tuentur Arquati.*” Heberden was disposed to regard this as a mere poetical fiction. But certainly it is sometimes, though very rarely indeed, a fact. Two women, whom he considered however to be of little credit, told Heberden that objects appeared yellow to them. I have been assured of the

same thing by a medical man who experienced it in his own person. If I do not mistake, Dr. Mason Good saw all things yellow when he was jaundiced. Dr. Elliotson has had some very interesting cases of this phenomenon. One of his icteric patients declared that objects seemed yellow when looked at with one eye, but not with the other; and in the eye that perceived the yellow tint he observed two large red vessels running towards the cornea. And in one or two instances, which he met with afterwards, of yellow vision with both eyes in jaundiced patients, he found inflammation, or distended blood-vessels, in both eyes. This very morning I saw in the hospital a patient of Dr. Wilson's, a middle-aged woman, affected with jaundice. She affirms that all objects seem yellow to her vision. In both eyes there are several varicose and singularly tortuous vessels, proceeding across the sclerotica towards the cornea, and some of them reaching its margin. It seems probable therefore that the ophthalmic vessels, in their natural state, do not permit the colouring matter of the bile to pass through them; but that when they become enlarged by disease, so as to admit the colouring particles of the blood, they may also give a passage to the yellow colouring matter, which tinges the humours of the eye: and in that case the objects seen through the yellow fluids would appear like those viewed through a piece of yellow stained glass. This is a point which is worth your attention in future.

The shades of yellowness are different in different patients. Those who are pale and fair present a bright lemon colour. But in those who are florid, or whose cheeks and skin are flushed with fever, the tint will more resemble that of the Seville orange. Again, if the patient be naturally swarthy, or if his visage be livid or dusky through imperfect arterialization of his blood, the superaddition of jaundice will give him a greenish or olive hue. These differences result from natural or acquired differences of complexion, antecedent to the icterus. But sometimes the bile that is re-absorbed is vitiated and dark; and we may have, for that reason, as Dr. Baillie has pointed out, cases of green or black jaundice. You will remark that from whichever cause the green or dark colour proceeds, whether from a mingling of the yellowness of the bile with the blueness of lividity, or from the circulation of green-coloured bile, such cases are especially unpromising cases.

Icterus depends, as I have said, upon various and different internal causes: and frequently we cannot determine at all, until death affords us the means of inspecting the parts concerned in its production, what the precise exciting cause may be; even when it is simply mechanical. Any kind of pressure made upon the excretory ducts of the liver will produce it: and such pressure may be exercised by tumours seated in the liver itself; or by a scirrhus pylorus; or by specific disease situated in the head of the pancreas, of which I have seen several examples; or by a diseased condition of the duodenum: and these possible causes of a detention of the bile in its receptacle should always be borne in mind when we are investigating an obscure case of jaundice.

The impediment, in the cases just supposed, is external to the ducts; but they may be obstructed within, plugged up by mucus, by inspissated bile, or by a biliary calculus. This forms one of Cullen's species of icterus—the *icterus calculosus*. The concretion is most commonly situated, I believe, in the ductus choledochus; sometimes, however, in the cystic, and sometimes in the hepatic duct. The pain that attends the passage of a gall-stone through these ducts is often dreadful. Perhaps there is no pain to which the body is subject that is more severe. You will not wonder at this, when you consider that through a tube, of which the natural size scarcely exceeds that of a goose-quill, there sometimes passes a stone as big as a walnut. The common duct has been found so dilated as readily to admit one's finger. Cullen's definition of this species is "*Icterus, cum dolore in regione epigastricâ, acuto, post pastum aucto, et cum dejectione concretionum biliosarum.*" Now the last of these circumstances, the

FIG. 140.



Gall-bladder and cystic duct, containing calculi, which have a crust of pure cholesterin. The two upper are divided. — From Dr. Budd's work.

voiding of biliary calculi by stool, may happen over and over again, without its being noticed, and it does not help us at all to judge of the nature of the complaint at its commencement, while the gall-stone is still within the ducts. With the pain, which is not constant, but comes and goes, there is much nausea and vomiting; and sometimes hiccup; and the matters vomited are usually very sour. The patient is flatulent, and dyspeptic; languid, and gloomy. At length the concretion passes into the intestines; the pain suddenly ceases, and all is soon well again. Attacks of this kind, having happened once, are very apt to be repeated.

Now this pain you might readily mistake for the pain of inflammation, were it not marked by these two circumstances—the absence of tenderness, and the absence of fever. Pressure, instead of augmenting, usually mitigates it. The patient keeps his hand firmly applied to his epigastrium; or rests, perhaps, the weight of his body upon some hard substance placed beneath his stomach. I speak now of the beginning of the attack, before there has been much retching; for a degree of tenderness of the abdominal muscles is often produced by repeated straining and vomiting. The pulse is unaffected, or I should rather say it is not accelerated, during the pain: occasionally it is even slower than natural, and the skin cold. Though there be no inflammation, rigors may occur; just as they sometimes happen when a solid substance—a bougie to wit—is passing through, and distending the *urethra*.

Nevertheless, inflammation does sometimes arise, and then the pulse becomes frequent, and the skin hot, and thirst and head-ache are complained of, and the epigastrium is tender; and if blood be drawn it exhibits the buffy coat. Sometimes the gall-stone makes its way, by ulceration, through the contiguous structures, and so is discharged outwardly, or into the bowels. In such cases there must have been inflammation.

FIG. 141.



Calculi in the gall-bladder. From a preparation in Dr. E. Wallace's possession.

As jaundice often occurs without any pain, so a gall-stone may enter and pass through the ducts, and produce pain, when there is no jaundice. The cystic duct alone may be blocked up, and that portion only of the bile be prevented from escaping which is accumulated in the gall-bladder. It is probable that re-absorption of the contents of that cistern is not very active. Or a calculus of an angular shape may stick in the common duct, and thus

impede, without entirely stopping, the egress of the bile. Dr. Heberden thought that gastrodynia was not unfrequently owing to biliary concretions: founding his opinion upon the fact that many persons suffer, for months or years, under occasional attacks of epigastric pain, which is at last associated with jaundice. But after all, this might happen from progressive disease in the stomach itself; and it is a pity that Dr. Heberden's views were not fortified by dissections.

When once a large calculus has forced its way through the natural channels of the bile, they remain permanently dilated; and smaller stones may be afterwards voided without pain or other notice of their passage. There are persons who get rid of scores of them in this way, during the course of their lives.

Sometimes a large concretion, after its extrication from the biliary passages, lodges in the more capacious intestines, and gives rise to serious obstruction there. I mentioned, recently, one case of this kind which had fallen under my own notice. But in general the concretions are presently voided with the stools: and they should always be looked for. The patient is much gratified by *seeing* that his enemy has been expelled; and also by the proof he thus obtains of the sagacity and judgment of his physician. The *faeces* should be mixed with water, upon the surface of which any gall-stones that happen to be present may probably float, since they are specifically lighter than that fluid. It is justly remarked, however, by Dr. Budd, that although lighter than water when dry, they sink after having been soaked in it for some time, and do not always float when fresh from the gall-bladder. I formerly told you that I had never but once succeeded in catching a concretion in the evacuations of a patient,

whose symptoms had led me to seek for it. Since that statement was made, three other patients of mine, taught how to search, have detected among the alvine discharges, this palpable source and explanation of their previous sufferings. One of the three collected, in this way, fifty-five small faceted biliary calculi, which he voided within the space of five weeks. He was jaundiced; and he began to pass them, four or five at a time, with paroxysms of severe pain, just after having had the hepatic region diligently shampooed and kneaded as he lay on his left side in a warm bath; so that they seemed to have been mechanically pressed out of the gall-bladder, and through its ducts.

When concretions pass which are small and angular, having several flat surfaces, we are to expect that more will follow them. If a single stone come, large, smooth, and roundish, we may hope that it has left none behind it.

We often find gall-stones, even in vast numbers, in the gall-bladders of persons who during their life-time had never been known to suffer pain about the liver, or to have jaundice, or to exhibit any token of the presence of such concretions. We infer from this that, while they remain in the reservoir of the bile, they are harmless; and that the suffering and the hazard they occasion are mechanical consequences of their transit through the gall-ducts. I have heard of an instance in which upwards of 1,300 gall-stones were taken from a human gall-bladder after death.

On the first day of June, 1854, a remarkably strong and healthy man, between 60 and 70 years old, with whom I had for many years been on terms of friendship, was engaged in inflating an air-bed by blowing with his mouth through a tube. The process required a long-sustained straining effort, and he suddenly felt that he was hurt in the right side of the epigastrium, near the edge of the false ribs. After a while the pain ceased, but it recurred with severity in the night, and the next day his skin was slightly yellow. Bating some trifling and early variations in its tint, the yellow colour became deeper and deeper, and he remained intensely jaundiced till the day of his death, which was the 29th of the following August.

During his illness, his liver gradually became very large and prominent. His stools were devoid of bile, and his urine was charged with it. The pains continued to return from time to time. He had been subject to similar pains in his youth, and had been taught to ascribe them to "spasm of the diaphragm."

There was a good deal of fat about the abdomen. He did not die of inanition. The liver was enormously enlarged, and full of bile: and the gall-bladder, which was much thickened, was filled with numerous calculi, and moulded, as it were, upon their irregular form and outlines. One large gall-stone, something like a horse's hoof in shape, completely plugged up the opening of the common duct into the duodenum. Branches of the hepatic ducts in the liver were so distended as to present little reservoirs of bile and mucus. It seems probable that some of these calculi were of very old date. Here life was extinguished in thirteen weeks, by the mere occlusion of the biliary ducts, and the complete barring up of bile in the liver. Dr. Budd relates a case in which the body was tolerably well nourished for more than twelve months, although the common gall duct was closed all that while by a gall-stone; and another in which a woman, suffering under a similar obstruction, lived more than eight months in a state of deep jaundice, and five months after the occurrence of the jaundice gave birth to a child, which she was able to suckle up to the time of her death.

These gall-stones are not, as you might suppose, mere lumps of inspissated bile. There are, I believe, concretions of that kind, but they are very rarely met with in the human subject. The ordinary calculi consist, in a great measure, of a peculiar substance, *cholesterine*, which exists in a state of solution in healthy bile, but which in some morbid conditions of that fluid, being released from its solvent, assumes its proper crystalline form. Very little is known respecting the circumstances under which the change takes place. Cholesterine, Dr. Prout tells us, is the product of some modification of the oleaginous principle. Biliary concretions seldom form in children. They are much more common in women than in men. They occur most frequently in persons who are corpulent, lead sedentary lives, use generous fare, sleep

FIG. 142.



Small, irregular gall-stones, composed of inspissated and altered bile cemented by mucus.—
From Dr. Budd's work.

Fig. 143.



From a gall-bladder, which was shrunken, a calculus being impacted in the cystic duct.

(a) Cholesterin tablets.

(b) Glomeruli.

much, and neglect their bowels: all which things foster or denote a torpid and congested state of the hepatic system. Cattle are said to be subject to biliary calculi when shut up in stalls during the winter, and to lose the complaint when they are again turned out into the pastures in the spring. Hence the absurd notion, countenanced even by Van Swieten, that grass is a good remedy for jaundice.

Another variety of jaundice, also noticed by Cullen, is supposed to depend upon mere spasm of the gall-ducts. "*Icterus spasmodicus*, sine dolore, post morbos spasmodicos, et pathemata mentis."

Now the existence of this cause is hypothetical. The gall-ducts, though not distinctly muscular, possess a vital power of contraction. I am not aware that the disease has ever been clearly traced to a connexion with "*morbi spasmodici*." It is an alleged cause which we can neither prove nor disprove. Certainly the "*pathemata mentis*" play their assigned part: fits of anger, and of fear, and of alarm, have been presently followed by jaundice: and it has also been produced by great bodily suffering, by a severe surgical operation, or, perhaps by the dread which attended it. Mr. North witnessed a case in which an unmarried female, on its being accidentally disclosed that she had borne children, became in a very short time yellow. A young medical friend of mine had a severe attack of intense jaundice, which could be traced to nothing else than his great and needless anxiety about an approaching examination before the Censor's Board at the College of Physicians. There are scores of instances on record to the same effect: and *this* is observable of such cases, that they are often fatal, with head symptoms: convulsions, delirium, or coma, supervening upon the jaundice. But with respect to the immediate cause of the icteric symptoms, they may, I say, depend upon a spasmodic constriction of the gall-ducts. Mr. Mayo suggested another cause, viz., the sudden formation of bile in unusually large quantity in the blood, by some influence propagated through the nerves. Dr. Budd is of opinion that in the cases which prove fatal, with delirium and stupor, some peculiarly poisonous matter is evolved in the system, most probably from decomposition of the retained elements of the bile.

Jaundice sometimes succeeds violent and long-continued vomiting; in which case the extremity of the gall-ducts is supposed to have been compressed by the coats of the duodenum.

Jaundice may also occur, as I stated before, as a symptom of acute or chronic inflammation of the liver; and then its treatment will merge in that of the primitive disease which occasioned it.

A high atmospheric temperature, long continued, appears to have a decided influence in producing certain forms of this disorder. I was struck with the frequency of a mild and manageable kind of jaundice, which affected young persons, chiefly females between ten and sixteen years old, in this town, in the autumn of 1846, just after the prevalence of extremely hot weather.

Icterus occasionally comes on during pregnancy; and disappears after child-birth. The pressure of the gravid uterus may thrust other organs, a loaded colon for example, against the liver, and so impede the passage of the bile. The little exercise that pregnant women are apt to take, and the costiveness that frequently attends their condition, may have some influence in causing the *icterus gravidarum*.

Almost all systematic writers follow Cullen in making jaundice a common disorder among newly born children. The *icterus neonatorum* occurs, they say, a few days after birth; is not attended with any suffering, or obvious disturbance of the bodily functions; and soon disappears. Now there seems reason to believe that this is not icterus at all; and has no relation to the biliary organs. The surface of the infant, at its birth, is frequently of a deep red, from hyperæmia or congestion of blood; presenting a condition which falls little short of a mild but universal bruise. By degrees the redness fades, as bruises fade, through shades of yellow into the genuine flesh-colour. Such, I am assured by those who are more conversant with these matters than myself, is the pathology of the *icterus infantum*. Of course true

jaundice may, as well as most other complaints, befall the earliest period of life; but I conceive that it seldom does.

The prognosis in jaundice is generally favourable; except when it depends upon structural disease of the liver, or supervenes suddenly upon some great mental or bodily shock. In both these cases the prognosis is bad, or doubtful. It is better, in that variety connected with hepatic disease, if this disease proceed from some known cause, by which a low degree of inflammation has been produced; and the cause be such as can be avoided for the future. Just, indeed, as in chronic hepatitis, of which the icterus is simply an occasional symptom. The prognosis is worst of all in old persons, when the constitution is impaired, and there is no obvious cause for the disease; and particularly when the colour of the skin is greenish, or approaching to black.

LECTURE LXXVI.

Treatment of the various Species of Jaundice. Diseases of the Gall-bladder; of the Spleen; of the Pancreas. Diseases of the Kidneys. Nephritis and Nephralgia. Phenomena constituting a "fit of the Gravel." Different kinds of Gravel. Diseased states of the Urine. Description and Remedies of the Lithic, Phosphatic, and Oxalic Diatheses.

IN the last lecture, after describing the symptoms, causes, and treatment of acute and chronic inflammation of the liver, and after pointing out various other forms of chronic disease to which that organ is obnoxious, I spoke of *jaundice*. I offered you some comments upon its phenomena; and I indicated several different internal conditions upon which it may, in different cases depend; and the lecture was closed with some brief hints respecting the *prognosis* of icterus. I have yet to consider the plans of treatment best adapted to the several varieties of the complaint.

Some kinds of jaundice are absolutely and obviously irremediable. In these take care not to *harm* your patient by senseless routine formalities. From others the patients recover, whatever treatment may be adopted, or without any treatment at all. Hence, as is customary in such circumstances, remedies the most worthless and absurd are extolled for their efficacy against jaundice. The patient gets well, and the drug last tried is held to have cured him. *Post hoc, ergo propter hoc*, is an argument more often applied I believe to the variations of disease, than to any other class of events.

In that species of icterus which occurs, sometimes, in connexion with acute or chronic inflammation of the liver, the treatment must be such as I yesterday recommended for acute and chronic hepatitis. Mercury forms an essential part of that treatment; and, unless the disease yielded sooner, I should urge the remedy until its effect upon the gums, and, therefore, its presence in the circulating blood was apparent.

But to the icterus calculosus, mercury is not so well adapted. What we want is, not a more plentiful or a more healthful secretion of bile, but to get rid of the mechanical impediment to its excretion; or, at any rate, if that cannot be accomplished, to ease the acute sufferings of the patient. Should fever attend the passage of a gall-stone, or should the epigastric pain become epigastric tenderness, leeches may be applied, or a vein may be opened. The abstraction of blood may prevent any thickening of the distended gall-ducts; or it may perhaps relax the spasmodic closure around the calculus. But, in general, blood-letting is not requisite nor of service in this variety of jaundice. Our great resource for relieving the pain, and for loosening the presumed spasm, is opium, given in full doses: and I can add but little, with any advantage, to the directions laid down, on this head, by Dr. Heberden. "This pain (says he) can only be assuaged by giving and repeating opium and its preparations,

as often as the continuance of the pain requires them; and because this pain is very apt to return, the patient should always be advised to keep by him, as long as the distemper lasts, pills of pure opium, each weighing one grain—or what is equivalent to them—that no time may be lost in quieting a sensation which it is so difficult to endure. One of these pills may be taken as soon as the pain comes on, and it may be repeated once or twice in the course of two hours, if the pain require it; and I have often found it both safe and necessary to give much more.”

This plan of giving opium in the form of *pills*, is the more judicious, because, from their small bulk, they are more likely to be retained than draughts would be. Sometimes the stomach is so irritable as to reject even a pill. I would add, therefore, to Dr. Heberden's recommendations, that of throwing an opiate injection into the rectum; half a drachm or a drachm of laudanum, mixed with a small quantity of warm gruel. Another very useful expedient is the warm bath. If this cannot be readily procured, hot fomentations to the epigastrium, the mustard-poultice, the turpentine stupe, are valuable substitutes for it. Dr. Prout states that he has seen more alleviation afforded by large draughts of hot water, containing the carbonate of soda in solution (one or two drachms to a pint), than by any other means. “The alkali counteracts the distressing symptoms produced by the acidity of the stomach; while the hot water acts like a fomentation to the seat of the pain. The first portions of water are commonly rejected almost immediately; but others may be repeatedly taken; and after some time it will usually be found that the pain becomes less, and the water is retained. Another advantage of this plan of treatment is, that the water abates the severity of the retching, which is usually most severe and dangerous when there is nothing present upon which the stomach can react. This plan does not supersede the use of opium, which may be given in any way deemed most desirable; and in some instances a few drops of laudanum may be advantageously conjoined with the alkaline solution, after it has been once or twice rejected.” The pain having been quelled, the bowels should be swept out by a brisk purgative.

When jaundice appears to have been suddenly engendered, by moral causes, the rationale of its production is obscure; and the treatment has a corresponding uncertainty. The bile, retained or readmitted into the blood, is supposed to operate somehow as a poison upon the nervous system. But the mental state which precedes and seems to occasion the jaundice, may possibly be itself the cause of the nervous symptoms that follow. In other forms of the malady patients remain intensely yellow, often for a long time together, without becoming comatose, delirious, or convulsed. Not that this is conclusive. We know that a given poison may influence different persons, very differently. The same dose of opium that will put one man to sleep, will stimulate a second to madness, and will have no sensible effect upon a third. In the very complaint before us, one patient is tormented with a universal itching, which we attribute to bile in his blood; and ten others remain free from that disagreeable feeling. If we were sure that the bile was the material cause of all the cerebral symptoms, we might hope to draw some of the poison off by blood-letting: but we are not sure of this; and reasoning upon the matter helps us not much towards the cure. The lesson which experience has furnished amounts to no more than this: that active purging is sometimes followed by evident amendment, and ultimate recovery. I would bleed also, if the *pulse* warranted venæsection, but not otherwise. In all the varieties of what, from its intensity and rapid accession, I may call acute jaundice, purging is strongly indicated: and we sometimes succeed in rectifying the whole morbid condition by thus applying a sudden *wrench* (so to speak) to the biliary organs; by giving, for instance, half a scruple or a scruple of calomel, and, a few hours afterwards, half an ounce of castor oil, with half an ounce of spirit of turpentine.

When *green* jaundice arises from hepatic disease, we can only palliate. Mild laxatives and anodynes, with occasional warm baths to promote perspiration, comprise all that such a state admits of. For the *icterus gravidarum*, delivery is the natural cure: it may sometimes be removed by the careful employment of aperients.

[Mr. Twining, in his work on the Diseases of Bengal, has presented some very interesting and important views in relation to the pathology of jaundice.

Mr. T. has found that jaundice, not only during its early stage, but for a long period subsequently, while the discoloration of the skin remains, is very generally

attended with some morbid sensibility when pressure is made over the situation of the gall-bladder and capsule of Glisson, though the uneasiness, during the absence of pressure, is most generally referred to the epigastrium.

When the disease occurs in plethoric subjects, and the stools are of a pale clay colour, Mr. T. has found it almost always attended with fever, and in some cases he has known robust patients to die, with symptoms of oppressed brain, within thirty-six hours after the sudden appearance of intense jaundice, for the accession of which no cause could be assigned.

In consequence of the acknowledged obscurity which exists in regard to the true pathology of the disease, he was anxious to ascertain the exact condition of the liver and biliary ducts in persons labouring under jaundice. The almost invariable existence of pain, increased upon pressure, confined to a circumscribed spot on the right side, just below the centre of a line drawn from the right nipple to the umbilicus, led him early to suspect circumscribed inflammation of some part of the liver as the most frequent cause of the disease. This opinion he believed to be confirmed by the good effects which, in the majority of cases, are produced by a systematic course of depletion: nevertheless, if circumscribed inflammation be the efficient cause of jaundice, he found it difficult to explain why the disease was so frequently absent during the progress of the most unequivocal and intense inflammations of large portions of that organ. The examination of individuals who had recovered from jaundice only a short time before death occurred from other diseases, exhibited no appearances in the liver indicative of any circumscribed portion of it having been the seat of recent inflammation. In the course of his dissections, however, Mr. T. found that albuminous infiltration into the cellular structure of the capsule of Glisson was sometimes present. Within this capsule are situated two small bodies, which, from their structure, appearance, and uniformity of situation, he is inclined to believe are absorbent glands: one of them is situated near the termination of the gall-bladder in the cystic duct, the other, at the upper part of the ductus communis; the superior gland is sometimes very small, and occasionally, it is more closely attached to the side of the gall-bladder than to the cystic duct; the lower one is more uniform in bulk, being usually half the size of a small bean; it is always placed just at the orifice of the common biliary duct. Irritations affecting the absorbent vessels passing through this gland may, Mr. T. conceives, cause in it such a degree of swelling as would produce transient compression and closure of the common duct, and thus prevent the passage of the bile into the intestines, and give rise to the phenomena of jaundice. So long as the obstruction is complete, the stools will be nearly white, or of a very pale grey colour; when, however, the jaundice is attended with severe fever and symptoms of intense gastro-enteritis, the stools will very often be coloured by the blood which is poured out by the capillary vessels of the intestinal mucous membrane, as well as by other morbid secretions.

When previous inflammatory disease of the capsule of Glisson has caused an infiltration of coagulable lymph into the cellular structure of that part, at a remote period, and its subsequent absorption has left a degree of induration and constriction, Mr. T. believes that a very slight enlargement of the lower gland will effectually compress the common biliary duct; he has seen its canal obliterated, from this cause, exactly at the point of contact with the swollen and indurated gland. He has, also, seen the cystic duct obliterated where it was in contact with the upper gland, in consequence of the enlarged and diseased condition of the latter; but he does not believe that this could have any influence in the production of jaundice.

Mr. T. does not deny the agency which biliary calculi — tumours of the pancreas, liver or spleen, or scirrhus pylorus may occasionally have in the production of the phenomena of jaundice, nor that, in some rare cases, the disease may have been excited simply by mental emotion; nor is he willing to place undue importance on the observations upon which his views of the most frequent cause of the disease are based. Should these, upon more ample investigation, be found to be correct, they will have an important influence upon the treatment of the disease.

According to Mr. T., the most successful plan of treatment in those cases of jaundice accompanied with pain, augmented upon pressure, of the right side of the abdomen, is by depletion by the lancet and leeches — active purgation — the daily use of the warm bath — and sudorifics, aided by low diet and perfect rest, in the commencement of the disease; followed by milder purgatives, and a small blister over the

region of the gall-bladder, kept open for a long time. Subsequently, a course of Cheltenham salts, or small doses of rhubarb and sal. polychrest, with gentle exercise, and frictions with camphorated liniment over the right hypochondrium are advisable: at the same time, it may be proper to allow a mild unirritating diet, in such quantities as shall improve the patient's strength. The disease may sometimes occur under circumstances that forbid depletion. This Mr. T. considers an unfortunate circumstance, as he has but little confidence in other modes of treatment.

In those cases in which the stools indicate the entire absence of bile from the intestines, Mr. T. considers the use of mercurials to be of doubtful propriety; but, when bile does pass into the duodenum, he admits that calomel may be useful, in conjunction with the remedies already detailed. Excepting in cases where there is pretty positive evidence of the existence of biliary calculi, which he believes to be less frequently a cause of jaundice than is generally imagined, he doubts the propriety of having recourse to opiates.

Whether the views of Mr. Twining in regard to the pathology of jaundice be or be not correct, must be determined by the result of future and more extended observations; they are sufficiently plausible and important, however, to demand the attention of the physician, and to influence him in his treatment of the disease. In respect to the plan of treatment recommended by Mr. T., we believe it will be found, with one exception only, to be, in a large number of cases, the only proper and successful one. The exception to which we allude, is the *indiscriminate* administration of active purgatives; jaundice is, not unfrequently, connected with more or less extensive inflammation of the stomach and small intestines, and in these cases, the employment of active purgatives to the extent recommended by Mr. T. would unquestionably be decidedly injurious.—C.]

The *gall-bladder* has its own diseases, which I do not stop to investigate, for they seldom become the objects of specific treatment. Sometimes it is found shrivelled up, and nearly empty; sometimes enormously distended; sometimes ulcerated; sometimes ruptured. Of these conditions, the distension of the gall-bladder is the only one that we can ever expect to recognize in the living body. The bag then projects beyond the edge of the liver, and is palpable externally, forming an elastic tumour in the right side. Authors lay down marks for distinguishing a distended gall-bladder from abscess of the liver, and from a hydatid cyst; but they are not much to be trusted in; nor is the precise diagnosis of any great moment. The practical rule seems to be that, when the swelling is adherent to the parietes of the abdomen, we may puncture it, whatever be its nature: but under no other circumstances.

I have already, incidentally, described most of the morbid states of the *spleen* which are susceptible of relief from medicine: especially the enlargement of that body constituting the *ague-cake* of the fens, and occurring in connexion with intermittent fever; and that other kind of enlargement which sometimes goes along with hæmatemesis and melæna. The spleen is liable to tubercles also; to deposits of other specific tumours, and of bone; and to softening of its substance. Sprinkled through it may not unfrequently be seen a number of yellowish or buff-coloured spots, which Dr. Kirkes believes, with good reason, to be little masses of fibrin, detached by the circulating blood from the interior of the heart, and arrested in the capillaries of this, and of some other organs, and especially of the kidneys. I mentioned these spots in a previous lecture.

The spleen is sometimes enlarged by the deposition within it of a whitish substance, albuminous in character, but called by some lardaceous, from its resemblance to hard bacon; by others waxy. The Malpighian bodies are first affected. This condition is the more interesting because it is apt to occur in the liver also, and in the kidneys. It is frequently found in more than one, or in all, of those organs at the same time. Hence we infer its origin from some constitutional cause. It is supposed to belong to the category of scrofulous disorders: and it has often been met with in persons labouring under strumous caries of the bones.

Going along with certain other enlargements of the spleen (or of other glandular bodies belonging to the lymphatic system) there has been observed in the blood a greatly increased ratio of the white or colourless to the red corpuscles. Professor

Hughes Bennett, of Edinburgh, who was the first to draw attention to this remarkable state of the blood, has given to it, or to the disorder which it constitutes, the name of *Leucocythemia*, or white-cell blood. As yet this disorder possesses more of physiological than of practical interest; and I must limit myself to this passing notice of it.

The best remedy for the ague-cake is the remedy for intermittent fever, *quina*. Purgatives also have the effect of reducing hypertrophy of that curious organ. One caution enforced by Dr. Abercrombie is that, in splenic disease, mercury should be sedulously avoided, or rather such an employment of mercury as would risk tenderness of the gums. Dr. Robert Williams, of St. Thomas's Hospital, has stated, that having made many trials of the *bromide of potassium* as a remedy in various disorders, he had satisfied himself of its utility only in cases of diseased spleen. Of this I know nothing.

Again, it may seem a slight to the *pancreas* to pass it over without noticing the diseases to which it is subject. But really those diseases appear to be but few; and they do not signify their existence by any plain or intelligible signs. I have, nine or ten times perhaps in my life; met with carcinomatous deposits in the pancreas. In every instance the head of the gland, that extremity which lies next to the bowel, has been the exclusive or the principal seat of the disease. I have known this change in the pancreas to cause jaundice, by obstructing the bile-ducts; I have known it in the same way to occasion very great enlargement of the liver itself; and I have known it to produce enormous and slowly fatal distension of the stomach by compressing the duodenum, and so preventing the free passage of the aliment through that gut. As to remedies for pancreatic diseases or disorders, I do not know of any.

Diseases of the *kidneys*—and disorders of their function—and alterations in the fluid they secrete—require more consideration. And I proceed at once to the subject of their *inflammation*; to *nephritis*; and it will be practically convenient to take *nephralgia*, or pain of the kidney, into the account at the same time. Nephralgia is commonly, but not always, produced by the transit of a urinary calculus from the pelvis of the kidney, through the ureter, towards the bladder. This constitutes what is called, in common parlance, *a fit of the gravel*. The symptoms are these:—pain, sometimes dull, but more frequently very severe, in the loins, usually on one side, and descending often along the track of the ureter of the same side; numbness of the corresponding thigh; in the male, retraction, and perhaps pain, of the testicle; a frequent desire to make water, which is generally high-coloured; nausea and vomiting.

If to these symptoms there be added pyrexia, we learn the important fact that inflammation is present: we have the symptoms of *acute nephritis*. The passage of gravel from the kidney sometimes does, and sometimes does not provoke inflammation of the gland. Nephritis is very seldom idiopathic. It may sometimes arise under the influence of cold; more frequently it is excited by calculous matter lodged in the kidney; by a blow or fall upon the loins; by the internal administration of cantharides, or of turpentine. It is to the presence of fever that we look, to establish the inflammatory character of the renal affection.

Nephralgic pains require to be distinguished on the one hand from rheumatic, and on the other from colic pains. In lumbago there is pain in the back, and it may or may not be attended with fever; but the pain usually affects both sides, and is aggravated by such movements of the body as call the muscles of the loins into action, particularly by stooping. It originates, frequently, in some strain or effort, of which the patient is made painfully conscious at the time. It is seldom accompanied by any notable trouble of the urinary functions. When rheumatic pain extends from the back into the thigh, it mostly follows the course of the great sciatic nerve, and is felt down the outer part of the limb; whereas the pain that accompanies nephritis or nephralgia shoots rather along the track of the anterior crural nerve. Lastly, lumbar pain, depending upon rheumatism, is not attended with nausea and vomiting.

The pain of colic is often associated with sickness and retching; and it may occupy those parts of the abdomen which correspond to the place of the ureters. The urinary functions are undisturbed; and this is a capital point of distinction. The

numbness of the thigh, and drawing up of the testicle, are sufficiently characteristic, when they happen; but they are frequently altogether absent.

Some years ago I was sent for by an exceedingly intelligent surgeon, who had been one of the house-surgeons at the Middlesex Hospital. I found him in bed. He told me he had pain in the abdomen. It had begun in the morning in the situation of the right kidney, and soon extended round to the right side of the abdomen and to the groin. Two days before, he had experienced a similar attack of pain in the renal region, stretching round into the hypogastrium. When I saw him he described the pain as lying more round the umbilicus than elsewhere; and he expressed a strong persuasion, from the feelings which attended it, that it would be removed by free action of the bowels. But he felt nausea; and had vomited some medicine which he had taken. He had no fever, no retraction of the testicle or numbness of the thigh, and the pain was not increased by pressure. Neither had there been any marked irritation of the bladder. He said, indeed, when I questioned him on that point, that he *thought* he had made water *rather* more frequently than usual the day before. I mention this case to show you the occasional obscurity of the symptoms. Here a well-instructed medical man believed that nephralgia, existing in his own person, was colic. To my judgment, however, it seemed most probable that a small calculus had been passing from his kidney towards, and perhaps into, his bladder. I may as well tell you the event of the case, which interested me a good deal; for it exhibits the train of symptoms that are apt to ensue after nephritic attacks; although in this instance they were but slightly pronounced. His bowels were well acted on by a purgative, and the next day he was free from pain, and apparently well.

Two days after this, he had more frequent calls to void urine than were usual with him, and having done so on one occasion, he presently felt the want again, and then passed a little blood. The urine had been of a clear amber colour throughout. At the expiration of two or three days more he called upon me to say that after making water he had perceived in the vessel a small crystallized mass, which he took out, supposing it to be (what it very much resembled) a fragment of sugar-candy. In fact he had been eating sugar-candy, and thought some portions of it had fallen down between his waistcoat and shirt, and afterwards into the chamber-pot. He had the curiosity, he said (some misgiving he must have had too, for I had told him my own opinion of the nature of his attack), to put a small crystal from this fragment into his mouth; and as it neither tasted sweet nor dissolved, he suspected it might be a urinary concretion, and brought it to me. And sure enough it was so; a piece of very pure oxalate of lime, which he had been fortunate enough thus to get rid of. It was a quarter of an inch in length, and less than one-eighth of an inch broad, consisting of an aggregation of small crystals. It was exactly similar in appearance and colour to a piece of brown sugar-candy of the same size. It would pass, longways, into a large crow-quill.

That it was oxalate of lime was proved in this manner. A little separate crystal was heated to redness on a piece of platinum foil, by means of a spirit-lamp and blow-pipe. By these means the oxalic acid was converted into carbonic acid, which was driven off by the strong heat; and quick-lime was left. This residue, moistened, and pressed into a powder on a piece of turmeric paper, gave the characteristic brown colour.

You see, then, that a nephritic affection may be mistaken for an attack of colic. In reference to practice, it would indeed be a mistake of no great importance, since the remedies that are proper in the one case are generally proper, or not improper, in the other. If the pain be attended with fever, antiphlogistic measures are alike indicated in each of the two diseases.

The numbness of the thigh, and the drawing up of the testicle, are analogous phenomena to the pain which affects the shoulders in hepatic disorders. Irritation of one extremity of a nerve, situate internally, and belonging to an organ which is not endowed with a high degree of sensibility, causes sympathetic sensations in the sentient extremities of other branches of the same nerve, or of communicating nerves.

And this sympathetic affection of distant parts is sometimes attended (as I formerly observed) not merely with pain, but with some degree of inflammation also. The testicle occasionally swells, and becomes tender, during a nephritic attack. On the other hand, as the nerves which communicate with those of the testicle or thigh may

or may not be implicated in the renal disorder, so we see how it happens that these curious symptoms, so instructive when they do occur, may frequently be wanting; as they were in the example I just now detailed to you.

When the symptoms I specified in the outset are attended with fever, we conclude that we have to deal with nephritis; and when inflammation of the kidney, however produced, lasts for a certain period, without abatement, *suppuration* is to be dreaded. Such suppuration is marked, sometimes, by the supervention of rigors, by throbbing perhaps, and it may be by a remission of the pain: but I believe it may take place without throwing out any such signals. Nay, I think it probable that inflammation, confined to the parenchymatous substance of the kidney, may arise, and run through all its stages, without denoting its presence or progress by any noticeable local signs; and that the sharp and peculiar symptoms ascribed by authors to acute nephritis, manifest themselves only when the investing membrane of the gland, or its pelvis and excretory tubes, are involved in the inflammatory process. However this may be, suppuration leads to ulceration, to the formation of renal fistulæ, to the establishment of a purulent discharge, and hectic fever; and finally, in most cases, to a fatal event; whether the inflammation was at first idiopathic, or dependent on a calculus.

I may illustrate these remarks, by stating the heads of a case which has occurred to me since this course of lectures began. I admitted Caroline Barnard, a married woman, forty years old, into the hospital, on the 18th of October. Among other things she complained of pain in the situation of the right kidney. She had been ill six weeks, and at the commencement of her illness her urine had been very turbid, as indeed it still was: and she had experienced much pain and difficulty in passing it, and after it had passed. From that time she had frequent nausea and retching, and occasional numbness of the right thigh. She had been losing flesh fast; and her pulse was frequent. There was some tenderness discoverable in the right renal region; and after a time a manifest fulness there, and hardness; and at length œdema of the integuments and *extreme* tenderness. She suffered also well-marked hectic fever, and had severe and repeated rigors. On the 4th of November, after a careful examination of the right loin, we satisfied ourselves of the presence of matter. I got Mr. Arnott, therefore, to see her, and to put a lancet into the abscess; and a large quantity of faint smelling pus came out. She was greatly relieved by the operation; and a purulent discharge, mixed with shreds of cellular membrane, came away in abundance for some days: but in time the discharge ceased, the swelling subsided, and the opening healed. We began to hope that it had been merely an abscess in the *neighbourhood* of the kidney, irritating it and affecting its functions. But in three weeks after the abscess was punctured, the swelling was found to have recurred; and she again began to suffer much. The tumour was again opened, and pus, of a more offensive character than before, evacuated. In the early part of December she sank.

We found the right kidney small, collapsed, and hollow; in some parts a mere flabby bag. On its posterior surface there was an opening, which formed a communication between the interior of the kidney and the abscess in the areolar tissue, which had pointed externally. The pelvis of the kidney was much dilated: and the substance of the gland destroyed to a considerable extent, by suppuration and ulceration. The ureter, where it left the pelvis of the kidney, was found to be impervious.

The other kidney was much enlarged; but of quite healthy and sound structure. That kind of compensation had occurred which I formerly mentioned as not unusual when, of double organs, one has been rendered incapable of its natural functions, and the other takes up its duty, and performs a two-fold amount of work. The organ of which the function is thus increased, becomes hypertrophied. This woman did not die because there was not urine enough secreted; but she sank under the wasting purulent drain, the irritation and pain she suffered, and the protracted hectic fever. In this instance the inflammation and suppuration occurred independently of the formation of calculous matter.

Sometimes the pus finds its way out of the body through the natural passages, and appears in the urine. This woman's urine was thought, by some of the pupils, to contain pus. It was quite thick, and of a yellowish colour. But heat rendered it transparent. You must not judge by a cursory look at the water. The effect of heat

proved that the yellow material was not pus; the impervious condition of the ureter showed afterwards that it could not have been.

When calculi exist in the kidney, they often betray their presence there, by causing *bloody urine*. But bloody urine may proceed from various causes; and in conformity with my usual custom, I shall by and by offer you some general remarks on hæmaturia, as one of the hæmorrhages.

I showed you, at our last meeting, that gall-stones might inhabit the gall-bladder in considerable numbers, and be quite harmless, unless they attempted to escape from their prison, through the very narrow channel of egress from it; and I intimated that the same observation was often applicable to urinary concretions. Renal calculi do indeed, in many cases, produce abiding uneasiness, or frequently recurring pain, in the situation of the affected kidney, bloody urine, and gastric disturbance; especially when the concretions are shaken or displaced by sudden jolts or jarring movements of the body; or when the system is deranged by intemperate habits. But in many other instances these calculi cause no pain or annoyance, so long as they remain in the kidney: although they inflict horrible suffering, in general, while, for the first time, they are forcing their way along the narrow ureter. A concretion cannot be formed in a moment; yet the attack of pain often comes on in a moment, without any previous warning. After a while it remits, perhaps as suddenly; the calculus having passed (it may be presumed) from the ureter into the bladder; and then indications, more or less palpable, usually begin to declare themselves of its presence in that receptacle. Moreover, it is not uncommon to find calculi in the kidney after death, of the existence of which there had been no symptom manifested during life.

The *treatment* of nephritis—or of the nephralgia calcuosa, when accompanied by fever, or occurring in young, strong, and plethoric persons—is just such as would be proper in cases of severe colic, or enteritis: and therefore it is that any mistake between these disorders at the outset is not of so much practical consequence. The objects of treatment are, to arrest the inflammatory process; to quiet existing irritation; and to obviate any fresh causes of irritation. Venæsection, therefore, in proportion to the strength of the patient and the violence of the symptoms, will sometimes be proper: and it will always be advisable to take away blood freely from the neighbourhood of the suffering part by cupping. Warm fomentations; the warm bath; the injection of warm water into the bowel; these are all expedients of which practical men acknowledge the value. The warm enemata not only clear out from the large intestines any irritating matters they might contain, but, from the proximity of the colon to the kidney, they have perhaps the effect of an internal fomentation. It is desirable also to get the bowels well acted upon by purgative medicines as soon as possible: the relief that follows free alvine discharges is often very marked. There is sometimes a difficulty, from the irritability of the stomach, in administering purgatives by the mouth. Calomel, however, will often be retained, when other substances are rejected. It is generally considered of importance to give those purgatives only which are not likely, after being absorbed into the blood, to irritate the urinary passages. On this account the *saline* purgatives ought to be avoided. Nothing is so good as castor oil, if the stomach will bear it; or infusion of senna, with manna, may be used; or, if the stomach be very queasy, *pills*, composed of cathartic extract and calomel.

When there is no fever, *i. e.* when the case is one of nephralgia, and a calculus is passing, after the intestinal canal has been cleared by a purgative, it will be necessary to give opium in full doses to allay the pain: and it may either be administered in the form of pill through the stomach; or introduced into the rectum.

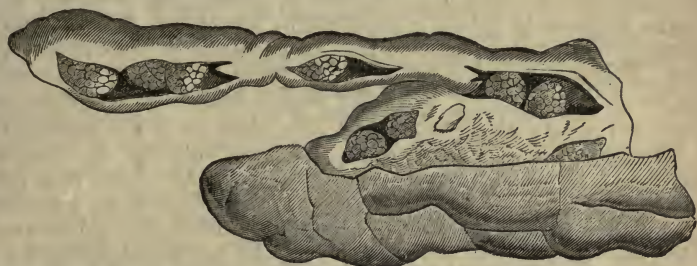
When a person suffers what is called a fit of the gravel, the pain, I say, is at length very suddenly relieved, in general, in consequence of the calculus having emerged from the ureter and entered the bladder. We judge that this has taken place, first, by the cessation of the pain; and secondly, by the supervention, sooner or later, of symptoms indicative of stone in the bladder: *viz.*, a more than usually frequent inclination to make water; pain, referred to the extremity of the urethra, especially just after passing urine; and stoppages and renewals of the stream of water while the patient is endeavouring to void it.

The time which a calculus takes in travelling from the kidney to the bladder varies a good deal. The painful journey may be over in a few hours; or it may last two or

three days. More rarely the symptoms continue, with irregular intervals of comparative quiet, for weeks. And sometimes, notwithstanding the peculiar pain, which amounts to torment, all morbid symptoms cease, and yet no calculus has passed, apparently, into the bladder: none, *i. e.* of the symptoms of stone, ensue; no calculus is voided by the urethra; and none is found in the bladder when the patient at length dies.

What is the explanation of these circumstances? Why, as calculi have been discovered in such cases in the *kidney*, it has been supposed that a concretion may get into the very beginning of the ureter, where it is a little larger than elsewhere, and give rise to the peculiar symptoms, yet never pass fairly into that narrow tube; but at length fall back again into the pelvis of the kidney: when the symptoms cease.

FIG. 144.

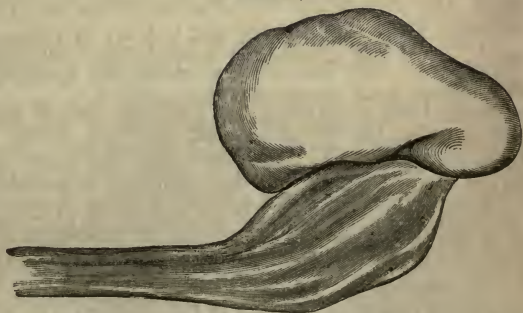


Calculi of the kidney and ureter. From a specimen in Professor Willard Parker's collection.

But the same symptoms undoubtedly occur, occasionally, when there is no calculus at all. Sir B. Brodie has referred to this form of complaint. In people who live intemperate and luxurious lives, pain is apt to seize upon one renal region, and to extend round and downwards into the groin; and these symptoms will be followed by frequent, difficult, and painful micturition, the urine being unusually acid high-coloured, and sometimes turbid. The whole irritation appears to be produced by this unhealthy urine: at least the complaint vanishes after cupping the loins, purging, warm baths, and two or three full doses of colchicum given at short intervals. It is highly probable that small colourless particles of oxalate of lime give rise to these symptoms.

Sometimes the little stone becomes immovably wedged in the canal of the ureter. When it completely shuts the tube, the urine accumulates behind it, and that portion of the ureter dilates. The obstruction usually proves fatal, by its influence upon the functions of the kidney, and thereby upon the whole economy. But if the urine find a passage by the side of the impacted concretion, this danger is averted, or postponed.

FIG. 145.



Dilatation of the ureter and pelvis of the kidney. From a specimen in Dr. Gross' cabinet.

When we have reason to believe, from the nature and course of the symptoms, that a calculus has come down from the kidney, and lodged in the bladder, then it becomes an object of deep interest to the practitioner, and of fearful importance to the patient, to try all means to bring about its expulsion before it grows too large to be voided. For grow it almost surely will, by the continual accretion of earthy matter upon its surface, if it remain long in the bladder. We know that it *may*, at first, be voided, provided the urethra be in a healthy and natural state; that whatever has passed through the ureter, may pass through the urethra also.

The objects to be kept in view are these : first, to procure a plentiful secretion of bland urine, wherewith the bladder may become filled ; secondly, by lulling the sensibility of the parts concerned, to prevent or lessen that spasmodic effort of the sphincter of the bladder, which the presence of the calculus is apt to provoke ; and, thirdly, to ascertain that the channel of the urethra is open and unimpeded.

To effect the first of these purposes, the patient should be instructed to drink freely of diluent liquors ; such as barley-water, or linseed-tea, in which may be mixed a small quantity of the sweet spirits of nitre. To fulfil the second, he should take a full dose of opium at bed-time. By these means the pain and irritation which may have been produced by the calculus, will be soothed ; and the bladder will gradually fill. He should then make water, having first placed himself in such a position that the outlet of the bladder shall be at the lowest part of that receptacle. He may stand up, and lean forwards ; or it may be well to make water while kneeling, in a warm bath. If these expedients are not presently successful, the urethra may be cautiously expanded, and habituated to the contact of a solid body, by the daily introduction of a full-sized bougie. Sometimes the calculus will follow the bougie, as it is withdrawn, through the urethra. In this way the patient will have a fair chance of getting rid of the stone. In this way a very near friend of my own, a physician now practising in this town, did expel a formidable, though not very large, piece of rough oxalate of lime several weeks after its entrance into the bladder. Out it came, at last, with a smart clink, which was music to his ear, against the chamber-pot. A gentleman was not long since sent up to me from Kent, by a former pupil of this College, with the following history. About a month before, he had been suddenly attacked with acute pain in the loins, extending forward into the left flank and pelvis ; and with nausea and vomiting. For nearly ten days these symptoms continued to occur at intervals ; then they ceased ; and then he began to be troubled by a frequent and very urgent inclination to make water, and by pain after voiding it, just above the arch of the pubes. I gave him directions, in accordance with the plan just now mentioned ; and wished him to allow some surgeon to explore the contents of his bladder. To this he would not, as yet, he said, consent. I saw him on the 2d of August. He returned into Kent the next day. On the 5th, while taking a walk, he was seized with a most imperative want to make water, but found that he could part with none. Concluding that a calculus had entered, and stopped up the urethra, he was proceeding homeward, but was soon constrained again to try to empty his bladder : and then he had the satisfaction of feeling, and seeing, a stone fly out with great force : but, as he had turned towards a hedge, he could not find it. From that moment he was quite easy.

When a calculus of a certain size has once traversed the tubes that lead respectively to and from the bladder, others sometimes follow it with more ease. I show you here a large concretion which was passed, or pissed if you will, by a patient of mine without his knowing it. He is subject to epilepsy, which is probably eccentric, and excited by renal disease. He is closely and anxiously watched by his wife. One day last year she noticed that the urine he had just voided was slightly tinged with blood : and she then found in the vessel this oblong stone, which is composed of lithic acid.

If the renal calculus, after it has reached the bladder, cannot be got rid of by the expedients I have been recommending, the question arises, whether medicine can do any further good, or whether the patient is to be delivered over to the surgeon.

Most of these small concretions admit of being mechanically crushed into smaller fragments, which are then readily washed out by the stream of urine. Larger stones are extracted entire, through incisions of the bladder. Yet there are many cases in which, for various reasons, surgery declines to attempt the removal of vesical calculi. Medicine still offers to these unfortunate patients the means of mitigating, at least, their sufferings. But it often can do more than this. It is very important for you to know that judicious medical treatment may retard or prevent, and that injudicious medical treatment may promote and hasten the enlargement of such calculi. Let us briefly consider the principles by which our judgment and our practice, in this serious matter, must be guided.

I have described a fit of the *gravel*. We say that a patient has the gravel when he passes concrete matter with his urine, whether in the form of powder, of grit or sand, or of more massive calculi. We do not apply that term to the cases in which

the urine is clear when recently voided, and warm; but throws down a powdery sediment as it cools: which sediment redissolves if the urine be again artificially heated. Now besides the different forms which the gravel assumes, of powder, sand, and little stones, there are (as you may have guessed from certain terms that I have been obliged to employ) several *kinds* of gravel; differing, I mean, in their chemical composition. The main signs — the pain, the sickness, the affection of the testicle, the subsequent bladder symptoms, — are much the same, whatever be the nature of the solid matter that descends from the kidney, and lingers in the bladder. But other circumstances differ widely. The qualities of the water previously to the formation, and to the discharge, of the sabulous matter; the state of the system at large. And it is quite impossible to treat cases of calculus in the kidney, or of stone in the bladder, with propriety, or safety, without constant reference to the condition of the urine. The morbid states of that secretion are of the greatest interest. I cannot undertake to enter upon the subject in much detail. Yet some outline of it I must attempt, especially where it touches upon points of practice.

The office of the kidneys is simply excretory. Through them, and with the urine, are drained away many of the impurities, habitual or accidental, of the circulating blood; and any excess of its aqueous ingredient. It does not fall within my province to go into the chemical composition of the urine. That is fully taught in other lectures. It is enough to say that it is complex, and perpetually shifting. Everybody knows by his own observation that, compatibly with the most perfect health, the urine may vary considerably in its sensible qualities; in quantity for example, in colour, in its specific gravity. The average diurnal quantity is from thirty to forty ounces. If much liquid be drunk, more urine is secreted. If much water pass off by the skin, or through the bowels, less passes by the kidneys; and contrariwise. Its natural colour resembles that of wheat-straw, of amber, or of pale sherry. Its ordinary specific gravity lies between 1015 and 1025: that of distilled water being represented by 1000.

You know, probably, that the urine voided by a person in health always exhibits *acid properties*, always turns litmus paper red. Not that healthy urine contains a free acid; but only that certain of the alkaline and earthy bases are not exactly neutralized, but exist in the state of supersalts. Of these, most probably the acid phosphate of soda is the one which usually gives the acid reaction to the urine. You ought also to be aware of certain variations which take place, with much regularity, in the acidity of the urine. Want of attention to this point — or rather the want of knowledge on this point — has been a fertile source of mistakes. Dr. Bence Jones has ascertained that the urine is most strongly acid just before a meal, and that it gradually becomes less and less acid while digestion is going on. The acidity is least about three hours after breakfast, and five or six hours after dinner. The explanation of these changes is this. During digestion soda is set free in the stomach and finds its way into the blood, and so into the urine. The acidity of the gastric fluids, and the acidity of the urine, are in inverse proportion.

Modern chemistry teaches (I repeat) that the acid reaction of healthy urine is commonly due to the acid phosphate of soda. Dr. Prout ascribed it to a super-lithate of ammonia; but it seems doubtful whether a super-lithate of ammonia ever exists. Neutral lithate of ammonia, however, is very readily soluble in the saline urine. But whether out of the body, or within it, the lithate of ammonia will, of course, be decomposed if any free acid be present in the urine, for which ammonia has a stronger affinity than it has for the lithic acid; and the latter, being insoluble, will be slowly thrown down, in the form of a red or yellow sand: little crystals, in point of fact, they are; to the naked eye very often like, in shape, size, and colour, to particles of Cayenne pepper. Under the microscope the lithic acid is seen of various forms, according to the nature of the urine from which it is deposited. Of these forms the most marked are here exhibited. I show you also some of this red sand, collected by one of my out-patients at the hospital. He must have passed a peck of it while under my observation: and I am sorry (having lately lost sight of him) that I did not procure a large quantity for the museum.

Now this lithic (or uric) acid, or red sand, or gravel, is liable to form in the kidney, if not in the bladder, and to congregate into calculi; and a calculus once formed, or, indeed, any solid substance, will constitute a nucleus, upon and around which a

FIG. 146.



Urea, prepared from urine, and crystallized from aqueous solution by slow evaporation.

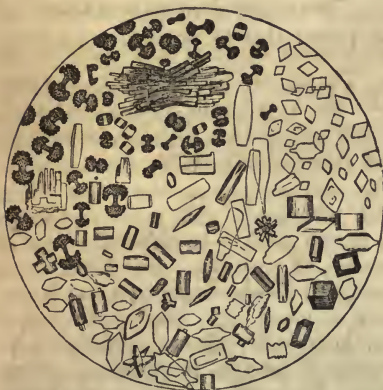
FIG. 147.



Fan-shaped aggregates of tubular crystals of uric acid, rather less often met with in urinary deposits.

further and repeated incrustation of a similar nature is almost sure to take place. You will at once perceive the importance of doing nothing to aggravate this disposition to deposit lithic acid; but of trying to prevent or stop it. If there be symptoms of stone in the kidney, or in the bladder, and we have reason to believe that it con-

FIG. 148.



Uric acid.

FIG. 149.



Uric acid.

sists of lithic acid, there are medicines which would tend to make matters worse, and there are others of which the effect would be to correct the lithic acid *diathesis*, as it is called. But how are we to know whether the presumed calculus be of that kind or not? or, rather, how are we to know that the lithic diathesis exists? Why, we learn that it exists by noticing the habitual qualities of the urine, and the habitual state of the patient's general health.

The urine of persons who have the lithic diathesis is bright, of a dark golden or coppery colour, like brown sherry. Sometimes it feels slightly pungent in the urethra as it is passing. It is more acid than the urine of health, and gives to blue litmus paper a deeper shade of red. Commonly it contains more than the usual amount of urea, and has a high specific gravity. It is apt, too, to fall below the average quantity.

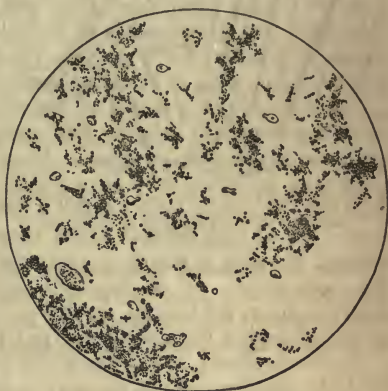
The lithic acid is not often thrown down before the urine is voided. When it is, it appears in separate crystals, in the shape of fine sand; or in coarser roundish grains, which are in fact minute concretions of crystals.

You must not confound this crystallized lithic acid gravel with those amorphous powdery deposits which are much more common and more copious, and which consist

of lithic acid in combination with ammonia, lime, magnesia, or soda. They are generally spoken of as lithate (or urate) of *ammonia*, but are chiefly composed, I believe, of lithate of soda. The colour of these lithates is sometimes pale, and almost white; more often of a yellowish brown; or red, like brick-dust; or occasionally of a deep purple, or almost crimson tint. They are never deposited till the urine has cooled. People are liable to be frightened by their appearance; apprehending that they may harden into a stone in the bladder. But you may always relieve their anxiety by stating that these sediments are never substantially present in urine at the temperature of the body. You may show that they presently dissolve and vanish, as the urine is again warmed. The lithic sand does not so disappear. The lithates are apt to stain the surface of the vessel, and they render the whole of the urine turbid when it is shaken: whereas the lithic sand rolls over at the bottom when the vessel is slowly tilted, and does not trouble the general transparency of the stirred water.

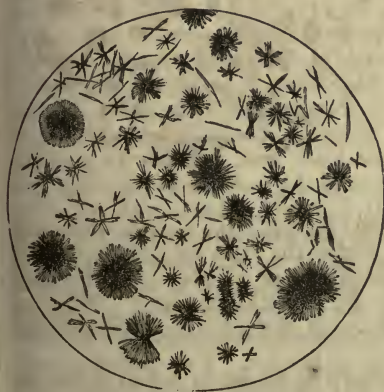
These, and all other urinary deposits, are in most instances discriminated more easily, more quickly, and more surely, by means of the microscope, than in any other way; and you may now carry in your waistcoat pocket a microscope which is practically sufficient for this purpose, and perfectly simple in its use. A single glance at a drop of urine containing the sediment will reveal its character with more accuracy than could be attained by a long and laborious chemical analysis. The lithate of soda appears in molecular granules, which are often arranged in little tufts, and look like fragments of moss. True lithate of ammonia sometimes presents itself in a rounded form, with one or more little projecting spikes—resembling a minute thorn apple.

FIG. 150.



Urate of Soda deposited from Urine.

FIG. 151.



Bi-urate of Ammonia.

FIG. 152.



Nitrate of urea, separated from very concentrated human urine by nitric acid.

Now the lithates of which I have been speaking do sometimes — do not seldom indeed — show themselves in urine which deposits the lithic acid crystals also; and you must then warm the urine, and disperse the lithates, before you can obtain a clear view of those crystals. But in such cases the nature of the disorder, and its proper treatment, are both determined by the presence of the lithic acid; and the concomitant lithates are of secondary importance. I shall have something further to say of them, however, presently.

The presence of the so-called lithic diathesis is likewise accompanied, and so far

denoted, by a tendency to feverish and inflammatory complaints. The patients are troubled with transient twinging pains in their limbs, and many of them are subject to gout or rheumatism. They are mostly also indolent and luxurious, or intemperate in their mode of life. Adults are peculiarly obnoxious to this condition of the system after the age of forty. But children, up to the period of puberty, are very liable to have lithic acid gravel: and in this early period of life such deposits indicate, according to Dr. Owen Rees, a tendency to grave disease.

Whenever a paroxysm of nephritic pain befalls a person whose time of life, whose habits, the character of whose health, and the habitual qualities of whose urine, are such as I have been describing, you may conclude that the concretion which has occasioned the symptoms is of the lithic acid kind: and you may expect that such attacks will recur; for it is observed of these lithic acid renal calculi, that they are generally numerous in the same individual. I speak of the *habitual*—or of the *frequently recurring*—qualities of the urine: for a deposit of lithic acid gravel, as well as of superabundant lithates, may occur to the healthiest individual, under accidental and transient disturbing causes. Many persons will tell you that their water presents a red sand whenever they have a cold. Febrile and inflammatory ailments may produce the sediment: even too full a meal: or exercise taken immediately after a full meal. In all such cases it seems probable that the customary evolution of free acid through the skin is somehow prevented: in consequence either of a check given to the perspiration, or of imperfect assimilation of the food. The free acid thus diverted from its natural emunctory—or some acid introduced from without, or generated within the system—is determined to the urine, and precipitates the lithic.

Dr. Bence Jones, in a paper which you may read in the *Philosophical Transactions* for 1845, has pointed out one way in which an excess of free acid sometimes comes to the urine. From disorder of the stomach an excessive quantity of free hydrochloric acid is there secreted, and remains there during the whole process of digestion. If urine be at that time passed, it may be found alkaline from fixed alkali; and so it continues until the contents of the stomach are absorbed, when the free acid which was in the stomach passes off in the urine, making it intensely acid, and precipitating lithic acid, or the lithates, according to the quantity of free acid, and the length of time during which the urine (after being secreted by the kidneys) is submitted to its action.

Now the formation of lithic acid in the urine attests its over-acid condition, and both the one and the other may be controlled by the exhibition of alkaline remedies. You will find that free livers use alkalis to neutralize the excess of acid which results from their intemperate habits; the carbonate of soda, or of potass. They do this, without any reference to the appearance of their urine, to prevent or appease the uneasy feelings produced by a debauch. But it is of importance to be aware that one of these alkalis is preferable, for the purpose of obviating the lithic acid deposits, to the other. Soda will sometimes combine with the lithic acid, and form an insoluble salt, as hard, and as pernicious, when deposited around a nucleus, as the lithic acid itself. With potass there is no such danger. If it should combine with the lithic acid, the resulting salt is perfectly soluble, and will pass away dissolved, in the urine. Magnesia is also a good medicine in such cases; but it has this disadvantage, as I showed you indeed before, that it is apt, when taken habitually, to cause *intestinal* concretions; and these may be as dangerous as the urinary ones. One of the best modes of giving the bicarbonate of potass is in the common saline draught. The salts of vegetable acids are converted, *in transitu*, into carbonates. The change appears to take place, not in the stomach, but in the blood; and to be caused by the action of oxygen. The remedial properties of the bicarbonate, thus administered, are the same with those of the pure alkali, while it is much less likely to derange or disagree with the stomach. The phosphate of soda is a powerful solvent of lithic acid; and this salt has been suggested by Dr. Golding Bird as a suitable drug in these cases. It tastes, in a dilute solution, somewhat like common salt; and it may conveniently be taken in scruple or half-drachm doses, dissolved in broth, or gruel. The Vichy water furnishes another efficacious alkaline remedy, which may be used as a beverage. Of course the mode of living ought to be changed when the lithic diathesis prevails; the patients should dine moderately and plainly, eating of one dish, and avoiding acids and all articles of diet likely to generate acid in the stomach; saccharine sub-

stances therefore, starch in all its forms, and fermented liquors. But, as I remarked in a former lecture, they will not, if they can help it, give up their accustomed indulgences: and they attempt, and we attempt, but the attempt is often made in vain, to remedy disorders, which might with ease and certainty have been prevented.

You must take care not to give these alkaline remedies too long; nor in too great quantity. You must not push them to such an extent as entirely to destroy the acidity of the urine: for if you do, your patient is exposed to the same danger as before, but from an opposite cause. A *white sand* or gravel will be apt to form in the alkaline or neutral urine: and this will collect itself, by the force of aggregation, around any existing calculus, or foreign substance. The white deposits consist mainly of the triple phosphate of ammonia and magnesia, mixed with amorphous phosphate of lime; and if you examine collections of urinary calculi, you will find that they are sometimes made up of concentric layers; and one layer may be composed of lithic acid, and the next of the mixed phosphates; and so on, as the condition of the urine has alternated. You must test the urine therefore, and see that it still reddens litmus, though perhaps faintly. Indeed it may do so, without containing acid enough to dissolve all the earthy phosphates, if they are present in excess; so that urine which only slightly reddens litmus paper may nevertheless be capable of sometimes depositing the earthy phosphates. The saline draught has always a tendency to make the urine alkaline; and thus it is, probably, that it proves of use in febrile disorders; but it may become a poison to those whose urine is already alkaline. Colicium has a similar tendency to diminish the acid reaction of the urine. So has mercury. And I may tell you — speaking generally of morbid states of the urine — that it is much more easy to correct too great acidity than to rectify the opposite condition. We can almost always make acid urine neutral or alkaline: but to render alkaline urine acid is often beyond our power.

In truth, the administration of alkaline drugs, which, by clearing the urine of superfluous acid, staves off a present danger, does nothing towards redressing that state of the system from which the excess of acid, and the danger, proceed. Alkalies operate upon an effect, but leave its cause untouched. A long continuance of them may even tend, apart from their immediate effects upon the urine, to lower the general tone of the body, to render the muscles flabby and weak, and the complexion pale. To cure the morbid disposition the patient's regimen must be prescribed. His diet must be regulated, as I have explained already: and it is scarcely less important to attend to the functions of the *skin*, in persons having the lithic acid diathesis, than to the functions of the stomach. The warm bath is often an excellent adjuvant in their treatment: or, where it can be borne, the daily use of the cold or tepid sponging bath, with subsequent friction by the flesh brush, or hair-glove. In cold weather warm clothing must be enjoined; and the avoidance, in all weathers, of such exposure to cold as might suppress or materially lessen the amount of healthy perspiration.

Active exercise in the open air, furthering the removal of acids through the skin, and of carbonic acid through the lungs, is also of great importance: and it may be requisite to promote the healthy action of the liver and bowels by mild aperients containing a small proportion of mercury.

A word or two more about the deposit of *lithates* in the urine. I have already remarked upon the variety in their colour. When this does not result from the presence in the water of some accidental colouring matter, useful inferences may occasionally be drawn from the peculiar tint of the sediment. Those deposits which have a tawny or reddish yellow, or what Dr. Owen Rees describes as a nut-brown hue, are the most innocent. They are frequently the effect of mere indigestion, of a common cold, or of some other slight and transient disturbing influence. The white lithates are of more equivocal import. They seem to precede sometimes, or to accompany, the excretion of a small amount of sugar through the kidneys. They should suggest vigilant care and inquiry. Those which present a pink or brick-dust colour are mostly associated with febrile states of the body, and are common in acute rheumatism. When such sediments are habitual, and without fever, they are often connected with organic visceral mischief. The purplish or crimson deposits were believed by Dr. Golding Bird to be "almost pathognomonic of disease in the organs in which portal blood circulates."

The quantity of lithates contained in healthy urine varies continually and considerably: it is generally the greatest a few hours after a meal. The precipitation of these natural constituents of the urine depends upon several distinct causes.

Do not fall into the common and not unnatural mistake of supposing that urine which throws down the lithates, is therefore and necessarily over acid urine. It may be so: but very often it is not so. The less acid the urine is, the more of the lithates is it capable of holding in solution, and so concealing. If the urine be very full of them, and at the same time be feebly acid or neutral, or alkaline, there may be no precipitate. Again, the urine may be very acid, yet if it contain but a scanty amount of lithates, none of those lithates may be rendered visible. The most favourable condition for their sinking down is when there is present a slight excess of their average quantity, and also a slight excess of acid in the urine.

Two other circumstances must always be taken into account; viz., the absolute quantity of the urine itself, and its temperature.

The amount of lithates being the same, they will be less readily retained in solution as the quantity of their aqueous menstruum diminishes: and the colder that menstruum becomes, the less of the lithates will it be able to hold dissolved. Hence we see how a slight cold, which implies generally some check to the perspiration, and a scantier secretion of urine, is apt to be accompanied with a deposit of the lithates. The appearance will be augmented, if the temperature of the atmosphere be low. It occurs in frosty weather often, when there is no appreciable derangement of the health whatever. It is clear that these are not cases for active alkaline remedies. All that they require is warmth to the surface, diaphoretics perhaps, to preserve the balance between the skin and the kidneys, or gentle diuretics, a few grains of nitre for example, to increase the quantity of urine excreted.

Bear in mind, then, that the two conditions, of increased acidity of the urine, and of an excess of the lithates or of lithic acid, may concur; but they require to be distinguished. There is no necessity for their coincidence. They are constantly met with separate and distinct the one from the other. In gout, in indigestion, and in some other disorders, there appears to be an absolute increase in the amount of uric acid or of urate of soda; and to this state of the system the term *lithic diathesis* ought, in strict propriety, to be confined.

You will have gathered, from what I have already said, that there is a morbid condition of the body, the opposite of that which is characterized by a prevalent deposit of the lithic acid: a state in which a readiness is manifested to throw down *white gravel*, and to which, by high authority, the title of the *phosphatic diathesis* has been annexed. But this phrase also will be apt to mislead you, unless you are made aware of what it means, and of what it does not mean. It does not imply, then, any excess of the phosphates in the urine: while it does signify their frequent appearance in that secretion under a substantial and visible form; in one word, their deposit. And as the deposit of the lithates depends often upon a superabundance of acid in the urine, so that of the phosphates is determined by the opposite condition, by a deficiency of acid, by alkalineity of the urine. And even with respect to this alkalineity, some further distinction is needed. There are two kinds of alkalineity:—alkalineity from the presence of a fixed alkali,—the carbonate of potash, or the carbonate of soda, or the alkaline phosphate of soda; and alkalineity from the presence of the volatile alkali,—the carbonate of ammonia. Urine alkaline from this last cause,—well known as ammoniacal urine,—especially indicates the phosphatic diathesis of Dr. Prout.

Earthy phosphates are very insoluble in alkaline fluids, and very soluble in dilute acids. The minutest trace of these phosphates will be made visible if the urine become any how alkaline; and a very great excess of them will be hidden from the eye in urine that is healthily acid, by their ready solution therein.

Here then, as before, the old rule, “*de non apparentibus et de non existentibus*,” would be fallacious. The non-appearance of the phosphatic deposits in the urine has been mistaken for their absence; and their appearance has been wrongly assumed to denote their presence in excess. All this has been clearly laid down by Dr. Bence Jones.

The white gravel which is deposited in that condition of the system to which Dr. Prout has applied the term phosphatic diathesis, but which is better characterized by the presence of ammoniacal urine, is composed of minute shining prismatic crystals

FIG. 153.

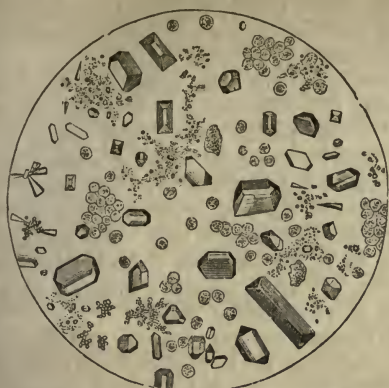


FIG. 154.



Fig. 153.—Urinary deposit of magnesia and ammonia, with mucous corpuscles, from catarrh of the bladder.

Fig. 154.—Urinary deposit of triple phosphate (phosphate of magnesia and ammonia), and urate of ammonia in alkaline fermentation, in paralysis of lower extremities.

of a triple salt, the phosphate of ammonia and magnesia. The way in which this is formed, according to Dr. Prout, is as follows. Healthy urine contains the phosphate of magnesia, which is very soluble, and therefore is dissolved, in that fluid. But, under certain circumstances, the urea of the urine becomes decomposed in the kid-

FIG. 155.



FIG. 156.



Figs. 155, 156, Varieties of crystalline forms. The triple or neutral phosphate of magnesia and ammonia.

neys, or in the bladder, and ammonia is extricated, which combines with the phosphate of magnesia, and forms a triple salt, insoluble in the slightly alkaline urine. Almost always with the triple phosphate just mentioned, there is also an admixture of phosphate of lime in the shape of an exceedingly fine amorphous precipitate.

The tendency to deposit the mixed phosphates, with a predominance of the triple phosphate, is accompanied frequently with local disease in some part of the urinary apparatus, especially in the bladder and prostate gland. It is no unusual consequence of injuries of the back, — or rather of some morbid state of the spinal cord. The immediate link in the chain of connexion between the cord and the urine, in these cases, seems commonly to be a chronic inflammatory condition of the mucous membrane of the bladder; the decomposition of urea being effected by the altered mucus. The urine is sometimes pale, sometimes of an orange or copper colour. It always contains crystals of phosphate of ammonia and magnesia, and in many or most cases pus globules also are made visible by the microscope. Upon its surface a sort of film is often formed, exhibiting, as you look at it in different lights, the various colours of the rainbow; an iridescent pellicle. This has been found to consist of the triple phosphate, and sometimes of the phosphate of lime. If you skim the pellicle off, by placing a bit of paper under it, and then suffer the paper to dry, you may distinctly see the little crystals. Urine of this kind speedily grows putrid and highly offensive, and has a strong ammoniacal smell. It turns red litmus paper blue, but as the paper dries the red colour reappears. The alkalescence is from the carbonate of ammonia.

This, then, is the most usual form of the phosphatic deposit: the urine becoming

alkaline after it is secreted, and precipitating the earthy phosphates. But I have stated that the urine may be alkalescent from a fixed alkali; and then, no ammonia being present, the triple phosphate of ammonia and magnesia is not thrown down, but the phosphate of lime alone falls as a fine white powder, or forms a scum of the most iridescent appearance on the surface. In these cases the urine is secreted alkaline, and frequently an over-acid state alternates with this alkalescence. The urine itself is pale, copious, slightly turbid or opaline, of a low specific gravity, and it does not smell like healthy urine: occasionally it has somewhat the faint odour of weak broth. The white sand is deposited as the water cools, and sometimes even while it is yet warm, and in the bladder; so that the last portion of the issuing stream looks milky. By such urine, reddened litmus paper is made permanently blue. No prismatic crystals of the triple phosphate can be seen in it, nor any pus globules detected; although mucus, and oxalate of lime octohedra, are often present; mixed with an amorphous, or more rarely a crystalline deposit of phosphate of lime.

There are conditions of health in which the absolute amount of earthy and alkaline phosphates together is increased, and to these conditions the term phosphatic diathesis is appropriate. The amount of earthy phosphate alone depends so much upon the quantity of lime or magnesia present in the urine, that the excess of that salt only might rather be considered to denote an *earthy* diathesis: while the epithet "phosphatic" might be limited to that in which there is an increase of the phosphoric acid (combined always with alkaline or earthy matter) in the urine. The total amount of the phosphates in the urine never displays itself to the eye, for the phosphates of soda, which are the most abundant of them, are held in solution whether the urine be acid or alkaline.

Bear in mind that an excess of the lithates, and an excess of the phosphates, are perhaps both of them rare; while variations in the acidity and alkalescence of the urine are certainly exceedingly common. Even during perfect health the urine not unfrequently becomes alkaline during the process of digestion; the alkalescence being then always due to fixed alkali. Pink litmus paper dipped into it becomes blue, and remains so upon drying.

That the prevalence of ammoniacal urine, — associated as it commonly is with disease of the mucous membrane of the urinary passages, which disease is often itself the result of some profounder fault in the nervous system, — that such urine, when constant or frequent, denotes a very unsafe and unpromising condition of the body, you will readily believe.

Moreover it is a fact of great practical importance that — apart from any such local damage — the tendency to alkalescence of the urine from fixed alkali, and therefore to phosphatic deposits, goes along with general debility, and signifies that the health has fallen below its natural standard. Persons who are jaded and spent through over-much toil, whose vital energies have been depressed by mental anxiety, by insufficient nourishment, or by sensual excesses, are very apt to pass water that is alkalescent, or but faintly acid, and to exhibit in their urine the tokens of the so-called phosphatic diathesis. They are, for the most part, cachectic, sallow, languid, spiritless, exhausted.

Anything which tends further to depress the powers of the system will aggravate this alkalescent disposition. When you find that your patient passes urine such as I have been last describing, which does not redden litmus paper, but on the contrary turns litmus paper that has been reddened by a weak acid, blue again, or even in some cases is alkaline enough to make turmeric paper brown — in such cases you must cautiously abstain from all drugs or measures that are calculated to lower the vital powers; from saline draughts, and alkalies of every kind; from mercury and colchicum; from bleeding; and even from active purgation; or you will add to the patient's dangerous weakness; and promote the more abundant deposit of the phosphates.¹

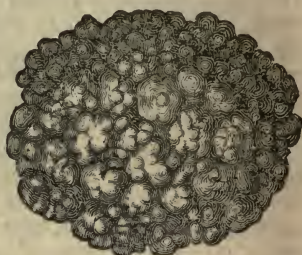
¹ Dr. Owen Rees believes that in cases of alkaline urine, associated with a morbid condition of the mucous membrane of the bladder, the urine, as secreted by the kidney, is over-acid, and tends to keep up, or to aggravate that morbid condition; which, in its turn, renders the urine contained in the bladder alkaline. He counsels, therefore, an alkaline treatment in such cases, and indeed in all inflamed states of the urinary mucous surfaces. In this way the urine, as first secreted, may be made alkaline, and then the inflamed mucous surfaces, no longer irritated by an acid fluid, recover themselves and cease to pour out their alkaline liquor; till at length the healthy and acid secretion from the kidney is voided through the urethra.

The soundness of the theory can be tested only by the success of the practice. I can say nothing on the subject from my own experience.

But you may do more than abstain from what is hurtful: you may counteract the alkaliescent tendency by a generous diet and by the exhibition of tonic medicines; bark, wine, and acids: the muriatic acid, or the nitric, or both together, may be given in such cases before meals with vast advantage sometimes. Opium is also a remedy to be employed in this form of disease. No single drug probably has so much power in rendering alkaline urine acid, as opium. And it is indicated for other reasons; it composes the nervous anxiety to which these patients are mostly a prey. Mental relaxation—freedom from care—the relinquishment of all exhausting habits and pursuits—these too are points of vast importance, whenever they are attainable.

There is yet another diathesis sufficiently common and important to claim your best attention. I mean the *oxalic*: in which there is a tendency to the formation, in the kidney, of the oxalate of lime, or *mulberry calculus*; an epithet derived from the occasional resemblance of the concretion to that fruit, in respect of colour and inequality of surface. This diathesis is not so obvious as the other two, but it is no less real. By the use of the microscope we find that it is nearly as frequent as that in which the lithic acid, or the lithates, are precipitated, and far more common than that which is marked by deposits of the phosphates.

FIG. 157.



Oxalate of lime or mulberry calculus.

FIG. 158.



FIG. 159.



FIG. 160.

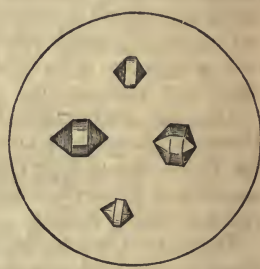


FIG. 161.



FIG. 162.

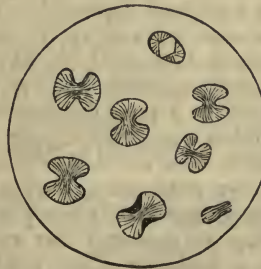


FIG. 163.



FIG. 158, 159, 160.—Oxalate of lime.

FIG. 161.—The octahedral crystals of oxalate of lime, as seen when allowed to dry.

FIG. 162, 163.—Dumb-bell crystals of oxalate of lime from urine.

The urine differs much in its sensible qualities from that of both the preceding varieties. Unlike the urine of the phosphatic character, it is often bright and clear; unlike that of the lithic, it is remarkably free from sediment. The mulberry calculus is solitary also; or recurs at long intervals; and is chiefly met with during the prime of life. In both these particulars the contrast with the habitudes of lithic acid concretions is striking.

The persons who manifest this disposition are usually dyspeptic; sometimes very much so; sometimes very slightly. They are uneasy during the assimilation of their meals; suffer flatulence when the stomach is empty; prefer vegetable diet to animal; are fond of sweets, especially of sugar. They are liable to boils and carbuncles, and to scaly cutaneous eruptions. According to their original temperament, they are nervous and irritable, or dejected and desponding in mind. A nephritic attack relieves them from all this discomfort for years perhaps. When the oxalic diathesis is strongly marked, the skin, Dr. Prout says, "is apt to assume an unnatural appearance difficult to describe, but the colour of which may be said to vary from dull greenish yellow in the sanguine, to dark olive or livid in the melancholic temperament." Young and old appear to be equally liable to this diathesis. In scrofulous children it is constantly to be observed. In fact, most of the residents in large towns who suffer even the slightest dyspeptic symptoms will be found to pass more or less oxalate of lime in their urine.

The formation of the oxalate of lime within the body depends, according to Dr. Prout, either upon the non-assimilation of oxalic acid taken with the food, or upon the mal-assimilation of saccharine aliments. Hence, as a general rule, both curative and prophylactic, *sugar* and other saccharine substances should be rigidly excluded from the diet of these patients. They should avoid, also, all kinds of fermented liquor. The young stalks of the *rhubarb*-plant, which of late years have come into such general use in this country for tarts in the spring; and *sorrel*, of which our neighbours, the French, consume a good deal in salads, and in other ways; both contain oxalic acid: and *hard water* contains lime. Dyspeptic persons who drink such water, and eat such articles of food, and are thus daily introducing, without suspecting it, the constituent ingredients of the mulberry calculus, are very likely indeed to incur the pain, and the exceeding peril, of a renal concretion of that kind. You must see, therefore, the great importance of detecting the oxalic diathesis; and of forbidding, to those who have it, all such viands as contain the oxalic acid, and of recommending them to use pure water, even distilled water, for drinking. Animal food, and the stronger farinaceous matters, and weak brandy-and-water rather than beer or wine, are best for them.

Professor Liebig first pointed out the close relation of uric acid to urea and oxalate of lime. He showed how these last two may be formed from uric acid in the laboratory: and some later experiments on animals by Woehler, have proved that they actually are so formed, within the body.

Agreeing with Dr. Prout, that the mulberry *calculus* is not of very frequent occurrence—Dr. Golding Bird first made us acquainted with the fact that small octahedral crystals of the oxalate of lime are extremely common: although from their transparency, and from their having nearly the same refractive power, and nearly the same specific gravity with the urine in which they exist, they do not frequently disclose themselves to the naked eye, nor sink down in manifest deposit. They are made plainly visible by the microscope: though sometimes its highest powers may be required, and it may be necessary to leave the urine for twelve hours after it has been voided, in order to allow the oxalate to crystallize out. From the quantity of epithelium which usually accompanies the crystals, a degree of cloudiness of the urine is frequently perceptible.

The same writer states also that the persons whose urine is thus charged with crystals of oxalate of lime are, for the most part, exceedingly sensitive and irritable, hypochondriacally apprehensive of impending evil, full of gloomy fears concerning their bodily and mental powers, dyspeptic, weak, and usually emaciated. But this description applies to extreme cases only. In both adults and children, slight cases present no symptom whatever; and it is only by the microscope that they can be recognised.

With respect to direct remedies for this diathesis, Dr. Prout tells us that he has seen more benefit derived from the mineral acids, alone or combined with tonics, than from any other. And certainly I have myself seen, in numberless instances, and at first not without marvelling, a vast improvement in the condition and feelings of such patients follow speedily upon the administering of the nitro-muriatic acid, in moderate doses, about an hour before their several meals. Preparations of iron also

are advisable if the patient be anæmic: and besides the prescription of these drugs, the avoidance of any excessive addition to study, or to business and its cares, and (as far as may be) of mental anxiety and worry, should be at the same time enjoined. The effects of the acids require to be watched: and when they begin to produce a deposit of the lithates, or of lithic acid, their use must be suspended. Dr. Prout was in the habit of recommending for patients who happened to live at a distance, the muriatic, or nitro-muriatic acid, till the lithates, or the lithic acid, began to appear in the urine; or for a *month*. "By adopting," he says, "such a course of acids three or four times in the year, and by a carefully-regulated diet, I have seen the diathesis gradually subdued, and at length removed altogether." Dr. Bird also testifies to the efficacy of similar measures.

There is another diathesis still, named after the *cystic oxide*, a substance which is chiefly remarkable for the quantity of sulphur which it contains. This is but rarely met with; yet it is worth mentioning.

FIG. 164.



Cystine from urinary calculus, and recrystallized from ammonia.

FIG. 165.



Cystine crystallized from ammoniacal solution by spontaneous evaporation.

FIG. 166.



Cystine crystals in groups.

These observations will serve, I hope, in some degree, as landmarks, to guide your treatment of patients labouring under renal or vesical calculi, or presenting symptoms such as warrant the apprehension that disorders so fearful may occur. It is impossible for me to do full justice to this important subject in these lectures; and I must refer you, for more minute information respecting it, to Dr. Prout's invaluable volume; to Sir Benjamin Brodie's most instructive book on the *Diseases of the Urinary Organs*; to the excellent work of Dr. Golding Bird, on *Urinary Deposits*; to the published *Lectures* of Dr. Owen Rees; and to various papers in the *Lancet*, and in the *Philosophical Transactions*, — full of original research and interest, — by Dr. Bence Jones.

LECTURE LXXVII.

Suppression of Urine. Diabetes; Qualities of the Urine; Symptoms; Anatomical Appearances; General Pathology of the Disease. Treatment. Diuresis.

SYSTEMATIC writers have adopted the term *Ischuria*, to express that condition in which no urine is voided. It includes, therefore, those cases in which no urine is secreted; and those in which, although secreted, it is not discharged from the body. Now these two conditions are exceedingly different from each other in most respects; and I shall prefer making use of the two plain English names, *suppression* of urine, and *retention* of urine. Even these terms are sometimes confounded with each other. In *suppression*, the secretion is suspended: in *retention* it may be as active as ever. Retention of urine is a surgical complaint; involving points of great practical interest. Suppression belongs the physician; and the technical term for it is *Ischuria renalis*. It is sometimes spoken of as paralysis of the kidney; a phrase to which I object, because I think palsy is a word which ought to be restricted to a loss of power over the muscular fibre.

The affection usually occurs in persons who are advanced in life, and inclined to corpulency. Why it should be so I cannot tell you, but such is the fact, as stated by most observers. Sir Henry Hallford has related one of five instances of this disease that he had met with in the course of seven-and-twenty years. He says it was an exact copy of all the others that had fallen under his notice: and as his account of the general course of the symptoms coincides with the statements of other writers, I may give you his narrative, in lieu of a formal description.

"A very corpulent, robust farmer, of about 55 years of age, was seized with a rigor, which induced him to send for his apothecary. He had not made water, it appeared, for 24 hours. But there was no pain, no sense of weight in the loins, no distension in any part of the abdomen:—and therefore no alarm was taken till the following morning, when it was thought proper to ascertain whether there was any water in the bladder, by the introduction of the catheter: and none was found. I was then called (says Sir Henry), and another inquiry was made, some few hours afterwards, by one of the most experienced surgeons in London, whether the bladder contained any urine or not: when it appeared clearly that there was none. The patient sat up in bed, and conversed as usual, complaining of some nausea; but of nothing material in his own view: and I remember that his friends expressed their surprise that so much importance should be attached to so little apparent illness. The patient's pulse was somewhat slower than usual; and sometimes he was heavy and oppressed."

"I ventured to state (continues the author) that if we should not succeed in making the kidneys act, the patient would soon become comatose, and would probably die the following night: for this was the course of the malady in every other instance that I had seen. It happened so: he died in thirty hours after this, in a state of stupefaction."

This is the curious and important point in the history of such cases. If no urine be separated from the blood, coma soon supervenes, and death. It is believed that these consequences result from the detention of *urea* in the system. Urea is a mere excrement, which, in health, is removed from the blood by the kidneys, as fast as it enters that fluid. When it is not so carried off, it accumulates in the blood, circulates with it to every part of the body, and acts as a poison, especially upon the brain. To render it thus poisonous, however, its decomposition in the blood appears to be requisite, as I shall explain more fully in the next lecture. This is one of numerous instances, showing that the carrying fluid of the body may become the vehicle of disease and death, if it be not duly purged of deleterious matters which pertain to the unceasing processes of organic life. If *carbonic acid* be not extricated by the lungs, the animal functions are as certainly and almost as speedily extinguished by that gas, as the flame of a taper might be. And we have recently seen that when the outlet

from the liver is shut up, when the blood is not purified from the excrementitious *bile*, the powers of animal life are weakened, and sometimes utterly and rapidly destroyed.

Suppression of urine, for a considerable time, is not, however, necessarily and universally fatal. Patients labouring under the epidemic cholera would secrete not a drop of water for some days; and yet recover. It was remarkable how entirely free such patients were from any approach towards coma. Was the urea here drained off from the blood in the enormous and unnatural flux from the stomach and bowels? I think it probably was: but chemical search has not detected that substance in the fluids so effused. Schmidt thinks indeed that it would be found, but for its rapid decomposition into carbonate of ammonia. There are, however, some very singular instances on record, of persons who have passed days and even weeks without secreting urine; and without showing any other indication of impaired health. What degree of credit such narratives deserve I do not know; but assuming that there was neither fraud nor mistake, it may be suspected that either the natural secretion was compensated by some vicarious or supplemental discharge; or that a *small* quantity of urine was actually separated by the kidneys. "If any water, however small the quantity (remarks Sir Henry Hallford), had been made in these cases, I should have thought it possible that the patient might have recovered: for it has often surprised me to observe how small has been the measure of that excrementitious fluid which the frame has sometimes thrown off, and yet preserved itself harmless. But the cessation of the excretion *altogether* is universally a fatal symptom in *my experience*, being followed by oppression on the brain." The same eminent physician states that in three of his five cases there was observed a remarkably strong urinous smell, in the perspiration, for twenty-four hours before death. This I believe is of common occurrence in such cases. Other patients have vomited, or passed by the bowels, watery matters possessing some of the sensible qualities of urine: and a urinous fluid is said to have been found in the ventricles of the brain in some of the fatal cases.

I have spoken of suppression of urine as a malady, though it probably is never anything more than a symptom. Yet it is one of those symptoms which from our uncertainty respecting their origin and determining cause, we are obliged to treat, and to study, as if they were substantive diseases. In the only well marked instance that I have seen of suppression of urine coming on in an apparently healthy person, some blood had appeared in the urine for a day or two before the secretion was totally suspended; and the kidneys were found gorged with blood. Extreme congestion, or inflammation, of the substance of the gland, is probably at the bottom of many of these cases. The same train of symptoms supervenes not unfrequently upon organic renal disease. They happen too, sometimes, when the ureters become impervious, from disease, or from impacted gravel. In this condition urine continues to be secreted, for a time at least, and distends the ureter behind the seat of the obstruction. The apoplectic state which ensues may arise from a re-absorption of the secreted fluid; or, in consequence of the obstacle, the secretion itself, after going to a certain point, may stop, and then the case becomes a case of suppression.

Respecting the treatment of this most formidable condition I can say but little. Cupping upon the loins, venæsection if warranted by the state of the pulse, the hot bath, sudorific medicines, purgatives, and large warm enemata, seem to me the kind of remedies indicated. To endeavour to force the secretion of urine by strong stimulating diuretics, would strike one, *à priori*, as being hazardous. Yet this practice has its advocates; and, should experience declare in its favour, theoretic objections ought to be disregarded. If benefit is so to be obtained, probably the best drug for our purpose would be cantharides. Dr. Elliotson refers to some examples of its success in the hands of Sir Astley Cooper; and afterwards of another practitioner who took the hint from Sir Astley. He suggests that as the tincture of cantharides is a very uncertain preparation, the remedy should be given in the solid form, a grain at a time, and that a large blister should be laid upon the loins. Beyond these hints I am unable to give you any assistance towards the management of this obscure but serious complaint.

The opposite condition of the kidney, in respect to its peculiar function — that, I mean, in which its secretion is largely and morbidly *augmented* — is mostly as fatal as the total suppression that we have just been considering; but it is not so rapidly

fatal. When the amount of urine secreted and passed is permanently too great, when it is constantly running off, as it were, from the system, the patient is commonly said to have *diabetes*: from *διαβαίω*, to pass through.

But it is not every case of an excessive flow of urine that deserves to be called diabetes. Great quantities of aqueous urine are passed by hysterical and nervous patients. We all make more water in cold weather than in warm; the functions of the skin and of the kidney compensating each the occasional defect of the other. Certain drugs and articles of diet are also well known to cause a temporary excess in the amount of urine secreted.

In fact, although the quantity of urine voided is the most obvious and striking symptom of diabetes, its definite and characteristic symptom is a most remarkable change in the *quality* of that liquid: in its becoming loaded with sugar. You will find, indeed, two species of diabetes mentioned by many authors—the diabetes *insipidus*, and the diabetes *mellitus*. The former term ought in my opinion to be abolished. If it refer merely to an unnatural abundance of urine, not otherwise differing in its composition from healthy urine than in containing a large proportion of water—by calling such a state diabetes we link together in the same genus two essentially different conditions. In true diabetes the urine is never without sugar. The quantity may indeed be small; and it may not be sensible to that coarse test, the *taste*; but modern observers almost all agree in rejecting any species of diabetes in which the urine is not at all saccharine. This condition of the urine was first described in 1684, by our learned countryman, Dr. Willis.

The sensible qualities of diabetic urine differ strikingly, in many particulars, from those of the urine in health. Its chemical quality differs strikingly too, as I have already told you; but it is in one particular only. Fortunately no extraordinary skill is required to recognize the morbid secretion.

Diabetic urine is commonly light coloured, and transparent; of a pale straw, or greenish tint. Being so copious, it rarely exhibits any visible lithates. Its odour is peculiar. According to Dr. Prout the scent somewhat resembles that of sweet hay, or that of milk: but to my nose it is more like the faint smell of certain apples, or rather of an apple chamber. Its taste is, more or less, decidedly sweet. Notwithstanding its limpid and aqueous appearance, diabetic urine is remarkably heavy. When boiled in a test tube with an equal quantity of liquor potassæ it assumes a claret colour of a greater or less depth, according to the quantity of sugar present. This is called *Moore's test*. Its delicacy may be increased, Dr. Garrod says, by *first* adding a drop or two of liquor potassæ to the urine, so as to make it slightly alkaline, and then decolorizing it by the further addition of animal charcoal, and by filtration through paper.

Another good and familiar test is that of *Trommer*. In a large test tube mix with some of the suspected urine just enough of a solution of sulphate of copper to give it a faint blue tint. Then add liquor potassæ in considerable excess. If sugar be present, a precipitate of hydrated oxide of copper first falls, which is redissolved in the excess of alkali, forming a dark blue solution. If this be gently heated to ebullition, a dense deposit of red sub-oxide of copper takes place.

Yeast, again, furnishes a ready and an easy test of saccharine urine. Invert a test tube filled with urine to which a small quantity of yeast has been added, into a saucer also containing urine. In a warm room, fermentation, if sugar be present, will soon commence, and carbonic acid rising in the tube will depress the upper surface of the urine.

What are called *torulæ*, minute vegetable confervoid growths, very soon appear in diabetic urine when it is freely exposed to the air in a warm place. This sugar fungus, as it is also named, has in fact been ascertained to be identical with the yeast plant; and during its growth (and probably in consequence of its growth) the urine undergoes the alcoholic fermentation, bubbles of carbonic acid gas are evolved, and a vinous odour arises. The presence of *torulæ* in the urine has therefore been regarded as good evidence of the presence of sugar also. But this is not a safe inference. Other confervoid forms, requiring an instructed eye to distinguish them from the sugar fungus, do often accompany it: and these are constantly developed in non-saccharine urine also, provided that it be acid, and that it contain albumen, or some other animal matter.

These other confervoid forms constitute stages in the growth of the fungus called *penicilium glaucum*, which is the same that so often gives to decaying animal or vegetable substances a mildewed or mouldy appearance. It has been recently asserted that the yeast plant, the *torula cerevisiæ*, is really identical with this *penicilium glaucum*. Dr. Arthur Hassall, however, in a very interesting paper on the subject, contained in the *Medico-Chirurgical Transactions*, affirms that these two fungi are readily distinguishable, the one from the other, under the microscope, and he gives drawings of both, in the various stages of their development up to their perfect fructification.

However this may be, if you observe torulæ in the urine of a patient, do not, unless you are expert microscopists, conclude at once that he is labouring under diabetes; but taking the hint which they furnish, employ other tests to determine whether his urine be really saccharine or not.

It was long believed that the quantity of urea in diabetic urine was reduced much below the natural standard; and that the sugar was somehow formed at the expense of the urea. Dr. Prout, in his earlier researches, always detected a little, and but a little, of this peculiar principle. Later observations have shown, however, that the urea is not so scanty; nay, that it is generally as abundant as in the urine of health, and sometimes even more so. The presence of the sugar conceals the urea; interferes with the action of the ordinary tests of that substance. By certain modes of procedure, which I need not stop to describe, the urea may readily be discovered: and it is often found, I say, to be rather excessive than deficient. The usual saline matters belonging to healthy urine are present also in that of diabetic persons; and in the same *relative* proportions; but, as might be expected, their *absolute* amount, in a given quantity of the liquid, is very much diminished. In short, the only essential deviation from the standard chemical constitution of the urine is, that it holds in solution a quantity of sugar. This explains its peculiar odour, its sweetness, and perhaps its excessive quantity. It accounts also for another very characteristic property of diabetic urine; I mean its high specific gravity. In general, you know, the specific gravity of the urine is inversely proportional to the quantity secreted in a given time; the more copious and dilute it is, the lighter it is. But in diabetes, so strong is the saccharine impregnation that the specific gravity more than keeps pace with the increased quantity of the liquid secreted. The specific gravity of diabetic urine is always much higher than that of healthy urine.

The *quantity* of urine secreted and voided is sometimes enormous; greatly more than could be supplied by the quantity of fluid taken as drink, although that, as I shall presently explain, is excessive too. A healthy person passes from one to three or four pints of urine in the twenty-four hours. The quantity, as you well know, is liable to considerable variation: perhaps the average may be safely laid at about forty ounces. But patients in diabetes will void 40 *pints* in the same time. I have myself known 26; 13 or 14 are not uncommon; and cases are recorded by writers of credit and veracity, in which 70 pints were passed daily. Nay, one Italian author declares that 200 pints have been discharged in that time.

The saccharine matter thus held in solution may be obtained in its solid form by evaporating the urine. I have seen large flat cakes of beautifully crystallized diabetic sugar. It differs somewhat from common sugar, the produce of the sugar-cane; and approaches more nearly to the sugar of grapes. This kind of sugar, which may also be produced artificially from starch, chemists have named *glucose*. By rapid evaporation of the water a thick syrup is procured, resembling treacle; but Dr. MacIntyre, who has presented to our hospital-museum some very perfect specimens of this sugar, prepared by Mr. Blandford, informs me that to get it well crystallized, the evaporation in a steam bath should be stopped while the urine is still of thin consistence. It may be quickly reduced to one-half, perhaps, of its original quantity. Then it should be set aside, in shallow plates; and in the course of ten days or a fortnight the sugar will be deposited in an irregularly crystalline mass.

The sugar is sometimes so abundant, that it undergoes a rude crystallization as the urine dries, wherever it happens to fall. A girl who was in St. Bartholomew's Hospital while I was a student there, observed that if her water were accidentally sprinkled upon her black stuff shoes, every drop left a white powdery spot behind it. So also an aged patient under Dr. MacIntyre's care expressed to him her alarm at finding that her black worsted stockings were sticky and covered with a white dust,

from the same cause. A man recently under my charge in the Hospital, complained that two pairs of his black cloth trousers had been spoiled in a similar manner. I remember hearing from a diabetic patient in the Edinburgh Infirmary, that his attention was first drawn to his urine by the number of flies and wasps which its sweetness attracted to the chamber-pot. In India the red ants have been observed to swarm in the same way about a vessel containing diabetic urine.

This daily production of sugar from the laboratory of the human body, is surely a very singular and surprising phenomenon. Sugar is not a constituent of healthy urine. Dr. Prout (whose experience on this subject was very large) says in his book that he had never known saccharine matter to occur in the urine of any other animal than man. I once had a coach-horse which I supposed might have diabetes. He was a greedy feeder, and drank eagerly, yet he grew thinner and thinner; and at whatever door I had occasion to stop, there he invariably began to stale: so that I became thoroughly ashamed of his leaking. Dr. Prout was good enough to examine his urine for me. It contained no sugar, but its healthy properties were much changed: it had less than the natural quantity of hippuric acid, and more of earthy matters. The disease, he told me, is known at the Veterinary College; whence specimens of such urine had been sent to him for inspection: but it is not true diabetes.

The unnaturally high specific gravity of diabetic urine is a constant quality; and you must attend to this, for it is almost always a faithful index, not only of the presence, but of the severity of the disorder. Dr. Prout places the specific gravity of healthy urine between 1015 and 1025, that of distilled water being represented by 1000. Different authors vary somewhat in their estimate of the natural standard; but we may be content to follow Dr. Prout. He says that the specific gravity of diabetic urine has been stated to vary from 1020 to 1050: that he has many times seen it higher than this, but very seldom so low. In fact it ranges generally between 1030 and 1060; and the average may be taken at 1040.

So much with reference to the quantity and qualities of the urine discharged in this complaint. It is attended, however, in well marked cases, with other and important symptoms, as you may suppose.

So much fluid being evacuated from the body through this channel, it might be expected that the *other* channels for the excretion of liquid matters would be comparatively dry: and so they are. The skin is arid, harsh, and unperspirable. The patients tell you that they never sweat: that they cannot get into a perspiration. This is a very general symptom; yet in some few patients, especially as the fatal period of the complaint draws near, the surface readily becomes humid. And a friend and patient of mine, whose urine was very copious, and contained a notable amount of sugar for several years together, without any perceptible abatement, however, of his general good health, perspired profusely every night. Again, the bowels are mostly costive, and the *faeces* remarkably solid and free from moisture. The tongue is dry, parched and sticky; sometimes unnaturally red and clean: and the waste of watery particles from the system seems to be felt and expressed by the inordinate thirst which the patients suffer. Their drought is often insatiable. I remember one girl's telling me that when she was debarred from an excess of water to drink, she would get up if she heard it raining in the night, and catch some of the descending drops to satisfy the tormenting sensation of thirst. And another patient, a very sensible fellow, informed me that, believing that it could not be good for him to drink so much, and feeling no confidence in his own resolution to refrain, he was in the habit of betaking himself in the summer time to the fields and dry pastures, where no water was at hand to quench his strong desire for it. The appetite for food is often, but not always, equally keen: and the patients, especially those in the lower ranks of society, are apt to think, while wondering at their loss of strength, that there cannot be much the matter with them, since they continue to eat and drink so famously.

Again, the enormous daily drain upon the system may be expected to cause various symptoms and sensations, which may all be referred to weakness and defective nutrition. A table has been published by Dr. Henry, showing the quantity of solid extract in every wine pint of urine of different specific gravities from 1020 to 1050. Taking 1040 as the average specific gravity, and ten pints as the average quantity, of the urine discharged daily, the patient would in this manner lose, every twenty-four

hours, 15 ounces 7 drachms—or more than a pound and a quarter—of solid materials. Dr. Garrod, after analyzing the urine in several cases of diabetes, found the daily quantity of sugar excreted to vary from half a pound to a pound and three-quarters. It is most abundant three or four hours after a full meal, and least abundant when the secretion takes place at the time most remote from the influence of food.

We need not be surprised, then, at the hunger, the wasting, the hectic fever, the feeling of emptiness and sinking at the stomach, the debility, the chilly state of the body, and especially of the extremities, the aching and sense of weariness in the loins and legs, the aversion to exercise, the loss of virility; all of which symptoms are generally present. I may add, to complete the picture, some others, enumerated by Dr. Watt, and confirmed by Dr. Prout, and consistent with my own experience of the disease. They are, uneasiness in the stomach after meals, flatulence and acid eructations, dimness of vision, redness of the whole interior of the mouth, sponginess of the gums, looseness of the teeth, and some degree of irritation and inflammatory redness about the external orifice of the urethra: these last are symptoms noticed in persons dying of inanition. Again, listlessness and depression of spirits, weakness, and peevishness of temper: “the once vigorous mind becomes feeble, oblivious, and vacillating—the once amiable temper, fretful, suspicious, and intolerant.” With all this there is a peculiar faint and unpleasant odour of the breath and person; an odour which Dr. Prout says is hay-like, which some call melleous, but which reminds me, as I said before respecting the urine, of the smell of a room in which apples have been kept. I have often recognized the complaint, upon first entering the sick chamber, by this peculiar scent.

Diabetes is generally a chronic disorder, creeping on at first insidiously, and spreading itself, under judicious management, over many years. Yet it is sometimes fairly entitled to be called an acute disease; for it occasionally breaks out suddenly, is attended with much febrile disturbance, and runs a short course, uncontrolled by any treatment. One such instance I have seen, and others I know of. Much more frequently it proves fatal through the supervention of some organic mischief, such as debility is calculated to foster and develop. It often becomes associated, in its progress, with pulmonary disease, especially with tubercular phthisis. So common is this, that some persons have thought it universal. But it is not so. I have myself witnessed more than one or two dissections of persons dead of diabetes, whose lungs did not contain a single tubercle. Sometimes the disease terminates in incurable dropsy: and sometimes the patient is cut off suddenly, either by apoplexy, or by some peculiar disorder of the stomach.

There is some kind of connexion between diabetes and certain affections of the skin, and of the subjacent reticular membrane. Dr. Prout remarks that it usually *follows* cutaneous complaints, but *accompanies* or *precedes* those which involve the areolar tissue. Persons have been known to lose chronic eruptions, upon the supervention of diabetes. On the other hand, carbuncles and malignant boils are frequently the companions of that disorder. Cut carbuncles and boils are not necessarily accompanied (as Dr. Prout suspected they were) with saccharine urine. Itching of the skin is not uncommon.

Dr. Garrod has found *gangrene* to be of frequent occurrence in diabetic persons. There can be no doubt that their vital power is always greatly lowered by the disease. They are apt to sink rapidly under any sudden mental shock, under bodily injuries, under surgical operations, and even under unusual fatigue or anxiety.

The same author remarks that upon the supervention of these secondary diseases the sugar frequently disappears from the urine; and cautions us that this disappearance, far from being an index of improvement in the patient's condition, is then really a warning of danger, and too often the harbinger of approaching dissolution.

The examination of the dead body throws little or no illustration upon the pathology of diabetes. We naturally look with interest to the kidneys. But we find nothing there to explain the symptoms noticed during life. What I have usually remarked has been a deep purplish red colour of the kidneys, which were veined and vascular, but not otherwise altered in texture. Andral and others tell us that the kidneys are found hypertrophied in diabetes. But hypertrophy, and unnatural vascularity, are circumstances which we are not surprised at, when we reflect upon the vastly increased quantity of work which the glands have been performing. We must

regard both of these unnatural conditions rather as being consequences, than as being causes, of the profuse and long continued flow of unnatural urine. In one instance, after sudden symptoms of gastritis, which followed the incautious potation of strong ale, I found the mucous membrane of the stomach distinctly inflamed, in its cardiac portion. I have seen also the mesenteric glands diseased, converted almost entirely into bone. But neither of these changes are constant. They were purely accidental in those particular cases.

What, then, is the origin and source of this strange complaint? whereabouts in the body is the sugar formed? is it made, by the kidneys, from the blood? That was one of the earliest conjectures. It was naturally thought that, if the sugar pre-existed in the blood, and was only withdrawn from it by the kidneys, it would be discoverable in the blood. Yet able chemists sought for it there in vain. Hence it was inferred, that by some new combination of its elements, saccharine matter was actually formed in the kidneys. The chemistry and the reasoning were both faulty. Sugar has, now, been detected both in venous and in arterial diabetic blood. It is detected with some difficulty, partly perhaps because its presence is masked by the albumen of the serum, but partly because its quantity is small; and its quantity is small because it is continually decanted out of the blood, as fast as it enters, and with it a profusion of water also, through the kidneys. In this respect the sugar and the urea are alike. They are both excretions which the blood is in haste to cast forth. It would seem also as if the sugar necessarily carried with it a large quantity of aqueous fluid from the blood, and was simply diuretic. When the amount of sugar eliminated is diminished, as by certain remedies it may be, the quantity of urine diminishes too.

Traces of sugar had, I believe, been found in the blood by some previous inquirers; but it is to Mr. M'Gregor, of Glasgow, that we are indebted for the full exposition of this interesting fact. His researches on this subject were published in the year 1837, in the *Medical Gazette*.

By a peculiar process, he did, I say, that which many preceding chemists had failed to accomplish; he detected sugar in the serum of the blood of diabetic patients. The serum had a milky appearance, he says; and I have seen that myself; its specific gravity was above the healthy standard. Having coagulated the serum by heat, he carefully dried it; then he cut the dried mass into very small pieces, and boiled them in distilled water; and lastly, he evaporated the decoction to a certain point. To the liquid thus concentrated he added a portion of yeast, and the presence of sugar was manifested by the fermentation which ensued, and which lasted for several hours. Yeast, I should have told you, is a very delicate test of sugar, and will readily detect half a grain in two ounces of liquid.

Mr. M'Gregor went a step further back. He obtained, by means of an emetic, the digested food from the stomachs of two men who had dined two or three hours before. One man was in health; the other had diabetes. In each case the food had been of the ordinary kind. Applying, after due preparation, the test of yeast, he found that the vomited matters fermented strongly; especially those from the diabetic patient.

Then he varied the experiment. Thinking that the sugar, in these cases, might have been introduced in the vegetable portion of the food, he adopted precautions to exclude that possible source of fallacy. He administered to a healthy man, and to a diabetic man, a vomit and a purge, to clear out the alimentary canal. Next, he fed them upon roast beef and water, and nothing else, for three days. Then, three or four hours after a meal, the contents of their stomachs were procured by the operation of the sulphate of zinc, as an emetic; and treated as in the former case. What the healthy man vomited did not ferment at all. What came from the diabetic patient fermented "pretty briskly."

The inference from these facts seemed direct and inevitable that the fault lay in the *digestive* organs; that instead of perfect and nutritive chyle, saccharine matter was formed by the stomach, and entered the circulation unaltered. In healthy digestion all amylaceous food is first converted, the chemists tell us, into glucose, which then undergoes further changes. In diabetes these further changes were somehow prevented. The food which should be transmuted into muscle, and fat, and bone, and nerve, and membrane, was hurried out of the system, as sugar, with the urine.

This plausible theory has been disturbed by the surprising discovery, made and au-

nounced by M. Claude Bernard, that within all healthy persons, whatever may be their food—nay, within all animals, whether herbivorous, carnivorous, or omnivorous—a manufacture of sugar is constantly going on. The apparatus for this manufacture is planted in the liver, in the tissue of which sugar may always be detected, while every other organ of the body is destitute of it. No sugar can be found in the blood of the portal vein: plenty of it exists in that of the hepatic veins, even when the food (as in experiments made upon dogs, and young owls) has been exclusively animal for months beforehand. The sugar is traceable onwards in that part of the vena cava inferior which lies between the entrance of the hepatic veins and the right auricle, in the right chambers of the heart, and in the pulmonary arteries. The rest of the blood, during the intervals between successive periods of digestion, is devoid of sugar. While digestion is in full process, the blood which has passed through the lungs contains a slight impregnation of saccharine matter, not enough, however, to reach, in any appreciable amount, the urine.

The sugar formed in the liver, disappears then, according to M. Bernard, in the lungs. The observations of Dr. Pavy, published in the *Guy's Hospital Reports*, make it probable that it is consumed in those organs, or converted into some other substance, perhaps into lactic acid.

Dr. Harley denies, however, that this sugar is wholly expended in the lungs; and states that it finally disappears in the capillaries of the general circulation.

Pushing his investigations somewhat further, M. Bernard has satisfied himself that, by a vital process, a substance analogous to vegetable starch is formed in, or by, the livers of all animals; and that, by a chemical process, this substance is convertible into dextrine, and sugar. Sugar continues to be produced in the livers of healthy animals, for some little time after their sudden death.

Pray notice, *en passant*, the remarkable fact brought to light in these researches, and wholly unknown and unsuspected before, that the liver exercises a twofold and contrasting office of secretion. While it separates outwardly and obviously what may be deemed an excrement from the blood, namely bile; it secretes, inwardly and occultly, an increment into the blood, namely sugar. This fact is pregnant with suggestions as to the probable functions of the ductless glands of the body, which have no outward secretions; the spleen, the supra-renal capsules, the thymus and the thyreoid glands.

Another most singular discovery of Dr. Bernard's, connected also with the subject before us, is, that *artificial* diabetes may be produced at will in an animal by irritating (by means of a puncture, or of a slight galvanic shock) a portion of the floor of the fourth ventricle of the brain, close to the origin of the pneumogastric nerves. Within an hour or two the urine of the subject of the experiment becomes saccharine; and it continues saccharine for a whole day, or more; until (probably) the slight injury done to the nervous substance is repaired. He ascertained also that the sugar-forming function of the liver was suspended by division of the pneumogastric nerves. Not that the hepatic secretion of sugar is determined by the direct influence of those nerves; for pricking the floor of the fourth ventricle is followed by the appearance of sugar in the urine, even after the section of the pneumogastrics, which must therefore be regarded, in respect to the sugar-forming power, as incident nerves.

From these interesting discoveries has naturally arisen a new and simple theory respecting the pathology of diabetes. Either the hepatic sugar is formed in excess; and there is more of it than can be consumed in the lungs, or in the capillaries of the general circulation: or, the natural quantity of sugar furnished by the liver fails, in some way, to be so destroyed. In either case the sugar, thus remaining in the blood, passes at length from that fluid into the urine.

But there are two facts which strike our attention at once as jarring somewhat with the simplicity of these conclusions. The natural secretion of sugar goes on in the liver of healthy persons irrespectively of the kind of food which they may eat: whereas the quality of the food has a most striking influence, as I shall presently show you, upon the amount of sugar in the blood and in the urine of diabetic persons.

And the doubt thus suggested concerning the sufficiency of the new theory is strengthened by some curious facts made known still more recently by Dr. Owen Rees. It has been found by himself, and by Dr. Pavy, that the hepatic sugar, and the sugar of artificial diabetes, which seem to be identical, and which may be distin-

guished by the name of *animal* sugar, differ sensibly from the sugar of true diabetes, which I have already told you is akin to the *vegetable* sugar derived from grapes. The reaction of the animal sugar with some of the tests is peculiar; but the main distinction is the facility with which it is destroyed when kept in contact with blood, or other animal matter. Grape sugar and the sugar of true diabetes possess a power of resisting decomposition far stronger than that which exists in the sugar obtained by producing artificial diabetes. "There is little doubt," says Dr. Rees, "that the sugar of diabetes is a higher quality of the saccharine principle, and that it can preserve its atomic arrangement with much greater force than the hepatic variety."

The great problem therefore is not yet solved, although we seem to have come nearer to its solution. Meanwhile a large field is left open for conjecture and query.

It is probable enough that the exciting cause of diabetes may sometimes lie in the digestive organs, as Mr. M'Gregor supposed. The results of his experiments do not conflict with M. Bernard's. Mr. M'Gregor, you may remember, found sugar in the partly-digested food brought up from the stomach of a diabetic patient. He detected it also in the saliva; and in the fæces, which when allowed to dry spontaneously, became covered, after the lapse of some time, with distinct crystals of sugar. And yeast having been administered to two diabetic patients in ounce doses after each meal, had soon to be discontinued because the patients, to use their own expressions, felt as if they "were on the eve of being blown up." There being sugar in the blood, we need not be surprised that he met with it in the gastric and intestinal secretions from the blood. Dr. Harley has observed that by injecting irritating matters into the portal vein (ammonia, ether, chloroform, alcohol) a saccharine condition of the urine may be artificially produced. It is conjectured that these substances act upon fibres of the pneumogastric nerve, whence an impression is transmitted to the nervous centres, and thence is again reflected upon the liver through the splanchnic nerves. It is not difficult to imagine that irritating substances may find their way into the portal blood, through a faulty digestion, or through the use of certain kinds of food or of medicine. Again, since contrived irritation of the brain at the origin of the pneumogastric nerves will make the urine saccharine, the cause of diabetes in the human subject may reasonably be placed, in some instances, within the skull; and we may understand how injuries or diseases of the brain, or even mental disquiet and dejection operating through the brain, may produce it. Some striking cases have been published by Dr. Gooldeen, in which head symptoms were accompanied by saccharine urine, and in which the diabetic symptoms were checked or removed by remedies addressed to the head affection; by blisters especially, and by purgatives. Nay, we may ask whether there may not, in fact, be two varieties of diabetes mellitus, in one of which the animal, and in the other the vegetable, form of sugar may be present in the urine: and whether the one of these varieties may not be more hopeful of cure or recovery than the other. Bearing in mind the name and the distribution of the pneumogastric nerve, may we not indulge the conjecture that disease or injury of the brain near the origin of that nerve may directly affect the functions of the *stomach*, and thus prevent its digestive power—or the functions of the *lungs*, and thus interfere with the chemical destruction of sugar in those organs. This last notion might seem to receive support from the frequent association of pulmonary disease or disorder with saccharine urine.

Dr. Rees appears disposed to place the main error in the hepatic functions; and to suppose that in diabetes the liver, from some diseased action in itself, or in the organs that influence it, "produces sugar differing from that of health—a sugar which cannot be destroyed by the changes taking place naturally in the blood—changes rapidly affecting and destroying healthy hepatic sugar." After showing how readily the different varieties of sugar are convertible into each other by simple processes, Dr. Rees adds some observations so new and so suggestive, that I shall take leave to read them to you:—

"In reflecting on the phenomena of this important disease, we are naturally led to consider the anatomical relations of the organs most obviously concerned in its causation; and the stomach and liver must most especially present themselves to our notice. The position of these organs—situate as they are in the immediate vicinity of the central masses of the sympathetic, the semilunar ganglia and the splanchnic nerves, and supplied also with branches having immediate connection with the cerebro-spinal

system,—is in itself suggestive of the high importance of the offices discharged by them, of their sympathies and close relation. In this part, then, of the living and moving organism, a most intimate union is effected between the sympathetic and cerebro-spinal systems—systems presenting a curious and doubtless intentionally different arrangement as regards their chemical relations, and one which has been (as far as I can ascertain) entirely overlooked by physiologists; an arrangement having most certainly a deep meaning, and one which I fully believe is to throw light on many obscure problems in medical science. I allude to the distribution of the ultimate fibrillæ of the sympathetic and cerebro-spinal systems. Thus the ultimate extremities of the sympathetic are almost entirely spread over parts possessing an alkaline reaction, while precisely the opposite is the case with the fibrillæ of the cerebro-spinal system. The former lie in mucous and serous surfaces, and in the blood-vessels, bathed with alkaline fluid; the latter lie in muscular fibre, and in the cutis as cutaneous nerves, and in both cases are bathed in strongly acid secretions. Now there is no part of the organism to which we can point in which these chemical affinities are so freely interwoven as in the neighbourhood of the liver and stomach; and here again we are attracted by the peculiar nature of the secretions poured out by these organs. They seem to be exceptions to the general rule—to be the parts, as it were, acted upon by the electro-chemical arrangement, and parts to which it is subservient.”

We know but little about the outward causes of diabetes. It is not a very common disorder; and in those who become afflicted with it, there probably has existed a predisposition to it. Dr. Prout remarks that the complaint runs sometimes in families, and is inherited. I had under my own observation, for some time, three children, two brothers and their sister, all affected with diabetes. The mother, a maternal uncle, and a sister of a friend of mine, all died of this malady. The same author mentions among the predisposing causes, long-continued intemperance, and especially the immoderate use of spirits, severe evacuations, excessive labour joined with a poor acescent diet. Distress and anxiety of mind are held also, and justly I think, to be among the predisposing causes. It occasionally seems to be produced, at once, by the operation of some exciting cause, such as exposure of the body to cold; or the drinking of large draughts of cold fluid while the drinker was hot and perspiring. Dr. Bardsley states that, in twelve instances of the disease which had fallen under his own notice, the patients attributed their ailment to one or the other of these two causes. Now these are common causes of disease; and that a predisposition does exist is probable from the fact, that where the exciting cause has acted on several individuals at the same time, one alone has become affected with diabetes. There is a narrative illustrating this, by Sir Henry Marsh, in the third volume of the *Dublin Hospital Reports*. A patient of his traced the apparent origin of his diabetes to exposure to wet, cold, and privation, at sea, while in imminent danger of shipwreck. Another of the crew fell ill of ague. Others escaped entirely, or had only common colds.

Diabetes is a malady which justly alarms those who are the subjects of it. But though too often a fatal malady, it is not necessarily so: and the older I grow, the less despondence do I feel upon first ascertaining that a patient is voiding saccharine urine. Whether it be that the disease is really more common, and at the same time milder and more tractable than it formerly was—or whether (what seems more likely) it is more carefully looked for now-a-days, and more easily recognized, certain it is that I both see, and hear of in the practice of others, many more instances of diabetes than I did some years ago. Of the well-marked cases, a few—not many—have recovered perfectly, and so far as I know permanently, even to the return of the urine to its natural specific gravity. Others have gone on, by slow or by rapid steps, to a fatal termination. In not a few, the main symptom of the complaint, I mean a considerable impregnation of the urine with sugar, has continued for months and years, without material deterioration of the general health, until the patient has been cut off by some other illness. It would seem that when the digestive organs are capable of properly assimilating a sufficient quantity of food to sustain the bodily fabric, other portions of the aliment may run off in the form of sugar with comparative impunity to the health. But even this, the best state in which a person who remains diabetic can be, is a perilous state.

I must caution you against a premature conclusion that your patient is well. Apparent recoveries—nay, apparent cures—are not very uncommon. And this it is of great importance to know. Remedies are not useless because they fall short of their full scope. It is better to keep a man on the edge of a precipice, if you cannot pluck him away from it, than to let him fall over. And many diabetic patients are kept in this predicament of dangerous safety. There are certain remedies that exercise a strong controlling influence over some of the most prominent and troublesome of the symptoms; and that sometimes even restore the patient to a state which he mistakes for health; and which a medical man, unwarned of its fallacious character, might also mistake. The urine may recede within its natural limits. There may remain *one* morbid circumstance only, and that of a nature easily overlooked: indeed it is sure to escape observation if it be not especially searched after. I allude to the unnaturally high *specific gravity* of the urine. So long as the density of the urine continues permanently and decidedly above the healthy standard, there is no real security. The smallest disturbing cause—exposure to cold, an intemperate meal, unusual exertion and fatigue, sudden or strong mental emotion—may bring back all the symptoms in their former severity. If these and similar hurtful agencies can be averted, life may sometimes be prolonged, in much comfort, for many years.

In an interesting paper, which you may see in the 36th volume of the *Medico-Chirurgical Transactions*, Dr. Bence Jones calls attention to cases of *intermitting* diabetes, of which he relates several. The peculiarity of such cases is, that without obvious cause the sugar disappears from the urine at intervals; and sometimes just before these intervals occur, the urine is found to be full of free uric acid, and of crystals of oxalate of lime. He notices also, and partly confirms the observation of Deschambre, that sugar is frequently present in the urine of old people without producing very urgent symptoms: sometimes, indeed, with none of those symptoms which would naturally suggest a search for it.

In the treatment of every case of diabetes, there are three objects to be kept in view.

First, to restore the defective power of the digestive apparatus, whether the fault be in the stomach or in the liver:

Secondly, to cut off, or restrict as much as possible, the supply of saccharine matter from without:

Thirdly, to mitigate or remove the most distressing symptoms.

If we could achieve the first of these objects, the other two would fall out of sight; for the disease, which may be regarded as a variety of dyspepsia, would be cured. But hitherto the resources of our art have, in this respect, been baffled. Our main hope of ultimate success must lie in the regulation of the *ingesta*; whereby, also, the second indication is to be fulfilled. *Some* of the food is, in every case, carried to the proper account, or the patient would speedily die. If we can succeed in directing a sufficient amount of healthy nutriment to the organic tissues of the body, the draining away of any superfluous sugar will be borne well enough.

About the beginning of the present century, Dr. Rollo discovered and taught that a diet composed exclusively of animal matters had a signal effect in reducing the quantity and in diminishing the sweetness of diabetic urine. And the reason of this is obvious. Animal food furnishes but scantily the materials for the formation of sugar. "The saccharine alimentary principles are chiefly derived (says Dr. Prout) from the vegetable kingdom, and indeed constitute what may be called, by way of distinction, *vegetable aliments*." If then, we exclude aliments of this kind, and confine the patient to animal food alone, we thus cut off at least one supply of the *materies morbi*; and without indeed curing the disorder, suspend its worst effects. But unluckily very few persons can long endure this mode of living. So far as they can endure it, they are comparatively safe. We are obliged to relax a little, sometimes, the rigour of our rule; and it is curious to observe how suddenly and decidedly the saccharine properties and the quantity of the urine are augmented, when, by stealth, or by permission, the patient adds to his meal the smallest portion of vegetable food—even a biscuit or two.

I quite agree with Dr. Garrod in believing that the regulation of the diet constitutes by far the most important part of the treatment. If care be taken to exclude all articles of food that contain saccharine matter, or that are readily convertible into saccharine matter, the condition of the patient improves at once. His thirst abates,

his appetite becomes more natural, the state of his tongue and of his skin improves, and his strength and weight augment. In one example, recorded by Dr. Garrod, the daily amount of urine was reduced within a few days, by regimen alone, from 354 to 100 fluid ounces, and the daily quantity of sugar voided with it from 26 ounces avoirdupois to rather less than 4.

In another instance, the quantity of urine under the ordinary diet of the hospital, averaged 267 fluid ounces daily. The patient being then put upon meat diet, the bran-loaf, cod's liver oil, and laudanum, the urine sank in quantity, in five days, to 91½ fluid ounces. And it rose again to 216 ounces upon the resumption by the patient of the ordinary diet.

In both these cases the general symptoms underwent a proportional improvement, as the amount of urine became less.

We must contrive then to vary the animal diet as much as we can; encouraging the patient by a free license to choose among the different kinds of meat, game, poultry, fish, and eggs, in their diversified modes of preparation, and admitting into his bill of fare as small an admixture as possible of vegetable substances. Green garden-stuff, the oleracea, spinach, cabbage, celery, water-cress, lettuce, and the like, may be taken with less risk of increasing the saccharine matters in the system, than potatoes, and those other articles of vegetable diet which contain a notable proportion of sugar or of starch. All kinds of fruit must be forbidden. You will seldom be able to debar your patients entirely from bread: none should be allowed but such as is well fermented, and somewhat stale, or thoroughly toasted; and even that as sparingly as may be.

The *bran-loaf* just mentioned — or, which are better, *bran-cakes* — form an excellent and not unpalatable substitute for ordinary bread. But they require to be carefully made. You may find Mr. Camplin's directions for making them, in the 38th volume of the *Medico-Chirurgical Transactions*, in a paper by that gentleman upon the *juvantia* and *lædientia* in diabetes, suggested by experience of the disorder in his own person.

It is of some importance to admonish the patient, whose appetite is generally ravenous, against eating too large a quantity, even of animal food, at any one time. Not only is the digestion still further weakened and oppressed by an intemperate meal, but the patient's life may be put in peril by every such act of unwise indulgence. Of this I have witnessed one example, and have heard of several more.

The quantity of drink should likewise be limited. It may properly enough consist of animal broths: and these should be taken *tepid*, for they are then more likely to be taken in moderation. The patients must, however, and will, have something else, to slake their urgent thirst. I have found (acting upon a suggestion of the elder Dr. Latham's) that distilled water, acidulated with phosphoric acid, appeases, more than most things, that painful sensation. The water of the Bristol Hotwell, which contains carbonate of lime in solution, is praised by Dr. Prout for the same purpose. Claret, as it contains no sugar, is an excellent and agreeable beverage for diabetic persons who can afford to drink it.

Dr. Christison gives an important caution in respect to drinks; viz., that, when the thirst has already been much indulged, the quantity of liquid taken must not be greatly reduced all at once. Sudden failure of the vital powers has not unfrequently ensued upon abrupt changes of that kind.

Very numerous are the remedies which have been tried, and which have been recommended for this disorder. I shall notice those only of which I have had personal and in some degree favourable experience.

The first of these is *blood-letting*, which has been strongly advocated by Dr. Watt, of Glasgow, and by Dr. Satterley, formerly one of the physicians to the Middlesex Hospital. It is not a remedy which would naturally occur to one's mind as being likely to prove of service in such cases; but both the authors I have mentioned speak of it in terms of high commendation. They affirm that, under small and frequent bleedings, the strength increases; the clamminess of the mouth, and dryness of the skin, diminish; and the blood, by degrees, assumes the buffy coat. I have once seen the method of frequent blood-letting put fairly to the test. At first the patient did seem to be benefited by it; but she ultimately died: and I am satisfied that her death

was accelerated by one bleeding too many, or by too large an abstraction of blood at one time.

This measure has the best chance of being useful, when the malady is recent, and attended with febrile disturbance. In chronic cases, in old persons, and whenever the debility is already great, venæsection can seldom be requisite or proper; although even then, as Dr. Prout remarks, it may be borne better than one might expect. Local bleeding is, however, of much service in relieving local uneasiness. Leeches may be applied to the epigastrium, if the patient have tenderness there, or complain of a sense of fulness or of burning in the stomach. Cupping to the loins, if they greatly ache.

Opium is a treasure to us in this disorder. It quiets the nervous irritability of the patient, allays many of his most distressing sensations, and restrains in a remarkable manner the morbid profuvium from the kidneys. But you must not suppose, from observing these favourable changes, that you are curing the disease by it. It appears to control the diuretic influence of the sugar in the blood; but it does not banish the sugar itself. And, as far as my experience goes, Dr. Prout is in the right when he states that moderate doses of opium generally suffice to check the excessive discharge. From three to five grains of Dover's powder, for example, three times a day, will do as much good, and on the other hand be productive of far less inconvenience, than larger quantities of that narcotic substance. The sudorific properties of this compound are thought to render it eligible: although it has seldom any apparent effect, in that way, in diabetes. If the ipecacuan which it contains should disagree with the stomach, an equivalent quantity of any other preparation of opium may be substituted for it.

There is another remedial measure which has also, in some cases at least, a most beneficial influence on the condition of the patient; I mean forced perspiration—perspiration induced by the hot-air bath. Of this I have seen some striking examples. A very well-marked case of diabetes came under my care in the Middlesex Hospital several years ago. A vapour, or hot-air bath, had just then been constructed in the hospital, and I thought it a good opportunity for trying whether the suspended functions of the skin might not be restored, and the extravagant action of the kidneys perhaps corrected, by that powerful mode of exciting perspiration. I should tell you that other plans of treatment had already been put in force, with but partial advantage. It would occupy more time than I can now spare to enter upon the details of this case; but I will read to you the man's own statement which he wrote down before he left the hospital, in evidence of the benefit he derived from the *sudatorium*.

"The urine" (these are his words) "is reduced more than one-half, and does not contain much sweetness, but sometimes tastes salt, with a mixture of bitter. My stools, which were dry, and like balls packed together, are now quite natural. The pains in my limbs are entirely removed. My spirits, which were very much depressed, are now revived and cheerful. The unpleasant aching of my kidneys, of which I spoke little lest I should be cupped in the loins, is now removed, only I feel weak there. I am cured of the pain in my stomach, and the circuitous working of the wind in my bowels, which formed lumps in my belly as it passed, resembling those formed by the cramp. I have likewise got rid of the palpitation at my breast, which was accompanied with a sort of dread. My breathing is much improved; perspiration in a great measure, restored; and my skin, which was dry, is now become moist. I sleep well at night, whereas I *could* not sleep more than two or three hours out of the twenty-four. My thirst, which was excessive, has ceased to be troublesome."

This man, who, in the statement I have just read, has so graphically described his own morbid sensations, and the relief from them which he had obtained, left the hospital thinking himself well; *but*, the specific gravity of his urine remained above 1030.

In about half a year afterwards, he went one evening to Hyde Park to see some fireworks, got wet feet, and began to cough. The diabetic symptoms returned more severely than ever; and he soon died. I found his lungs stuffed with tubercles.

In furtherance of the principle upon which the use of the hot bath is recommended,

the other well-known methods of promoting the natural functions of the skin should be followed; friction, and more especially warm clothing.

Steel is sometimes singularly beneficial in repairing the strength, and enlivening the spirits; as indeed it is well known to be in other forms of disease attended with a copious and permanent drain upon the system, and with a diminution of red blood. Of course it may be combined with opium or with any other medicine which the circumstances of the patient may render needful.

I scarcely need say that the bowels require attention. Not that active purgation is advisable, but simply their regulation. Castor-oil, rhubarb, aloes, lenitive electuary, are more appropriate in these cases than the purgative salts, which are apt to be diuretic also.

There is one other drug from which I think I have seen beneficial effects. I mean the *creasote*. I first became acquainted with its virtue from prescribing it, almost accidentally, until a consultation could be arranged with Dr. Prout, for a child, eight years old, in whom the disease was well marked, and who had been brought to London from the country for advice. She had been rapidly wasting away for ten weeks, was extremely feeble, soon tired, very thirsty, especially at night, and had (what was also new to her) an enormous appetite. She was passing from three pints to two quarts daily of pale urine, having a specific gravity greater than 1040. I desired that her diet should be as exclusively animal as she could bear, and that she should take one minim of creasote, suspended by means of mucilage in an ounce and a half of water, three times a day. Curiously enough, the child liked the tarry flavour of the medicine. Upon this plan, with gentle aperients occasionally to regulate her bowels, she remained for upwards of a twelvemonth. Her urine soon fell in quantity within the limits of health, and in density to about 1030. She regained her lost flesh, strength, complexion, and spirits, and grew considerably. At length she suddenly sank under an obscure affection of the chest. A brother of this little girl has lately manifested unequivocal signs of the same complaint; and in him it appears to have been equally checked by the same method of treatment. I have detected sugar in the urine of another boy belonging to the same family.

There were two points in this young lady's case which deserve a passing remark. As her urine diminished in quantity it began to deposit the lithates. This Dr. Prout considered a very promising earnest of a return to a better diathesis: and I find that in cases which have crept on insidiously from the first, he was accustomed to *date* the malady from the time when lateritious sediments, previously common, disappeared from the water.

The other point was, that calomel always sickened this child, and aggravated all the diabetic symptoms. This is consistent with Dr. Prout's experience, who tells us that he has never seen mercury do good in diabetic disease; but on the contrary almost invariably do mischief. I would advise you to read his judicious observations upon the too common abuse of that mineral.

I am quite aware of a possible fallacy in the case I have been mentioning, with respect to the efficacy of the creasote. With the use of that drug was associated a strict adherence to a purely animal diet; and it is impossible to estimate with precision the separate effects of these two remedial measures. I have frequently prescribed the creasote with similar results. It would however, in my opinion, be a mistake to go on increasing the dose. To most stomachs it proves irritating and hurtful when pushed beyond a certain small amount. The late Dr. MacIntyre told me that he had found the creasote very useful in diabetes. I must however acknowledge that, in common with others, I have sometimes been totally disappointed by it.

For many years of my professional life I had not met with an instance of what has been termed diabetes *insipidus*: from which I infer the rarity of that disorder. I shall use the term *chronic diuresis* to express this affection.

Very recently a marked instance of such chronic diuresis has presented itself in the hospital. A boy, eleven years old, not unhealthy looking, but lean, was admitted there under my care. He was much troubled by thirst; and by frequent micturition, which even in the night disturbed him many times. His bowels were costive, he had a capricious appetite, and his skin was dry. He voided during the twenty-four hours several pints, seldom less than nine or ten, of simply dilute urine, of a faintly yellow-

ish hue, and having the specific gravity of 1002. Sometimes, indeed, it was found to be scarcely heavier than distilled water. In other respects he appeared tolerably well. It was supposed that he had been affected for about twelve months; his desire for drink having been the first symptom noticed.

During his residence, of many weeks, in the hospital, under my observation, I made trial of every plan and drug that I could think of, for repressing the unnatural flux of urine. Nothing did him any good: some things, I fear, by disturbing his stomach and bowels, did him some temporary harm. He went out much as he came in.

Some of these cases appear to depend upon excessive thirst, arising from an unhealthy state of the mucous membrane of the pharynx, and are apt to end in phthisis. M. Becquerel has applied to the disorder the title of *polydipsia*. I have mentioned the foregoing example because it afforded me a proof that liquid may be absorbed into the body from the atmosphere; either by the external skin, or by the pulmonary mucous membrane, or by both these surfaces. Dr. Prout, to whom I showed both the urine and the patient, advised that, for a time, his supply of drink should be limited. Accordingly, very much to the poor boy's sorrow, he was put upon a daily allowance of a pint and half. I have no doubt that my injunctions were strictly observed, both by himself and by the nurses. Nevertheless, without losing flesh or weight, he passed, during the corresponding twenty-four hours, ten pints and a half of urine.

I got evidence of the same fact in another way also; namely, by weighing the boy at short intervals: although the experiment was not repeated so often as I wish it had been. I give you the results of one of these trials.

Immediately after he had emptied his bladder, he was found to weigh 3st. 8lb. 0oz. 3dr. Three hours subsequently, having taken nothing in the interim, he weighed 3st. 9lb. 0oz. 2dr. Then he voided 16oz. of urine: after which his weight was again 3st. 8lb. 0oz. 3dr. So that he must have imbibed about a pound of liquid in that brief space of time.¹

In this instance there was merely an excess in the aqueous ingredient of the urine: the solid matters were apparently there, in their due proportion to each other, but in a very small ratio to the water.

But the aqueous ingredient may be in excess, while the absolute quantity of *urea* is deficient. On the other hand, with an excess of the watery material, there may be an excess also in the quantity of *urea* it contains.

Dr. Willis has distinguished these three varieties of chronic diuresis by the terms *hydruria*, *anazoturia*, and *azoturia* respectively. Although I am no friend to the multiplication of technical names, I must tell you that the distinctions expressed by these terms are real, and of some importance. *Anazoturia* we shall find to be often symptomatic of a peculiar organic disease of the kidney, which I hope to describe in the next lecture.

Azoturia, which is accompanied by an unnaturally high specific gravity of the urine, is apt, on that account chiefly, to be mistaken for diabetes. As recoveries from it are not uncommon, it may be suspected that some of the boasted cures of diabetes were cures of this less serious disorder. In the one case, the yeast-test detects the presence of sugar in the superabundant urine; in the other case it finds none.

¹ This boy lived more than two years afterwards, and continued to void an excessive quantity of pale, neutral urine, scarcely heavier than distilled water. After death, scrofulous tubercles were found in his brain, and in his lungs. His kidneys were gorged with venous blood, but of healthy structure. There was nothing wrong, apparently, in his organs of digestion.

LECTURE LXXVIII.

Albuminous Urine. Means of detecting the Albumen. What it imports. Anatomical characters of Bright's Kidney. Symptoms to which this renal disease gives rise. Nature of the affection.

ANOTHER morbid condition of the urine, imperatively demanding your attention, is that in which it is habitually impregnated with *albumen*. This albuminous condition is much more common, and in general not less serious, than the saccharine condition which I described yesterday.

There is no albumen in healthy urine. Neither can we recognize its presence, in any urine, by mere inspection. We detect it by certain tests: and I will tell you, in the first place, what these are; and how to use them.

Albumen — of which we have so familiar an example in the “white” of eggs — begins to pass from the fluid to the solid state at the temperature of 160° *Fahrenheit*. When diluted it may require for its complete coagulation the heat of 212° . Hence one simple and easy test of its presence. We discover that albumen is contained in the urine, by heating that fluid to the boiling point. This is most conveniently done in a small glass tube, by the flame of a spirit lamp. It is seldom that any preparation of the suspected urine is requisite. It may, perhaps, be hazy in consequence of its containing *mucus*; and if its transparency be much troubled, it will be well to filter the fluid before testing it. When, as sometimes happens, albuminous urine is already turbid from the presence of the lithates, these dissolve as the heat is applied, and the urine first becomes clear; and then, as the temperature rises, the albuminous opacity begins to be visible.

The phenomena observable in the heated urine vary in different cases, chiefly by reason of the variable amount of albumen. The whole is sometimes converted into one gelatinous mass: but this is uncommon. Usually the albumen first appears in the form of a whitish cloud, of which the constituent particles multiply, and collect, in proportion as the quantity is considerable, into small curdy fragments or flakes. These soon subside to the lower part of the tube, leaving the supernatant liquor clear. The amount of albumen is of course to be estimated by the height to which, after such subsidence, the tube is filled by it.

Now this test, by heat, is not conclusive, nor sufficient. There are circumstances that may impede or prevent its effect in coagulating albumen, which, nevertheless, is present. On the other hand it may, under other circumstances, produce a fallacious appearance of albumen where none exists.

Albuminous urine has often a less acid reaction with litmus paper than healthy urine. The reason of this I will explain presently. When recently discharged from the bladder the urine may be neutral or even alkaline; or it may become neutral or alkaline, by spontaneous decomposition after it has left the bladder. In any case, the urine thus alkaline or neutral will not coagulate when heated, even though it may be full of albumen. The alkalescence must be owing to the presence of ammonia, or of soda; but the compounds formed by albumen with these alkalies are neither of them coagulable by heat.

Again, although there may be no albumen, heat may cause a flaky precipitate, consisting of the earthy phosphates.

We avoid, or we remedy, these sources of fallacy, by testing the suspected urine with nitric acid also, which has the property of precipitating the albumen in a flaky or pulpy form. It will thus detect albumen when the tested urine is alkaline. It has likewise the effect of redissolving the spurious precipitates which may be thrown down by the application of heat, and consequently of showing that they are spurious.

Nitric acid alone, however, is not, any more than heat alone, an unequivocal touchstone of the presence or absence of albumen: for if the lithates, or even if urea, be present in excess, a buff-coloured amorphous precipitate of lithic acid may be thrown down—or nitrate of urea may be formed—upon the addition of nitric acid, when there

is no albumen. But this defect is compensated by the complementary criterion of heat; these precipitates being redissolved by raising the temperature of the urine, while any coagulated albumen remains insoluble.

And always this test, by nitric acid, requires to be applied with a certain degree of care and delicacy. The risk is, of being misled through using too little of the acid, or too much. The compound resulting from the union of nitric acid with albumen—what may be roughly spoken of as the nitrate of albumen—is soluble in water, and is not coagulable by heat. If, therefore, just so much nitric acid (and no more) be mixed with the urine, as combines with all the albumen that it contains, no precipitate will take place. But when more is added, the nitrate of albumen, being insoluble in dilute nitric acid, becomes at once apparent in the fluid. Again, any great excess of nitric acid may redissolve the albumen.

Hence, in testing suspected urine, it is better to operate on a small quantity of it, a fluid drachm for instance, filtering it first if it be cloudy, then boiling it, and then dropping into it two drops of nitric acid: instead of nearly filling the test tube with urine, and adding a drop or two only of the acid, as is often done—or of adding as much acid as there is urine, which is perhaps a less common mistake.

Also, take care that your test tubes are *clean*; and do not contain, as they may do if merely just emptied, any trace of acid, or of alkali.

Furthermore, albuminous urine of excessive acidity, from its containing a free acid, may fail to exhibit albumen when heated. Either the acetic, or the hydrochloric acid will prevent it: in other words, the acetate and the hydrochlorate of albumen are both of them soluble in water, and uncoagulable by heat.

This source of error is also to be obviated by the addition of a small excess of nitric acid, after heating the urine: so that by observing the directions just given, you may avoid all risk of mistake.

Other chemical tests there are, frequently spoken of, and sometimes recommended: particularly the ferrocyanate of potass, corrosive sublimate, oxalic acid, and creasote. They are unnecessary, in addition to heat and nitric acid; and they are liable to fallacies from which these last, when combined, are free. Unless you are expert chemists, you had better avoid them.

Dr. Bence Jones has suggested the following simple and ready trial, when chemical materials happen not to be at hand. Evaporate a drop of the suspected urine on a slip of glass over a water bath. When the evaporation of the water is completed, any albumen which may be present will adhere to the glass so firmly, that it will be no easy matter to clean it. The evaporation may be effected in a few seconds, in a watch glass, over a spirit lamp, care being taken to hinder the urine from boiling, by holding it far from the flame.

Now it is quite certain that the presence of albumen in the urine does often accompany and bespeak a very serious organic disease of the kidney. For this disease we have no appropriate name. I wish we had. Some call it *granular degeneration* of the kidney, but the epithet granular is not always applicable. It is most familiarly known, both here and abroad, as *Bright's kidney*, or *Bright's disease*; after the eminent physician who, in 1837, first described it, and showed its great pathological importance. These are odd-sounding and awkward terms; but in the lack of better I must employ them.

It is very difficult to describe, in words, the anatomical characters proper to this renal disease; for they are neither very definite nor very constant. I am speaking now of that coarse anatomy of the diseased organ, which is cognizable by the unassisted vision. The rough sketch that I am about to attempt will be made more intelligible by Dr. Bright's plates, and those of M. Rayer, which are both before you.

The morbid appearances presented by the substance of the kidney are such as denote some change in its intimate structure. Its cortical (or secreting) portion is the primary and chief seat of this degeneration; yet what is called its medullary (*i. e.*, its excreting) part, is also sometimes implicated, but in a less degree.

These morbid appearances relate to the size, figure, and consistence of the kidney; to the colour and condition of its surface, and of its interior. With respect to some of these points there is much variety in different cases; and studying this variety under the light which is thrown upon it by the clinical history of the disease, we have reason to believe that, in part, it is connected with different stages of the disorgani-

zing process. Partly also, and perhaps chiefly, it appears to be the result of essential differences in the nature of the intimate structural changes. Thus if we look to the size of the diseased organs, they are sometimes much larger than natural, sometimes of the ordinary magnitude, sometimes considerably smaller. The average weight of the adult human kidney is between four and five ounces. In this disease some have been met with weighing twelve ounces, others weighing scarcely two. Both the increment and the decrement of the natural bulk belong principally, if not altogether, to the outer secreting portion of the gland. If a longitudinal section of the exaggerated kidney be made, its cortical part is seen to be unduly broad: and the same part is disproportionately narrow when the whole organ is smaller than common. For this reason, in the latter case, the radiating medullary portions or pyramids approach nearer to the surface than they are observed to do in a healthy kidney. And it furthermore appears that the enlargement is most commonly coincident with the earlier, and the contraction or shrinking with the later, stages of the renal disease.

The consistence of the diseased gland is variable also. Sometimes, and for the most part I believe in the earlier periods, it is soft and flabby: sometimes, and especially in the later periods, it is remarkably compact and hard. The size and the consistence of the kidney are, in most cases, inversely proportional to each other.

Again, the *form* of the kidney, in the disease or diseases in question, often undergoes some modification. As the structural change proceeds, the exterior of the gland shows a tendency to become indented by linear depressions, and to present a lobular shape. This, however, is by no means a constant phenomenon, even in the most advanced stage of the malady.

The proper investing tunic of the kidney is generally stripped off with ease from the large and softer varieties; with difficulty from the contracted and harder. When it has been removed—and less distinctly *through* the same tunic, before its separation—the surface of the gland appears mottled, marbled, or stained; of a yellowish grey colour in one place, and of a dark or purple tint in another. Occasionally it is pale throughout its whole extent; more commonly of divers hues, and variegated with little streaks, which are portions of vessels containing red blood. Sometimes the surface is curiously speckled; often uneven, as if strewed with prominent grains; in some instances quite rough and scabrous; in others perfectly level and smooth. I may add, that when the surface is granular, the kidney very frequently is contracted and small; when smooth, it is usually larger than natural. These several unnatural appearances are generally the more conspicuous, in proportion as the complaint is the more advanced.

FIG. 167.



FIG. 168.



FIG. 167.—Granular degeneration, in its incipient stage.

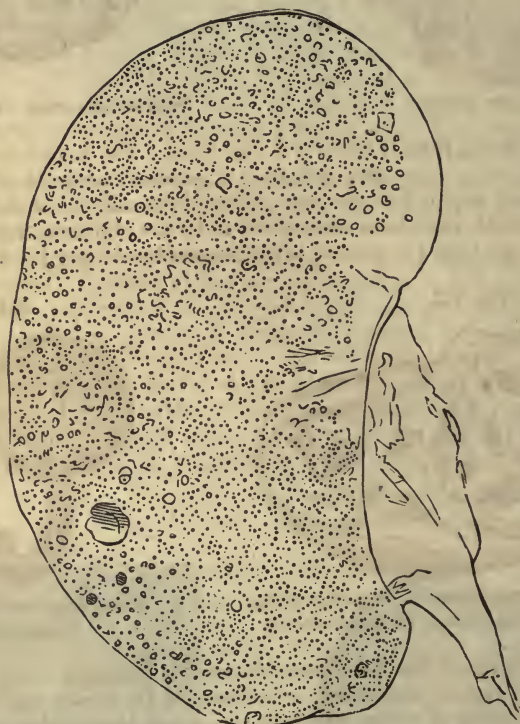
FIG. 168.—Cortical part of a very granular kidney, containing very numerous microscopic cysts. The tubes are very much degenerated and broken up. Two Malpighian bodies are shown.

The most uniform, however, and the most characteristic of the morbid appearances, are those presented by the cut surface of the kidney, when it has been divided into two symmetrical portions by a longitudinal incision. We then perceive that the cortical substance is the main seat of the morbid alteration. It has lost, in a greater or less degree, its proper red colour and uniform aspect. Sometimes it puts on a speckled or granular appearance; but this, in my experience, is less common than a

pale, nearly homogeneous surface, somewhat like the section of a parsnep. Its natural striae are confused or obliterated. The incised surface gives one the notion of some deposit, whereby the original texture of the part is obscured. The blood-vessels seem, many or most of them, to have been emptied by compression, or to be blocked up by yellowish solid matters; while the healthier pyramidal masses belonging to the medullary portion of the kidney are displaced, and pushed aside, or encroached upon by the same yellowish matter, which sometimes interposes itself between, and opens out, their radiating tubuli. Together with these changes of appearance and structure, I have several times found the veins that emerge from the kidney firmly plugged up by coagula of blood.

In some rare cases the kidney is studded, both on its surface and throughout its interior, with numerous small cysts or cells, containing a thin transparent liquid. These cysts have been inaccurately termed hydatids. It is not at all uncommon to meet with one or two larger cysts of the same kind in this diseased state of the organ.

FIG. 169.

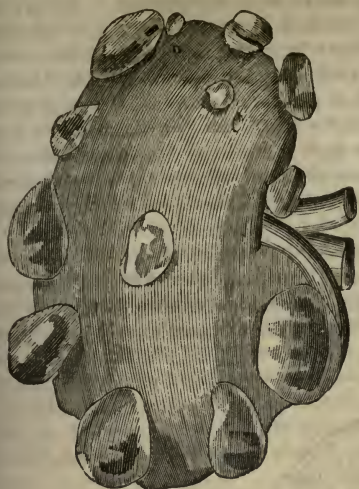


Serous cysts of the kidney. From a preparation in Dr. Gross' cabinet.

It has been made a question whether the various appearances which I have been attempting to describe, and which sensibly differ in degree and combination in different cases, are characteristic of different morbid conditions, or merely of different stages and varieties of the same essential change. Our knowledge of the subject is scarcely sufficient to supply a positive solution of this question. The marked physical differences, obvious to the naked eye, suggest however the strong probability of two or more kinds of degeneration of structure: and this *primâ facie* inference is strengthened, as I will presently explain, by the later revelations of the microscope.

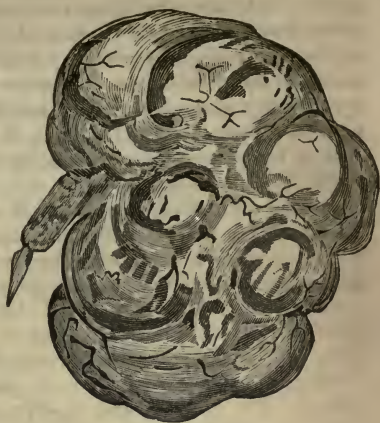
There is still another state of the kidney, very different to the eye from any that I have yet mentioned, but which has been thought, and which I think, to be, in some cases at least, the first stage of all in the disorganizing process. This state, which I referred to when speaking of suppression of urine, may be briefly described in two words—*sanguine congestion*. The whole organ is gorged with blood, which

FIG. 170.



Cysts of the kidney.

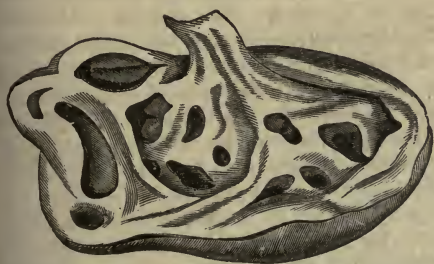
FIG. 171.



Cysts of the kidney. External appearance.

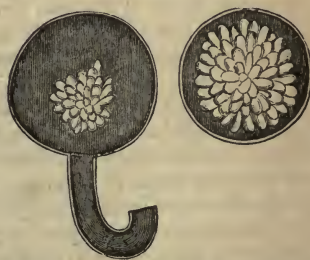
sometimes drips freely from it when it is cut open. The kidney is in general large, somewhat flabby, of a deep dark red, even of a chocolate or purplish colour, nearly uniformly diffused, except that the cut surface is usually diversified by still darker tuft-like spots, which have been ascertained to be the Malpighian bodies, turgid with blood. This change from the natural appearance of the kidney is evidently of a recent

FIG. 172.



Internal structure of the cystic degeneration of the kidney. From a preparation in Dr. Gross' cabinet.

FIG. 173.



Hemorrhage into Malpighian capsules compressing the tufts.

kind; and the symptoms that have been observed to belong to it are these:—Fever, preceded often by rigors; uneasiness or dull pain in the loins; nausea and vomiting; a very scanty secretion of urine, which is sometimes tinged with blood, and always albuminous; occasionally complete suppression of urine. To these symptoms there is presently added, in most cases, sudden and general anasarca—what is commonly called inflammatory, active, or febrile dropsy. If the secretion of urine be entirely suspended, death soon ensues by coma, as I explained to you yesterday; but if not, the disorder frequently proves fatal through the supervention of some acute internal inflammation; pleurisy, or pericarditis, or pneumonia, or peritonitis. Many persons recover completely from the condition expressed by this combination of phenomena. Many seem to recover, but bear about with them the germs or beginnings of those more chronic and latent changes which constitute one form at least of “Bright’s kidney.”

And what are the signs which indicate, to an instructed eye, the presence of those changes? Some of them are precisely the same, in kind, as those which denote the acuter disorder; only mitigated in degree, and of slower march and succession. The patients are subject to obscure lumbar pains, though these seem rare; to sickness

from time to time, and retching; and their urine is apt to be red, brown, or dingy, as well as albuminous, from the intermixture of some of the colouring matter of the blood. They are obnoxious to inflammations of the serous membranes also; and more particularly to head affections, of which, often, they die; drowsiness, convulsions, apoplexy. And, to finish the resemblance, many of them, aye most of them, become sooner or later anasarca. Besides these symptoms there are others which are not seen in the acute and recent malady; because it is recent. Gradually increasing pallor is almost constant; disease of the heart is common; and the skin, in general, even in the absence of fever, is remarkably dry and unperspiring. The patients are troubled by a frequent want to make water, especially at night, when they are in the horizontal posture; by flatulence of the stomach and intestines; and by caprice of the bowels, which are sometimes obstinately costive, sometimes prone to diarrhœa.

Now it is worth your while to remark, with respect to this category of symptoms, that (nocturnal micturition and the state of the urine excepted) they have no special *primâ facie* reference to renal disease. They are all common enough in various other complaints. In truth they are mere secondary consequences of Bright's disease; and in so far as they are symptoms of it, they are *indirect* symptoms. Before Dr. Bright, no one perceived, in such symptoms, any indications of disease of the kidney. The primary and fundamental organic malady reveals itself by no direct signals excepting those which are furnished by the urine.

Seeing then, that this structural disease of the kidney is coupled with effects so grave and perilous, and seeing that one of its most positive and distinctive marks is an albuminous state of the urine, two questions of great interest at once present themselves.

1. Does albuminous urine *always* imply the presence of Bright's disease?
2. Is Bright's disease, when present, *always* accompanied by albuminous urine?

To both these questions the answer is — no.

I believe that some articles of food, and some medicines, have the effect, in some persons, of rendering the urine for a time albuminous: perhaps it would be more correct to say that certain forms of indigestion may cause this change. Albumen has also been detected in the urine after a blister upon the skin, or under that general state of irritation of the surface, called *eczema rubrum*, which is produced occasionally by mercury. In the crisis of some febrile disorders, in some cases of pregnancy, of heart disease, and of delirium tremens, and in epidemic cholera, the same phenomenon has been observed. Whenever blood, proceeding from any part of the long tract of mucous membrane which lines the urinary organs, mingles with the urine, that fluid of necessity contains albumen, and coagulates if tested by heat or by nitric acid.

On the other hand, when the kidney is really affected in the way already described, the admixture of albumen with the urine is apt to disappear, for a while, even suddenly. I have known it vanish for several hours, immediately after the effectual application of a hot air-bath; and after profuse purging by a full dose of elaterium. Sometimes it is absent for a longer period.

Another important question, therefore, now arises. Finding albumen in the urine, how are we to know whether it does, or does not, indicate the presence of Bright's kidney?

We may judge, in part, by frequently testing the urine, and noticing whether the albuminous impregnation be transitory or persistent. If, week after week, it remain steadily present, it is almost surely indicative of that renal disease. *Almost surely* I say, because it is held by M. Rayer, and though not improbable by Dr. Owen Rees, that uric acid crystals, occurring in the urine of gouty persons, may sometimes, by irritating the urinary tubules, give rise to enduring albuminuria, when there is no degeneration of the kidney. Certain it is that in a very few cases, albuminuria goes on even for years without any serious inconvenience to the patient, or much visible impairment of his general health. If M. Rayer's pathological doctrine be true at all, it probably is true in these cases of abiding albuminuria; and in such cases the albumen will readily disappear under alkaline treatment, which forms a test of their nature. Partly, again, we form our judgment by the absolute amount of the albumen in a given measure of urine. If the water be deeply charged with that unnatural ingredient, the presumption is strong that the kidney disease is in progress; and when that disease is confirmed, another remarkable change is found to have taken place in

the urine. Its specific gravity is very low; and strikingly in contrast with that of diabetic urine. This is therefore a very cogent additional diagnostic circumstance.

On Dr. Prout's authority we have assumed the specific gravity of healthy urine to range between 1015 and 1025. Other writers make it higher. But the urine voided in Bright's disease is sometimes as low as 1004; and its mean specific gravity does not exceed 1013.

I need scarcely again remind you, that the question of specific gravity must always be viewed in relation to the absolute quantity of urine secreted. The specific gravity depends, of course, upon the proportion of the solid constituents of the urine contained in a given quantity. If the aqueous portion be augmented, the effect upon the absolute density will be the same as if the solid contents were proportionally diminished. But when, as frequently happens in certain stages of this renal disease, the specific gravity decreases while the quantity of the urine decreases also, that conjunction of phenomena becomes especially significant.

The density of the urine being thus unnaturally low, notwithstanding the addition of the new substance, albumen, it follows, as a matter of inference, that the solid constituents proper to healthy urine must be sensibly diminished: and they are found, in fact, to be so. These solid ingredients consist mainly of urea, and of certain salts. The aggregate solid contents amount, in health, to sixty-seven or sixty-eight parts in every 1000. In Bright's disease the quantity has been ascertained to have sunk to twelve or fourteen parts; and even, in an extreme case, to less than this — to about six parts.

The urine contains, then, albumen; and it is deficient in urea. These two facts suggested, naturally enough, to M. Solon, and to others, the notion that the albumen might be formed, by a sort of conversion, at the expense of the urea; since these substances, by a slight alteration in the ratio of their elements, pass respectively each into the other. But it is not so. Dr. Christison had observed many years ago, that when the urine was deprived of the greater part of its urea, the quantity of albumen contained in it was small; and, on the other hand, in cases where the urea was considerable in quantity, the albumen was plentiful also. In a more recent work on this subject, the same physician states that the whole of his subsequent experience has been in conformity with this observation.

It being certain, therefore, that the albumen is not vicarious of the urea, what (you may ask) becomes of the urea? It is detained in the blood; and may readily be recognized there in considerable quantity: and herein lies, as I conceive, the secret of the secondary affections which belong to this disorder, and of its great fatality. The body is poisoned in detail by the retention of its own excrements. The blood not being duly purified through that great emunctory, the kidneys, is spoiled for its purpose of nutrition. Besides containing urea, it undergoes other and more manifest changes. Its proportion of fibrin varies; and it gradually becomes poor in colouring matter; the serum is less albuminous also, and of a lower specific gravity than in health. The quantity of albumen in healthy blood averages from sixty-five to sixty-nine parts in 1000. In this malady Dr. Babington has found it reduced to sixteen parts. The average specific gravity of healthy serum is 1028; but in Bright's disease it descends to 1024, 1020, and even to 1013. Now Dr. Christison has made out the very interesting fact, that there is a definite inverse ratio between the coagulability of the urine, and the density of the serum. The more albumen there is in the former of these fluids, the less is there in the latter, and the lower is its specific gravity. So that the deficiencies of the one fluid balance the superfluities of the other. All this is very different from what takes place in diabetes, in which sugar is excreted with urine that is otherwise healthy; whereas, in Bright's disease, urea, which ought to be discharged, remains in the blood; and albumen, which ought not to be separated, is taken from the blood and carried out with the urine.

I have now described the changes exhibited to the naked eye by the kidneys in this disorder, the symptoms which attend it, and the morbid conditions both of the urine and of the blood. But these all vary and fluctuate at different periods of the complaint. I must next, therefore, endeavour to state what has been ascertained of its course and progress.

When the chronic disorder is not a legacy left by the more severe and acute form of disease which I have termed febrile dropsy, it is apt to creep on very insidiously,

and to escape our notice : and its history is not yet fully known. It will be enough if I distinguish two stages of the malady—the early, and the advanced.

In the early stage the urine is generally scanty. Instead of about 40 ounces in the twenty-four hours, the patient voids 16, 12, 8, or even so little as 2 or 3 ounces. Sometimes the secretion is nearly or quite suppressed : and then the head seldom fails to become affected in the way already described. The urine has also an unnatural appearance. It is red, or dark, obscurely turbid, like muddy beer. Smoky, is an epithet frequently applied to it : that is, it looks, sometimes, as if a very minute quantity of soot had given it a tinge. The hue depends upon the presence of a little of the colouring matter of the blood, darkened by the acid properties of the urine. It froths also more than is usual. If you blow into it through a tube, you raise bubbles similar to those which may be formed on soapy water ; and the bubbles remain long unbroken. Its specific gravity is somewhat, yet not greatly reduced ; about 1021, perhaps ; it is seldom at this period so low as 1016. It contains an abundance of albumen. Very rarely does it deposit the lithates.

FIG. 174.



Drawing of red deposit from urine in intense renal hyperæmia.

At the same early period, blood drawn from the arm exhibits the buffy coat. The serum is much diminished in density, and contains a considerable quantity of urea. There is no decrease in the fibrin ; perhaps it is a little augmented : and there is no great change in the amount of colouring matter.

In the more advanced stages of the disease, the quantity of urine is frequently not below the standard of health ; and it sometimes considerably exceeds that standard, so as to constitute one variety of chronic diuresis (*anazoturia*), which some call diabetes insipidus. It is usually pale, clear or slightly opaque, and of a very low specific gravity ; 1014, 1010, 1007. Once, when the quantity of the urine was *not* in excess, Dr. Christison found the specific gravity to be no more than 1004. There is a corresponding reduction in the natural solid ingredients of the urine. Albumen, too, is present, but more uncertainly than in the early periods : fluctuations in this respect are more common than before. It is a mistake to suppose that the amount of albumen increases as the disorder advances. The contrary rule would be more near the truth. In general the albumen is plentiful and almost constant in the outset of the malady ; less surely present as it proceeds ; and sometimes entirely absent in its latter periods : and it is of importance to remark that the alteration in the specific gravity follows the opposite law. The declension of density, so far from being corrected, augments with the progress of the disorder. Hence the one of these morbid phenomena is a valuable check upon the other, considered as an index of what is going on in the kidney.

And another fact, which it is essential for you to know and to remember, is, that, in any stage of the disease, the supervention of febrile disturbance, from local inflammation or whatever other cause, tends to renew, for the time, those qualities of the urine which belong to the early period.

Meanwhile, the disease advancing, the serum of the blood recovers more or less its lost specific gravity, in proportion to the decrease of albumen in the urine. The quantity of fibrin seems, in some cases, to diminish. But the striking and most characteristic change is the rapid disappearance of the colouring matter, the hæmoglobin, as it is called. This may at length be so much reduced, as to form less than a third of the healthy average. If venæsection be occasionally employed, this process of depravation is accelerated; but it takes place whether blood be artificially withdrawn from the system or not. "I am acquainted (says Dr. Christison) with no natural disease, at least of a chronic nature, which so closely approaches hæmorrhage in its power of impoverishing the red particles of the blood." Hence the peculiar pallid or dingy hue of the patient's skin; the leuco-phlegmatic and even waxy aspect which invariably stamps the victims of this complaint.

These characters, then, of the urine and of the blood, when rightly compared and interpreted, reveal not only the existence of the renal disease, but also, with much probability, the stage or degree that it has reached.

Let us next review, a little more in detail, those secondary affections which I have already pointed out as being incidental to the subjects of this renal malady. They are of much consequence: for, in the course of the disease, more or fewer of them are almost sure to occur; most of them are productive of very serious distress: and some of them place the patient's life in immediate jeopardy, and often bring it to a premature end. Moreover, it is by these secondary affections that our suspicion of the primary disease upon which they depend is, in general, first awakened: and it is to the prevention or the removal of these same secondary affections that our curative endeavours must chiefly be directed.

The most common, and practically the most important, of them all, is anasarca; but of this, though I mention it first, I shall postpone, for a while, the further consideration.

Another very common, and very important secondary complication, is the occurrence of what we compendiously called *head-symptoms*: various manifestations of derangement in the cerebral functions: headache, misty vision, noises in the ears, drowsiness, delirium, epileptic seizures, apoplexy. So frequently indeed is the death of the patient preceded by coma, with or without convulsions, that Dr. Christison considers this to be the "natural termination" of the disease, or "the mode in which it proves fatal when life is not cut short by some other incidental or secondary affection." Of seventy fatal cases observed by Dr. Bright, death was ushered in by well-marked cerebral symptoms in thirty.

I have already told you the circumstances under which these affections of the brain usually arise. They almost always follow any great and sudden diminution, or the entire suspension, of the secretion of urine. But this rule is not so strict as to admit of no exception. Occasionally, but I believe very seldom, the urine, in this disorder, is reduced to a very small amount, while the head remains undisturbed. Of this Dr. Christison has recorded a remarkable instance. One of his patients voided no more than two ounces of light urine daily, for nine days before his death; yet he continued sensible to the very last minute of his existence, and died simply of inanition. Sometimes apoplectic symptoms occur, and carry the patient off, although there has been no extreme or material reduction in the quantity of urine.

If the secretion of urine become again abundant, these head-symptoms are mitigated or cease.

Now when death has thus taken place in the way of coma, and the case had been complicated with anasarca, and serous liquid is found accumulated in unnatural measure in the cerebral ventricles, or in the tissue of the pia mater, it seems reasonable to ascribe the coma to the presence and the pressure of that liquid. The dropsy has extended to the brain. And this view of the matter is strengthened by the connexion which may sometimes be noticed between the accession of coma and the visible increase of the dropsy in other parts of the body. My own experience accords entirely with that of Dr. Christison, as expressed in the following statement. "If the dropsical fluid be allowed greatly to accumulate, drowsiness, the first symptom of the affection of the head, very soon makes its appearance in the generality of cases, and it will speedily pass into fatal coma if not controlled, but the removal of the dropsy will usually remove the drowsiness."

To many cases, however, this explanation will not apply, there being no morbid collection of water within the skull, nor any other appreciable change there; nor, perhaps, any dropsy elsewhere. In such cases the ultimate symptoms, the stupor and the death, used to be ascribed to the poisonous influence of the urea in the unpurified blood, upon the organs of animal life. Yet this explanation had its difficulties. Urea must often circulate with the blood without affecting the brain. Dr. Christison states that he has repeatedly known the daily discharge of the solids of the urine to be reduced, for weeks together, to one-fourth of the natural amount, while, moreover, analysis of the blood showed that it was loaded with urea, without the appearance of any head-symptom. Dr. Bright also relates a case to the same purpose. A person labouring under this disease of the kidney lived for four or five years under his occasional observation. The blood was analyzed in the earlier stage, and found to contain a large quantity of urea; as much as the urine itself contained. Yet this patient had no *fits* till towards the close of his life. Urea has even been injected in considerable quantity into the veins of living animals, without any other effect than an increased secretion of urine. Dr. Bence Jones believes that urea is "probably not much more poisonous than nitrate of potash."

The doctrine, therefore, of the dependance of the head-symptoms upon the retention of urea in the blood, *as urea*, has been abandoned by most pathologists, both in this country and abroad. Frerichs, a German physician, who has written an able "monograph" on Bright's disease, offers a new and plausible theory of these nervous complications. The symptoms in question result, he says, from the poisonous agency, not of the retained urea itself, but of the carbonate of ammonia, into which, while still in the blood-vessels, it is liable to be converted. He produced similar symptoms in animals by injecting a solution of carbonate of ammonia into their veins. So long as the peculiar symptoms continue, whether in the sick man, or in the animal subjected to experiment, the expired breath is tainted, he affirms, with an ammoniacal or urinous odour. Reddened litmus paper, held before the mouth or nostrils, is turned blue; and a glass rod, dipped in hydrochloric acid, and exposed to the issuing breath, is presently surrounded with a whitish cloud. When no ammonia can thus be detected in the breath, the nervous symptoms cease.

I have sometimes fancied that the pale and watery condition to which the blood is at last reduced, may have something to do with the stupor and coma. I showed you some time ago, when speaking of spurious hydrocephalus, that similar symptoms are apt to ensue, in conjunction with a similar defect of hematosin. It would seem that, under such circumstances, the functions of the brain are exercised irregularly, languidly, and at length not at all, in consequence of the failing supply of its appropriate stimulus through the arteries.

Another striking circumstance observable in this disease, is a readiness of various organs of the body to inflame, and particularly of the serous and the mucous membranes. According to M. Solon, who has published a thick volume on *Albuminurie*, this disposition has not been so manifest in France; but of its frequent appearance in this country I can add my own testimony to that of Dr. Bright, of Dr. Christison, and of Dr. Gregory. Such intercurrent acute inflammation is not an uncommon cause of the patient's death. The pleura appears to be much more often affected in this manner than either the peritoneum or the pericardium.

Bronchial irritation is exceedingly common. Dr. Wilks, drawing his conclusions from a very extensive observation of cases, remarks of bronchitis, "I am not sure, if all the symptoms were numerically taken, this would not be more universal than any other single symptom, albuminous urine alone excepted."

Disorder of the stomach and bowels is another frequent companion of the renal malady: nausea, vomiting, flatulent distension, diarrhoea. Frerichs observes that in the later stages of the disease, the matters vomited — and the perspiration, when any occurs, which is seldom — are ammoniacal.

It is a plausible explanation, therefore, of these affections of the mucous surfaces, that they are excited by the poisonous material retained in the blood, and seeking a vent through supplementary channels of excretion.

From this tendency, it follows that when we come to inspect the dead body, we seldom find the kidney to be the only part in which structural changes are plainly visible. Most commonly evident traces of disease are met with in various organs.

It would appear, however, that these incidental and secondary complications prevail with irregular frequency in different places. They are probably determined, in some measure, by local and peculiar agencies. Thus vomiting and diarrhoea have been more familiar to the Edinburgh observers, than in London to Dr. Bright, or in Paris to M. Solon: while the headaches and coma, so often witnessed by the British physicians, have been comparatively uncommon in France.

Disease of the heart, if not a secondary consequence, is a very frequent accompaniment of Bright's kidney. It is possible that the cardiac disease, and the renal disease, have sometimes no connexion in respect of cause and effect, but are both results of some common cause; of habitual intemperance, for example.

I am, however, of opinion, that the renal malady has a direct tendency, by its effect upon the blood, to generate disease of the heart. It induces anæmia: and anæmia, as I showed you on a former occasion, implies debility of the muscular texture of the heart, and leads to dilatation of its cavities; and the weak muscle, becoming irritable also, grows thicker as it labours more. But there is another probable cause in operation. Dr. Bright originally suggested that the altered quality of the blood might "so affect the minute and capillary circulation as to render greater action necessary to force the blood through the distant subdivisions of the muscular system." This view of the matter is fully adopted by Dr. Johnson, who points out the probability of "impeded circulation through the systemic capillaries, consequent upon the retention of the urinary constituents in the blood, similar to that which Dr. Reid detected by the hæmadynamometer when black unaërated blood was circulating through the arteries and the systemic capillaries of animals" dying of strangulation. I remember hearing Mr. Paget say that in cases of dropsy from renal disease, attended with hypertrophy of the left ventricle of the heart, he had found *all* the arteries enlarged without being thickened—his attention having been first drawn to that fact by his having noticed that the lower part of the aorta and the common iliacs were large in such cases, and that the external iliacs especially were large and contorted: whereas he had not seen this condition of the arteries associated with hypertrophy of the left ventricle dependent on disease of the aortic valves. Now it is this kind of cardiac disease which, more than any other, has been found coincident with the peculiar changes in the kidney. Among 100 cases, recorded in a tabular form by Dr. Bright, there were 27 in which no affection of the heart could be detected. In 52 instances the heart presented the characters of hypertrophy, and of those no fewer than 34 were free from any trace of valvular disease. Among the 34 there were 11 cases of disease affecting the aorta: in the remaining 23 no cause for the existing hypertrophy and dilatation could be found in the heart itself, or in the great blood-vessels. The true cause may therefore be reasonably supposed to have been the renal disease, operating upon the involuntary muscle through the morbid qualities of the blood.

Dr. Kirkes has recently borne his testimony also to the frequent coincidence of disease in the coats of the aorta with Bright's kidney: and he attributes the arterial change to the excessive force with which the blood is driven into the aorta by the hypertrophied left ventricle, a force unchecked by any valvular disease. "The arterial blood (he observes) is under the continual influence of two unnaturally great causes of pressure, excessive ventricular impulse at one end, and impeded capillary current at the other; and it can scarcely be expected that the walls of the vessels should escape over-distension, and consequent liability to structural change."

Whether the renal disease be ever produced by the cardiac, is more questionable. In the acute renal affection, when it proves early fatal, the kidney is always found to be gorged with blood. And the customary intermixture of blood with the urine warrants the belief that the same condition was present to the patients who have recovered. From this state of engorgement springs, apparently, sometimes, the subsequent series of changes. It is therefore a plausible conjecture that whatever tends to produce congestion of the kidney, tends also to aggravate, and may even cause, the peculiar changes in question. I need not now tell you that disease of the heart does frequently occasion congestion of the venous system, and gorge the viscera with blood. Under this influence the liver often *enlarges*. On the other hand, disease of the heart, even such as gives rise to venous congestion and to dropsy, often lasts long, and proves ultimately fatal, without the occurrence of albuminous urine, and without any appreciable change of structure in the kidney.

Pain or tenderness of the loins, is sometimes, but in my experience rarely, an accompaniment of the renal disease. It occurred, however, in one-third of twenty-eight cases narrated by M. Solon, and Dr. Gregory noticed it in the half of his patients. This symptom appears to be more often present in the early than in the later stages of the malady.

The *causes* of the disease of which I have been endeavouring to sketch the outline, are often obscure. Its more obvious symptoms, in the chronic form of the malady, have been observed, in very many instances, to begin soon after the exposure of the body to wet and cold under unfavourable circumstances. But it is by no means certain—indeed the probabilities preponderate on the other side—that, in these instances, the renal disorder had not previously existed in a latent state.

It is certain, however, that the acute kidney affection, which may be considered identical with febrile dropsy, does often arise under similar circumstances of exposure, and is attended with a marked disturbance of the functions of the kidneys. And Bright's disease in its chronic form has been noticed as occurring in persons who have previously suffered, and had apparently recovered from, an attack of febrile dropsy. Are we not warranted in believing that the recovery was *imperfect* in such cases? that the kidney had sustained irretrievable injury? and that the disease, although from the treatment employed, or by lapse of time, it had become tranquil or latent, was ready again to give indications of its existence upon any repetition of its exciting cause?

Again, it is matter of common observation that intemperate habits have often preceded the development of this disease. Yet we may conclude that intemperance in drinking is rather a predisposing than an essential cause, from the fact that the malady is not unknown among children, and other persons whose manner of life has been strictly sober. I had lately an example of this in a young girl, fifteen years old, who had never menstruated. And this leads me to remark that the renal disorder has been known, in many instances, to follow a sudden check or suppression of the catamenia. It has sometimes seemed to owe its origin to blows received upon the loins.

The complaint happens at all ages: less often, however, in extreme youth than afterwards. Sabbatier records that he saw, while in the service of M. Baudeloque, a young infant affected with anasarca and albuminous urine. The first case described by M. Solon is that of an infant, seventeen months old, in whom similar symptoms appeared shortly after exposure to cold and wet. In 1838 a boy between five and six years old, anasarcaous, and passing bloody and albuminous urine, was in the Middlesex Hospital, under the charge of my colleague Dr. Wilson. M. Constant, in the *Gazette Médicale* for 1835, cites the case of a child five years of age: and M. Rayer gives two plates, representing the kidneys of two children, the one five and the other six years old, who both died of dropsy with albuminous urine, the sequel of scarlet fever. In each of these the changes described by Dr. Bright were well marked, and the bulk of the kidney was considerably increased.

The malady is, however, much more common in adults: not, in all probability, because the kidney is more readily susceptible of it at one period of life than another, but because, as life advances, the circumstances which tend to produce or to foster it become of more frequent operation; intemperance, exposure to great vicissitudes of temperature, and (perhaps) disease of the heart.

It occurs, I presume for the same reasons, oftener in men than in women.

Dr. Christison suspects that Bright's kidney happens chiefly in persons of scrofulous habit; and he found it, in several instances, coincident with phthisis pulmonalis. My own experience would not have led me to that opinion. I partake in M. Solon's doubts, whether the co-existence of pulmonary consumption and of this renal malady is more than casual. Dr. Bright tells us that "the instances in which phthisis, or any form of scrofulous or tubercular disease, has been connected with the renal affection, have been decidedly rare."

What, after all, is the true character and essence of the organic metamorphosis which constitutes this formidable disorder, Bright's kidney? Since I last spoke upon this subject our knowledge has made several steps in advance. On former occasions I have expressed to you my belief that some of the grosser changes of structure ascertained by the ordinary mode of inspection, were ultimately owing to an undue accumu-

lation of blood in the renal vessels; to congestion—passive or inflammatory. And to this conclusion, which observation and reasoning had suggested, direct experiment lends strength. In this way it has been shown, by Dr. George Robinson, that a mechanical impediment to the venous circulation in the kidney, whether it be partial or absolute, whether rapidly effected or slowly, is sufficient to produce albuminous and even bloody urine. Rayet calls the complaint *albuminous nephritis*; and perhaps the congestion (which unquestionably is present in what I consider the acute form of the malady) may sometimes pass into chronic inflammation. We do not, however, find that it ever terminates in decided abscess: yet suppuration of that kind is no uncommon event of true inflammation of that part, excited by violent injuries, or by the lodgment of calculi within it. It seemed to me more probable that, in one form at least of the disorder, the mischief done to the kidney was the result of extreme congestion, and its usual consequences—the oozing forth of the blood in substance, or of some of its constituents, into the interstitial textures, as well as into the excretory tubes of the kidney. The appearance of these ingredients of the blood, and even sometimes of blood itself, in the urine; the increased size of the gland in the earlier stages; the various shades of colour which its surface and parts of its interior present, as the colouring matters of the effused fluids are more or less absorbed; the impermeability of those altered parts by artificial injections; the shrinking (in many cases) and hardness of the organ as the disorder becomes chronic, and absorption proceeds; these are all consistent with this theory. The morbid conditions of the urine depend, in part, upon the mechanical transudation of certain portions of the blood, which pass through the kidney unchanged, as through an inert filter. Mixed with the urine we find serum, with its albumen, and its salts, which diminish the acidity of the mixture, or even render it neutral; and in many cases we find more or less of the colouring matter also of the blood. Those portions of the extravasated fluid which have no outlet of escape, solidify, and thus obliterate the natural texture of the part they have invaded. The obstruction of the emergent veins of the kidney by firm clots of blood is in harmony with the same supposition.

But more has been learned than this. New light has been thrown, of late years, upon the doubtful pathology of this important and complex renal affection, by that minute scrutiny of texture and of textural changes, as well as of conditions of the urine, which the microscope perfects or facilitates. Yet I have not the satisfaction of being able to tell you that the interesting problem has been finally solved. Though 30 years have passed away since Dr. Bright announced the dependence of certain forms of dropsy upon structural disease of the kidneys, it still remains a disputed point among pathologists whether “Bright’s kidney” is one disease, assuming different

FIG. 175.

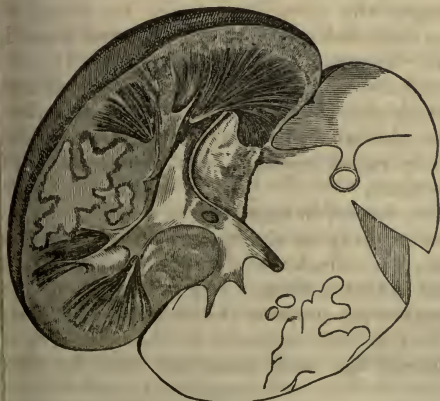


FIG. 176.

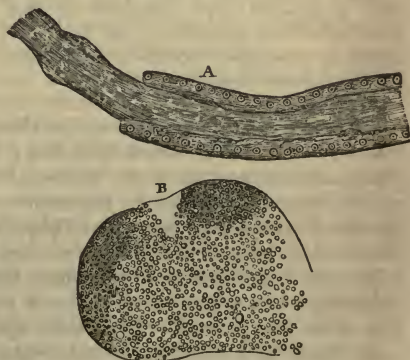
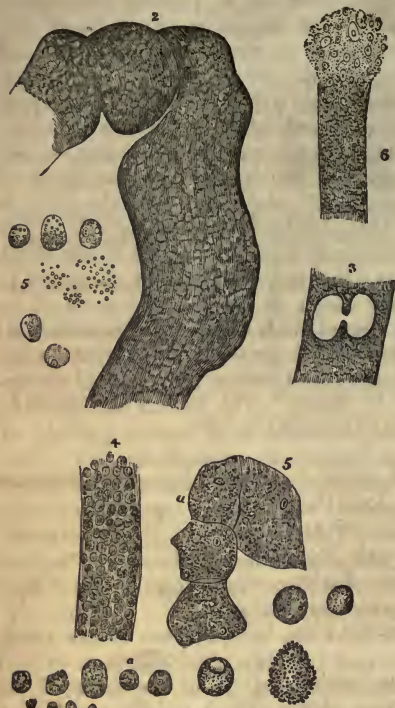


FIG. 175.—Fibrinous deposits in a granular kidney. The situation of the patch is marked by the irregular outline, which was a deep red.

FIG. 176.—(A) Tube containing an homogeneous cast, which projects from its broken end. (B) Malpighian body; the capsule is filled with oily matter.

aspects in different cases and circumstances, or whether there are not two, or even more, distinct renal diseases which, giving rise to similar symptoms, have been comprehended and confounded under one common name.

FIG. 177.



(2) Cortical tube, infarcted with epithelium, and bulged in a good part of its extent. (3) Cortical tube containing a dumb bell crystal of large size. (4) Cortical tube, infarcted by epithelium at (a) below it; some of the separate particles are shown more highly magnified. (5) Bulky epithelium from cortical tube; the group at (a) are remarkably enlarged, those below them are more or less fatty. (6) Medullary tube much infarcted; the contents are seen escaping from the upper end.

their condition, when life is not destroyed. And I must tell you that death in this form or stage of the renal disease is a comparatively rare event. In 20 instances only out of 292 cases observed or collected by Frerichs, were the kidneys in this first stage—the stage of inflammatory congestion. This renal derangement is often completely recovered from: but doubtless it also forms, in many instances, the starting-point of the more chronic phases of Bright's kidney. Frerichs, Reinhardt, and other German writers hold that it is so in *all* cases; that all the renal changes to which that name is applicable, are only successive stages of one morbid process. Calling this the first, they describe a second and a third stage.

In the second of these alleged stages, the kidney is still much larger and heavier than is natural, and smooth on its surface; but the sanguine congestion is diminished, or gone; while the inflammatory exudation into and among the proper tissues of the gland is great and manifest. This exudation, by its presence and its pressure within and around the tubules, empties the capillary vessels of their blood, and keeps them empty. The cortical portion of the kidney, still unduly broad, loses its red colour, becomes pale or yellowish, and contrasts more strongly with the red lines of the pyra-

All agree in opinion about that condition of the kidney which has been ascertained to exist in what is called inflammatory dropsy; and in the dropsy which not unfrequently occurs during convalescence from scarlet fever. That condition I have already described. The kidney is large, soft, red, and bloody. But something more requires to be said respecting the phenomena presented by the urine in such cases. Not only is it always albuminous; and often dingy, in colour, and sometimes even reddish, from an admixture of blood; but it is found, when examined through a microscope, to contain a number of little hair-like threads or cylinders, which are, in fact, very slender fibrinous coagula, moulded in and discharged from the urinary tubules of the kidney. These cylinders or casts denote, even more distinctly than any amount of albumen in the urine denotes, extreme congestion, which may well be deemed inflammatory, in the affected kidney. They are commonly studded with minute epithelial cells, which have been detached from the surface of the tubules. Sometimes they contain blood disks also, and even specks of lithic or oxalic acid. By the help of the microscope similar casts may be seen still remaining in many of the tubules. Other tubules appear to be filled with epithelial cells which have been shed from their walls. And similar cells are scattered separately through the urine.

These are the anatomical characters of what Dr. Johnson calls *acute desquamative nephritis*.

Such, then, is found to be the condition of the kidneys in fatal cases of this kind; and such we may safely infer from observation of the symptoms and of the urine, to be

FIG. 178.



FIG. 179.



FIG. 178.—(A) Cortical tubes, containing a very fatty epithelium. (B) A short homogeneous cast, containing two corpuscles. (C) Portion of a medullary tube, containing three casts, looking much like cysts and oily matter.

The figures from 176 to 179 inclusive, are intended to illustrate the changes observed in the enlarged form of degenerated kidney.

FIG. 179.—Microscopic view of epithelium-cells and fibrinous shreds from the tubuli uriniferi of a kidney affected with Bright's disease. (1) Epithelium-cells from the tubuli uriniferi, loaded with oil-globules, magnified 400 diameters. (2) Fibrinous shreds from their interior, having blood-corpuscles and oil-globules entangled in them, magnified 200 diameters. (3) One of the tubuli from a kidney affected with Bright's disease. Oil-globules are seen through its walls.

midal bodies. Gradually the matters exuded suffer further change, and sometimes undergo a fatty degeneration. The tubules lose their uniform cylindrical shape, and bulge a little here and there: their epithelial cells enlarge, become opaque, contain granular matter, and perhaps oil globules: finally, they crumble down, and are partly washed away with the aqueous portion of the urine which proceeds from the Malpighian capsules.

That this is a faithful picture, drawn from nature, I make no question. That it represents a further stage of the condition previously spoken of, as the effect of inflammatory congestion, is less certain, though very probable. That it is a transition stage, towards what I shall presently describe as the third stage of the German writers, is in my judgment more than doubtful. It seems to be identical with the result of what Dr. Johnson calls chronic non-desquamative nephritis. In the common parlance of English physicians it is *the large white kidney*.

This state is much more often met with in the dead body than the former. It characterized 139 of the 292 cases of diseased kidney already mentioned.

The third stage is the stage of absorption. The kidney is contracted and small. Its surface is irregular, rough, knobby; its substance firm and tough. Its capsule is detached with difficulty. The cortical substance is unduly narrow. Some of the tubules, from which the casts and epithelial cells have been washed away, are collapsed and atrophied. Those which remain full, project; and give a granular aspect to the outside of the gland as well as to its cut surface. This is the issue, in Dr. Johnson's classification, of chronic desquamative nephritis. We speak of it familiarly as *the small contracted kidney*.

This also is a form of disease that is frequently seen. It constituted the remaining 133 of Frerichs' 292 cases.

Besides the fibrinous casts already described, in which epithelial cells are commonly entangled, the urine, in Bright's disease, not unfrequently contains tube casts of a whitish or waxy appearance, without any cells, and of comparatively large size; large, because moulded in tubes previously denuded of their epithelial lining. Sometimes similar wax-like casts of smaller diameter are met with, which have come from tubes in a state of wasting and contraction.

Now I shall not attempt to harmonize the fluctuating and conflicting opinions which have been expressed respecting the relations that may subsist between these

FIG. 180.



Epithelial cells in urine.

different appearances and conditions of the kidney. Authentic materials, sufficient for a satisfactory solution of the difficult questions in dispute, have not yet, I think, been collected. In this uncertainty, I can only set before you my own conjectures upon the probabilities of the matter.

It has been objected to the simple and plausible doctrine of the German authors

FIG. 181.

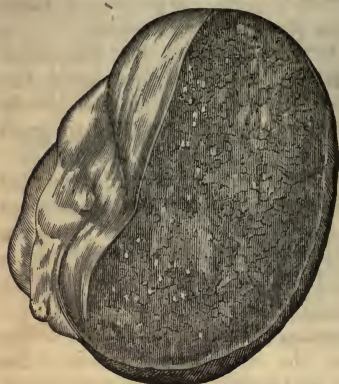


FIG. 181. — Drawing of atrophied kidney.

FIG. 182.



FIG. 182. — Cortical part of a very granular kidney, containing very numerous microscopic cysts. The tubes are very much degenerated and broken up. Two Malpighian bodies are shown.

whom I have cited, that regard being had to symptoms, their second and third stages are often met with when there has been no first stage: and again, that the third occurs without any preceding first or second. To the former part of this objection I attach no great weight: the latter I believe to have more validity. Extreme congestion, nay, inflammatory exudation, may rapidly befall the kidney, and yet escape detection, either by the patient or by his medical attendant, if he has one. Of the two things which would mark its presence most distinctly, one, namely, the sudden anasarcaous swelling, may be inconsiderable, transient, or perhaps wanting altogether; the other, namely the peculiar condition of the urine, may easily pass without notice, inquiry, or suspicion. Both may, I believe, come and go, without strongly arresting attention, or permanently impressing the memory.

I think it, then, probable, that the second and third stages of the German writers may, both of them, be truly stages or consequences of their first: not, however, successive stages, but each a *final* stage. The large white kidney I can conceive to be the result of so much inflammatory exudation,—blocking up or obliterating a great part of its texture—as could never be absorbed down to the size and appearance of the small contracted kidney. In the small contracted kidney I can conceive the stress of the inflammatory disturbance to have fallen mainly upon its free secreting surface, to have operated in spoiling the tubules, in destroying and discharging their epithelial lining, and in wasting the whole apparatus. The one seems to me to have some analogy with the lung which has been enlarged and rendered heavy and solid by hepatization in pneumonia:—the other with the shrunken lung, which has suffered more or less of collapse during the progress of chronic bronchitis.

Other forms of Bright's disease are spoken of, but I should only puzzle you and myself were I to dwell much upon them. There is the large, firm, *waxy* kidney, which appears to occur chiefly in persons of the scrofulous habit, and to owe its peculiar appearance to the presence of a lardaceous or bacon-like substance blocking up, more or less, and so spoiling, the cortical portion of the gland. I showed you formerly that the liver and the spleen are both of them subject to a similar change of texture. In the kidney the Malpighian tufts, and their small afferent arteries, are the first parts to be invaded. It is probable that this morbid condition arises gradually and insidiously—is chronic from the first—is not necessarily preceded by an acute stage of active or inflammatory congestion. If, in a patient presenting symptoms of Bright's disease, such as dropsy with albuminous urine, you find the liver large, smooth, and

painless under pressure, if there be any palpable tumour of the spleen, if there be any scrofulous caries of the bones, and, *a fortiori*, if two or all of these unnatural conditions be coexistent, you have presumptive evidence that the renal change is of the waxy, lardaceous, or albuminous kind : and the presumption will be strengthened if wax-like casts, large or small, of the urinary tubules, are voided with the urine.

Again there is the red *coarse* kidney, which, when not the result of mere mechanical congestion, is probably an earlier state only of the ordinary large white kidney. A similar account may be given, I fancy, of the *mottled* kidney. With any of these changes there may, or there may not, be an admixture of fatty degeneration.

It will be more to the purpose if I direct your attention to certain distinctions, of practical importance, between the two well marked forms so often mentioned, the large white kidney, and the small contracted kidney. In doing this, I shall chiefly follow Dr. Johnson, who has cultivated the whole subject of renal disease with great diligence and success.

It is observed, then, of the large white kidney, that "it never proves fatal without the previous occurrence of dropsy, which is one of its most usual and prominent symptoms ; while the small contracted kidney proceeds, in many instances, to its extreme limit of degeneration, and at length destroys its victim without giving rise to dropsy in any form or degree." "The first" (says Dr. Wilks) "invariably kills the patient ; the second may be found when death is brought about by other means."

Another remarkable difference between the two, explanatory, indeed, of the difference just stated in respect to dropsy, is that where the large kidney exists, "the urine is almost invariably less copious than in health, and contains a large amount of albumen ; whereas the hard contracted kidney furnishes, as a rule, a quantity of urine considerably above the natural standard, which urine also contains much less albumen." The blood is less robbed of its proper albumen, and therefore less impoverished, while its aqueous part is drained plentifully away. Both of these circumstances are adverse to the occurrence of dropsy. But in this form or condition of disease, any sudden check to the quantity of urine is apt to be followed by dropsical effusion, and speedy death.

In correspondence with these differences, it is found, as might be expected, that the specific gravity of the urine proceeding from the large white kidney is notably greater than that from the small contracted kidney. The former is seldom below 1015, and may be as high as 1025 or even 1030. The latter is commonly below 1015, and varies downwards to 1010, or even 1005.

By attending to these external symptoms you may generally decide, in a given case, with which of these two forms of renal degeneration you have to deal, and frame your prognosis accordingly.

When death has occurred, the kidneys present physical differences which help to explain the observed differences in the symptoms. "In that form of disease," says Dr. Johnson, "which leads to the small contracted kidney, the gland cells become disintegrated, detached from the basement membrane, and finally washed out with the urine. The tubes are thus either entirely denuded, or they are found to be lined by a layer of delicate cells, entirely different from the normal epithelium. It is probable that a tube in either of these conditions, while it has lost the power of secreting the solids of the urine, may yet retain that of separating the watery constituents from the blood," and thus may be explained the "abundant flow of urine, pale in colour, and low in density." In the large white kidney, "the gland-cells are never so detached, disintegrated, and swept away as to leave the tubes denuded. They remain adherent to the basement membrane, and undergo changes, varying from a slight granular opacity to a complete oily degeneration, or they become replaced by an albuminous or fibrinous material, which more or less fills the tube." This condition seems to him not so favourable as the former for the transudation of water.

Again, in the large kidney "the number of pervious blood-vessels, if not greater than in the healthy kidney, is rarely in any considerable degree less than normal. In the contracted kidney the opposite condition is found. As the disease advances, many of the uriniferous tubes shrink, and the vessels which supply them — both arteries and Malpighian capillaries — have their canals obstructed, and their walls covered with oil globules, so that the kidney is reduced to the condition of an organ

but scantily supplied with blood ; and obviously in the same proportion the materials for a copious secretion of *albumen* are wanting."

I have made no mention of simple fatty degeneration of the kidney, which I believe to be a real but a rare disorder, and which Dr. Johnson was the first to recognize and to describe. It does not, however, appear properly to belong to the class of cases known to us under the comprehensive title of Bright's disease. Dr. Johnson admits that it is sometimes met with, both in the human subject and in the lower animals—in cats and in dogs—unconnected with albuminous urine or with any other functional symptom of renal disease. It is nevertheless a very serious and formidable affection. Its pathological characters are these. The gland is large, but (except in accidental complications) it does not contain any inflammatory or albuminous exudation within its proper texture. In all the tubes of its cortical substance a large quantity of oil is to be seen, which for the most part is enclosed in the epithelial cells.

This change, too, has its analogue in the simple fatty degeneration of the liver. When it is accompanied by albuminous urine I suspect that it will always prove to have been complicated, to a greater or less extent, with one or other of those structural faults which are characteristic of Bright's kidney.

Respecting the origin of the minute vesicles and larger cysts which I mentioned before as being sometimes associated with the other renal changes, there is much discrepancy of opinion. Dr. Johnson believes that they are simply dilatations of the urinary tubules, after those tubules have been denuded of their epithelial lining. Mr. Simon ascribes them to an effusion of cell germs from broken tubules. Certain diseased states tend to block up these tubules : "the obstruction at length produces rupture of the limitary membrane, and then what should have been the intertubular cell-growth continues, with certain modifications, as a parenchymic development." Dr. W. T. Gairdner, again, regards these microscopic cysts as being parasitic, and entirely foreign to the structure of the kidney. He maintains that the attempts to connect them with morbid conditions of the tubuli uriniferi and epithelium, have been founded on imperfect observation. But for further detail on these points I must refer you to Dr. Johnson's book *on the Diseases of the Kidney* : to Mr. Simon's paper in the 30th volume of the *Medico-Chirurgical Transactions* : and to Dr. Gairdner's observations, published in the *Proceedings of the Edinburgh Physiological Society*, in the Session of 1852-53.

The single fact that in Bright's disease both the kidneys are always more or less affected, sufficiently demonstrates that the disorder falls primarily within the category of symmetrical, and therefore of blood disorders. You have seen how the organic changes which arise in the course of that disorder re-act in their turn upon the blood, and operate in further spoiling its natural constitution and properties. Whether the renal mischief results, as Dr. Johnson supposes, from the passage through the kidneys of some specific poison—the poison of scarlatina for example, the poison retained in the blood through suppressed perspiration, or the like—I do not know, and need not here discuss.

We are living in an age when organic chemistry, and microscopic research, are, severally and together, bringing vast and continual accessions to our knowledge both of the essential nature of diseases, and of their rational management. No better instance of this could be adduced than the progress which has recently been made towards unravelling the complex and intricate pathology of "Bright's kidney." That progress I have now endeavoured briefly to set forth ; but I must again remind you that towards the full solution of the problem certain steps only have yet been made. Much that I have now been telling you will, doubtless, require to be remodelled hereafter ; and not a little, probably, to be corrected.

There is yet something to be said respecting the *dropsy*, which is so common an accompaniment of these renal changes ; but I must defer it till to-morrow.

LECTURE LXXIX.

Anasarca ; its consideration resumed. Distinction of chronic General Dropsy into cardiac and renal. Characters and signs of each of these varieties. Treatment.

WE were yesterday occupied with the circumstances of that remarkable malady, which has never received a good, distinctive name, but which is sometimes called *Bright's disease*, after the distinguished living physician who first recognized and described it, sometimes yellow or mottled degeneration of the kidney, sometimes granular degeneration. I endeavoured to represent to you, by words and by drawings, the coarser changes of structure which are visible by the unaided eye in the several stages or forms of the diseased kidney; and I stated some of the results of that minuter insight into its morbid conditions which the microscope has brought within our power. I spoke of the symptoms which appear *essential* to the malady, and which consist in certain striking changes in the urine and in the blood of the patient. I mentioned also the symptoms which are *incidental* to the renal disease. But of one of those incidental symptoms, or secondary consequences, I postponed the full consideration till to-day: I mean the *anasarca*, with which most commonly, yet by no means always or necessarily, it is complicated.

This is, for several reasons, a very important symptom. It is usually the first thing that prompts us to suspect, and to inquire after, the renal malady. It was through his researches into the relation subsisting between chronic dropsy and the conditions of the urine, that Dr. Bright was led to the discovery of the associated affection of the kidney. The dropsical accumulation adds greatly to the patient's distress, and sometimes constitutes nearly the whole of it. It adds proportionally to his danger. Moreover, it is that consequence of the renal disorder over which our curative measures have the most control. Indeed, under this complication we have, practically, to consider the remedies of the dropsy, distinct from the remedies of the renal change.

In most cases, at the outset at least of the dropsy, the skin is dry, and the urine is scanty: and the anasarca is observed to increase, or to decrease, as the quantity of urine diminishes or augments. The aqueous fluid, which should escape from the surface and through the kidneys, collects in the subcutaneous areolar tissue. As the disorder advances, the tendency to effusion of serum through the sides of the blood-vessels is probably increased, not only by the sluggish movement of the blood in the veins, from progressive debility of the heart, but also by one of the causes of that debility, the thin and watery condition of the blood itself; a condition which I yesterday pointed out to you as one of the most uniform and striking effects of the primary disease.

And here I again take up the subject of anasarca and general dropsy. You will remember that, in the early part of the course, I entered somewhat fully into the general pathology of dropsies. At the same time I promised you that I would afterwards, and when you were better prepared to understand them, endeavour more fully to explain some grand distinctions which have been found to exist between different forms of general dropsy. Having now, at last, brought before you all the organic changes which are apt to give rise to anasarca, I am in a position to redeem that promise. In doing so I shall probably have to remind you of some things which you have already heard from me.

Anasarca, you will please to recollect, signifies the filling up of a considerable part, or of the whole, of the subcutaneous areolar tissue, with serous or watery fluid: and when to this is added a collection of liquid in the large serous cavities also, we call the complaint *general dropsy*.

It is obvious that this condition may exist, and in nature it does exist, in various degrees: from slight infiltration of the areolar tissue, scarcely noticeable until, after some hours passed in the upright posture, it accumulates in visible œdema about the ankles—to the other extreme, in which the integuments are everywhere stretched to the utmost, even to bursting; the insteps bulging upwards; the legs and thighs enormously enlarged, cylindrical, unshapely, and exhibiting partial vesications; the sur-

face of the trunk of the body capable of being kneaded and moulded like dough; the skin of the penis distended, and in consequence of its confinement by the frænum, twisted and circumvolved so as materially to impede the outward passage of the urine; the scrotum, as big as a child's head, preventing the miserable patient from bringing his thighs together, and from lying upon either side; the hands swollen; the face and neck bloated. With all this, the peritoneum is generally full of liquid, and at length the pleuræ; and as the scene is about to close, there is water in the ventricles of the brain, or an anasarcaous pia mater.

Now from whatever cause this watery condition of the whole body may arise, the effects resulting from the presence of the water are the same. And of what do patients in this state usually complain? Why of shortness of breath, and palpitation of the heart; of a sense of impending suffocation if they attempt to lie down, or to bestir themselves actively; of tightness and distress across the epigastrium, relieved somewhat by eructation, augmented by food and drink; of weight and stiffness of their limbs; and, sometimes, of drowsiness.

The explanation of all this is easy and obvious. The shortness of breath may be accounted for on various grounds: by œdema of the lungs themselves, a state that is revealed to us through auscultation; by water in the pleuræ; by the pressure upwards of the diaphragm, which embarrasses still more the labouring heart and lungs; and this upward pressure is increased by any kind of distension or repletion of the stomach, diminished when the stomach is collapsed and when the upright position is maintained. The heaviness and want of pliability of the unwieldy limbs are, like the rest of these phenomena, purely mechanical. All parts are oppressed by the unnatural load of water.

But we must look beyond the dropsy: and inquire whether the complaint has set in suddenly, and simultaneously with febrile disturbance, invading all the districts of the body at once, and quickly reaching its present degree; or whether it has crept upon the patient slowly and by stealthy steps: whether it has had any obvious or probable exciting cause: or whether it has approached insidiously, we know not whence or why: whether (in a word) the case be one of active and febrile dropsy, or of chronic and passive.

Now setting aside, for the present, any more particular consideration of the acute or febrile form of general dropsy, and contemplating those forms only which are chronic, we find that all, or nearly all of them, may be arranged in two great classes; those which depend upon disease or debility of the *heart* forming one class, those which depend upon disease of the *kidney* constituting the other. To these classes we accordingly apply the terms *cardiac dropsy*, and *renal dropsy*. They are often combined in the same individual; but taking the pure cases of each form, we may proceed to inquire into their peculiar features, how they may be distinguished, and what differences of treatment they may require.

And first of cardiac dropsy.

The mode in which disease of the heart may occasion general dropsy has been sufficiently explained already. We infer that the dropsy, in a given case, has this origin, if we find that thoracic symptoms, such as cough and dyspnoea, preceded the dropsy: or if we perceive direct signs of cardiac disease, such as distended jugular veins, irregular movements of the heart, unnatural impulse, altered sounds: or if we trace the history of some previous acute disease affecting especially the left side of the thorax: or if we learn that the patient has formerly suffered acute rheumatism: or if the patient's age be so much advanced as to make it probable that some of those organic changes in the heart and large blood-vessels are in progress, which are almost natural in the decline of life. And our inference is confirmed if there be no discoverable indication of renal disease.

But we see many persons who labour unequivocally under organic disease of the heart, yet who survive, even for many years, without becoming dropsical. The interesting question therefore arises, of what kind of heart-disease is dropsy a consequence and symptom? You already know the answer. It is such disease as offers a certain amount of permanent obstruction to the passage of the venous blood. Hence dropsy is especially associated with dilatation of the right chambers of the heart. It would not be correct to say that the anasarca is *dependent* on such dilatation, for the dilatation itself is at once an effect and a sign of impeded transmission of blood from

the right side of the organ. Nor is such dilatation a necessary attendant on the general accumulation of water. The impediment may be sufficient to gorge the right cavities, while it is yet to slight in amount, or too recent in duration, to have dilated them.

What, then, are the physical conditions which oppose to the blood in the veins such an impediment as we are now considering? The two great vital organs contained within the thorax, the heart namely and the lungs, form different parts of one common mechanism, the object of which is to supply every tissue of the body with blood that has recently been purified by exposure to the air: and these organs, thus closely related in their functions, are moreover so reciprocally dependent, that structural disease occurring in the one, tends to produce disease, sooner or later, in the other also.

And I wish you again to observe the order and direction in which disease is, almost always, propagated from one part of this apparatus to another. It is a backward direction—opposite, I mean, to the course of the blood. There are, strictly speaking, two hearts, which lie side by side, in respect of their anatomical position, but which, reference being made to their vascular intercommunication, are really separated from each other by the lungs. The great veins precede, the great arteries follow, this chain of connected organs. Disease occurring in any one part of the chain becomes a cause of consecutive disease in the part immediately behind it. And this law obtains, as I have shown you before, in regard to the several chambers of the heart, considered as a single organ.

Thus, structural disease situated at the aortic outlet of the heart, and of such a kind as to hinder the exit of the blood from the left ventricle, gives rise to permanent changes in that ventricle; to hypertrophy with, or less commonly, without, dilatation. The hypertrophy is strictly a compensating and conservative change; and when it is exactly proportioned to, and keeps pace with, the impediment which has given it birth, so as precisely to balance and counteract it, no delay takes place in the stream of arterial blood, and the injury is, as yet, confined to the left ventricle. That chamber is remodelled, and adapted to its purpose by the *vis medicatrix naturæ*; and no other evil manifests itself than, perhaps, some slight encroachment and pressure upon the neighbouring parts, in consequence of the augmented volume of the heart.

So long as the mitral valve remains healthy and effective, it offers a barrier of protection against the extension of the disease in the direction which is retrograde to the course of the blood. But at length, in most instances, the stress becomes sensible further back. The left auricle and the pulmonary veins become choked and distended; the blood is detained in the lungs. Then commences *dyspnœa*: at first occasional only, whenever the heart is tasked with the conveyance of a greater quantity of blood in a given time than usual, as in brisk movements of the body, or sudden emotions of the mind; or when it is oppressed by circumstances that diminish the capacity of the chest; by a full meal, therefore, by flatulent distension of the stomach and intestines, by the recumbent posture. Afterwards the shortness of breath becomes more or less constant and distressing.

Now this loaded and embarrassed state of the lungs, even when it is permanent and has reached a considerable degree, may exist without materially interfering with the functions of the right or venous heart; for the pulmonary plethora may be relieved by increased secretion from the bronchial mucous membrane. *Dyspnœa*, even when it has become habitual, may precede for some time any appearance of dropsy.

At last, however, the effects of the original evil augmenting and extending, the right ventricle also becomes unable duly to propel its contents into the pulmonary vessels; it continues morbidly full, is first distended occasionally, then permanently, and at length really dilated; and with that dilatation we have a turgid venous system, of which we see a part in the prominent veins of the neck.

In this way, then, may be explained a series of symptoms which you will often witness, and be consulted about, in persons who are growing old. You will find irregularity of the pulse; preternatural impulse perhaps of the heart; occasional shortness of breath; large crepitation, habitually audible in the lower and hind portions of the lungs; more or less expectoration, sometimes tinged, sometimes even loaded, with blood. Eventually the ankles begin to swell; and the patient (if his

life be not cut short earlier in some other way) becomes by degrees decidedly and universally dropsical.

Many of the direct signs of diseased heart may exist, therefore, while there is no anasarca: intermissions and irregularity in its movements, palpitation, the impulse proper to hypertrophy. But when dropsy has supervened, we may expect those signs also which denote dilatation of the right chambers. The heart is heard and felt to beat beyond the præcordial limits; the pulsations become feeble and unequal, if they were not so before; the patient is liable to fluttering palpitations, to extreme and panting dyspnoea on the slightest exertion, even on taking food into the stomach, or adopting the recumbent posture; his skin assumes a dusky hue, and his lips and extremities are apt to be livid.

In these cases the anasarca first becomes manifest about the ankles. During the earlier stages the oedema disappears in the night, and returns towards the next evening. It is sometimes confined, for a long while, to the legs; but ultimately it creeps up towards the trunk of the body; the thighs enlarge, the loins and flanks become doughy, the scrotum fills, and water collects in the serous bags of the abdomen and thorax. In extreme cases the dropsy is universal, pervading the areolar tissue of the head and face and upper limbs.

As the accumulation of serous liquid is commonly gradual, the reticular tissue, partly perhaps through maceration, but chiefly from continued pressure and stretching, loses its elasticity; and the oedema is soft, and *pits* readily.

Sometimes, the fluid continuing to increase, the cuticle is raised by it, and large vesications take place on the limbs; or some part of the areolar tissue sloughs, and a breach of surface is made, and the fluid drains off by this vent in great abundance, to the signal relief of the patient. This beneficial accident furnishes us with a valuable practical hint.

Such, then, is the consummation of disease commencing in the left heart, and working its gradual way, through the lungs, to the right heart. But the obstacle may originate at a less distant link in the chain. The circulation may be checked, first of all, in the intermediate lungs: and it may be worth our while to consider, for a moment, the relations which subsist between general dropsy, and certain *pulmonary* diseases.

Whenever, in pneumonia, a large portion of one or of both lungs becomes impervious to air and to blood—or when pleurisy fills one side of the thorax with liquid, which, by its pressure, shuts out both air and blood at once from one-half of the respiratory apparatus—the egress of the blood from the right heart, and, therefore, from the venous system, cannot but be checked. We might expect that dropsical effusion would be the result of these changes; and in truth it does sometimes occur. That it does not happen more frequently is to be attributed, I believe, to the free evacuations and the strict abstinence, which are early put in force in those complaints, and which relieve the venous plethora before it produces effusion.

So, again, lungs that are hollowed out into large cavities, or rendered solid over a wide space by numerous tubercles, are manifestly incapable of admitting into their vessels from the right ventricle the ordinary quantity of venous blood. In these cases, however, the whole mass of blood is diminished, and kept within the limit which does not imply distension of the veins, by the constant agency of various causes: by the imperfect nutrition consequent upon abdominal disease; by the sometimes copious expectoration; by the wasting diarrhoea; by the profuse nocturnal sweats. Accordingly anasarca is an unusual symptom in pulmonary phthisis, or shows itself in the latter periods only of the disease, in the form of oedema of the legs: and its occurrence then is mainly owing to the debility which affects, in common with the other muscular parts of the body, the moving organ of the blood.

The pulmonary disease which more commonly and certainly than any other, though often very slowly, leads to dropsy, is *emphysema* of the lungs. I showed you, some time ago, that in this morbid condition many of the smaller blood-vessels of the lung become gradually obliterated; and when the disease is extensive and advanced, large portions of the organ are visibly white and bloodless. Meanwhile, the nutrition of the body is not impaired; the same quantity of blood continues to be returned towards the heart, but it finds not a ready entrance into the pulmonary blood-vessels, when delivered from the right ventricle. A certain amount of accumulation becomes

habitual in that chamber, and in the great veins; nay, the very cause of the emphysema tends also to dilate the heart; at length the capillary vessels feel the mechanical congestion, and more or less anasarca ensues.

So much for dropsy that is purely cardiac. Let us next consider the circumstances from which, during the lifetime of the patient, we draw the conclusion that the dropsy he exhibits is of renal origin.

There is not much, that I know of, which is very peculiar or distinctive in the characters of the anasarca itself. This however is observable;—that whereas in cardiac dropsy the anasarcous swelling begins in the lower extremities, in renal dropsy it is often noticed first in the face and in the upper extremities; in the eye-lids, in the cheeks, and upon the backs of the hands. These you will remark are uncovered, and therefore visible parts. I believe that an equal amount of swelling would at the same time be detected, were it looked for, in the feet and ankles. Resulting more than the cardiac variety from an unnatural state of the blood, the watery effusion takes place more suddenly and more universally; from the vessels of every region of the body at once. Cardiac dropsy arises from mere mechanical delay of the blood—and that delay is felt most at the greatest distance from the heart, and in the most depending parts of the body. This early anasarca of the face and hands is the more marked in proportion as the renal disorder is recent and acute; or when, being chronic, local inflammation, or febrile disturbance, is suddenly superadded. According to my experience it is less noticeable when the dropsy comes on slowly and insensibly during the progress of chronic degeneration of the kidney. The more rapid and copious the effusion, the less do the dropsical parts pit upon being pressed. It may be said also of this renal form of dropsy, that accumulation in the larger serous cavities is not, in general, a prominent feature.

If we find, upon due scrutiny, no material or adequate embarrassment of the respiratory functions, no deviation from the natural sounds of the heart, no derangement of its regular movements, no alteration in the force of its pulsations, or in the space over which they can be felt and heard, no distension of the large veins of the neck—then we have strong reason for suspecting that the anasarca is connected with some vice of the kidney.

But we cannot infer, from the *presence* of heart-symptoms, that the kidney is free from disease.

Our judgment is guided, or assisted, in some degree, by the *complexion* of the patient. When general dropsy depends upon disease of the heart, the cheeks and lips are occasionally florid, often purplish or livid, frequently dusky and loaded. Sometimes (as in chlorotic women, where the heart may be temporarily distended without any positive organic disease, and the blood is thin and poor) the face and mucous membranes are pale: but in the renal variety of dropsy there is a very characteristic hue; an evident lack of red blood, indeed, in the capillaries, but withal an unhealthy dingy sallowness, significant, to a practised eye, of some deep-seated alteration of structure.

Our suspicion that the kidney is the organ primarily in fault is strengthened, if we trace certain accidents in the history of our patient. An attack, for example, of illness, attended, perhaps, with temporary swelling of the body and disturbance of the urinary functions (acute dropsy, in short), soon after some exposure, under unfavourable circumstances, to the influence of cold; either applied to the external surface, or to the stomach by a draught of cold drink. Or, a similar though transient anasarcous condition, which came on during convalescence from scarlet fever. For, as I told you yesterday, there is reason to believe that in *acute* dropsy is often laid the foundation of those peculiar changes in the kidney which, since they were first pointed out by Dr. Bright, have been chiefly studied in their connexion with *chronic* dropsy. That as rheumatic carditis may occur, and become latent as to its effects for some time, and yet implant the germs of future cardiac dropsy, so the stress or strain which befalls the kidney in cases of febrile anasarca, may set on foot a morbid process that long works silently and unobserved, but at last declares its operation by symptoms; the reproduction of the dropsy in a more chronic form being the most significant symptom of all. The acute attack may have been forgotten; there may have been no obvious (though there may have been ill-understood) indications of the renal affection; and its existence has been, therefore, unsuspected.

The discovery of intemperate habits would also be of importance in aid of our diagnosis, if these same habits had not a like influence in causing disease of other organs as well as of the kidney, and especially of the heart. There may, however, be no such episodes as these in the patient's history. The dropsy may have come on immediately after some exposure, or obviously injurious influence, yet not with acute symptoms, and in a temperate subject: in which case it is probable that the renal disease had pre-existed in a latent state. Or the anasarca may have stolen on by degrees, without any apparent cause.

But the most conclusive evidence of the renal disease is to be found in the conditions of the urine; which were fully explained to you in yesterday's lecture. I shall therefore only add four rules upon the subject, which have been laid down by Dr. Christison, and generally acquiesced in by Dr. Prout. These rules assert that organic disease, or granular degeneration, or granular disorganization of the kidney, — all which phrases we may condense into the compendious expression "Bright's kidney" — is present:

1. In most cases of febrile dropsy, including those which are consequent upon scarlet fever. To this proposition I have already given my full assent.

2. In all cases of anasarca, wherein the oedematous parts are elastic, and do not pit upon pressure. I am not so sure of this rule. I believe that the firmness of the oedematous parts has relation rather to the rapidity with which the effusion has taken place, than to the local origin of the dropsy. In proportion as the accumulation in the areolar tissue has been chronic and gradual, does the swelling receive and retain indentations from external pressure.

3. In most, or all cases of dropsy attended with diuresis, provided the urine be not saccharine. Such cases, Dr. Christison truly remarks, are far from being uncommon. Of course the diuresis here spoken of is such as occurs independently of diuretic remedies.

4. In all dropsies associated with urine of a very low specific gravity (say below 1010), and not exceeding the natural standard of quantity, whether it be albuminous or no. To the last two rules there are probably very few exceptions.

The dropsy which is dependent upon "Bright's disease" yields, not unfrequently, to treatment; but it is prone to recur. Of the renal disease itself the prognosis is, no doubt, very unpromising. Yet I do not think so despairingly of these cases as I once thought; and as many think. I have known a few instances of what seemed complete recovery after well-marked symptoms of the disorder. Certainly success enough has followed careful treatment, to encourage us to use all diligence in prosecuting such remedial measures as are already known, and in devising new ones. Of the two main forms of the disease, the large white kidney is the more immediately perilous. The appearance of oil in the urine is probably always of fatal omen.

I have been speaking of dropsies that are purely cardiac, and of dropsies that are purely renal. But I have already told you that disease of the heart and disease of the kidney frequently go together; and I have endeavoured to estimate their relation, in such cases, to each other. When both organs are structurally affected, the disposition to dropsical accumulation must evidently be augmented. What share they have, respectively, in producing the dropsy, it would be very difficult, and practically it is not very important, to determine.

In chronic general dropsy of a purely cardiac origin, the kidneys, being sound, offer the most convenient and eligible channel for carrying off the superfluous water. Diuretic medicines, therefore, rank among the most important of our curative expedients. When they fail to act, or prove insufficient for the purpose sought, we next have recourse, the state of the bowels permitting, to drastic or hydragogue purgatives.

Diuretics are notoriously of most uncertain operation; sometimes completely answering our wishes, oftener perhaps disappointing them altogether. Something may depend upon the way in which they are administered. There can be no doubt that liquids, after being conveyed into the stomach and intestines, pass thence into the blood by imbibition through the capillary vessels — nor any doubt that living membranes are subject equally with dead membranes to the physical laws of endosmose and exosmose. Hence it follows that remedies which are meant to reach the kidneys must be in a liquid form when taken, or must be capable of being dissolved afterwards in the fluids of the alimentary canal. In the last edition of his volume on

Urinary Deposits, Dr. Golding Bird lays down the further requisite condition, that the density of the solution must be considerably below that of the liquor sanguinis, or of the serum of the blood; lower, that is, than 1028. The proportion of solids (he says) dissolved in aqueous vehicles should always be less, when the purpose is diuresis, than 5 per cent.; otherwise that purpose is sure to be defeated: strong solutions of saline substances proving purgative through the exosmosis which they cause out of the blood—and weak solutions diuretic through the endosmosis which they cause into the blood. Dr. Headland, however, in his essay on the *Action of Medicines*—while he admits that a weak solution is more likely to pass off by the kidneys and a dense one by the bowels—questions, indeed I may say disproves, Dr. Bird's explanation of these facts. He shows that salines are in all cases absorbed into the blood, and that whether they are subsequently excreted through the kidneys or by the bowels, depends more upon the *quantity* administered than upon its degree of dilution. In fact the kidneys are not able to eliminate more than a certain amount of these saline medicines, which, to obtain the desired diuretic effect, should therefore be given in small doses, moderately diluted. The operation of diuretics is apt to be foiled when the bowels are irritable or lax. So likewise any impediment to the free entrance of liquids into the tributaries of the vena portæ from disease or congestion of the liver, which keeps those veins full—any mechanical hindrance to the subsequent course of the same liquids towards the emulgent arteries, from disease of the lungs or of the heart, producing general venous congestion—will tend to baffle the aim of drugs which are esteemed diuretic.

When the urine is strongly acid, and deposits, on cooling, a sediment like brick-dust, it may be well to try, at first, the alkaline diuretics, and particularly the salts of potass. Nitre added to the common saline draught; or a combination of the acetate and bicarbonate of potass; or the bitartrate in small doses; or the iodide of potassium; or the liquor potassæ. In my own experience the tincture of squills also has seemed to correct this superacid and turbid condition of the urine, while it increased its quantity.

The benzoate of ammonia is another salt which I have found to operate very powerfully, in several instances, as a diuretic.

Digitalis sometimes promotes, in a remarkable degree, the flow of urine; and this, in my judgment, is its most useful and manageable property. Small quantities of the tincture, or of the infusion, may be added to other formulæ. Or the powdered leaves may be combined in pills. But one of the best modes of exhibiting digitalis for this purpose is to give larger doses of the infusion, half an ounce, for example, in some cordial water, at intervals of four or six hours, till three doses have been taken in succession; and then to pause and note its effects; and to repeat the three doses, or not, accordingly.

The spirit of nitrous æther, and the compound spirits of juniper, and of horse-radish, have all of them well-marked diuretic properties, and may, with propriety, be added to most of the liquid formulæ for augmenting the discharge of urine. And, as vehicles for more active, or more concentrated ingredients, those vegetable infusions or decoctions should be chosen which are reputed to possess similar virtues; such as the decoction of broom-tops, or of juniper-berries, or of winter-green, or the infusion of buchu.

Squills, turpentine, the tincture of cantharides, are drugs of a more stimulant nature, more peculiarly adapted to cases in which there is no febrile disturbance, and the kidneys are obstinately inactive.

Sometimes a combination or farrago of diuretic substances proves more efficacious than larger doses of any of the ingredients administered singly: and the operation of some of these combinations is undoubtedly quickened and exalted, in many instances, by the addition of mercury. A fluid drachm of the officinal solution of the bichloride in each dose of a mixture; or small quantities of calomel, or of blue pill, when the medicines are given in a solid form. A very useful pill of this kind, much recommended by the late Dr. Baillie, consists of three or four grains of the pilula hydrargyri, mixed up with one grain of the dried powder of squills, and half a grain of the dried leaves of digitalis; to be given twice or thrice a day. Dr. Baillie states that squills and digitalis are much less effectual by themselves, than when combined with mer-

cury: which operates probably in relief of the portal circulation, by promoting a free secretion from the liver.

In choosing purgative drugs to aid the effect of diuretics in carrying off the dropsical fluid, or to take their place when these fail to act, we select those which produce copious and watery discharges from the bowels. A combination of jalap and cream of tartar has been long and deservedly esteemed for its excellent operation in this way. Gamboge is also a good cathartic. It may be given two or three times daily, in grain or two-grain doses, with a drachm of cream of tartar, suspended in two ounces of peppermint water. Or half an ounce of cream of tartar, mixed in six ounces of peppermint water, may be administered in one dose every morning. Croton oil, and elaterium, are still more powerful evacuates of serous liquid from the intestines. One or two drops of the former, or from a quarter of a grain to a grain of the latter, will be about a proper dose. It is astonishing how much relief to the feelings of the patient, and how great a diminution of the dropsical symptoms, are sometimes obtained by these active cathartics. Patients will earnestly beg for a repetition of them, even when their operation is attended, for the time, with considerable pain or sickness, and much general distress.

In addition to these measures for the removal of the collected water, attention must be paid to the actual condition of the heart. If the dropsy have been the result of anæmia, or of cachexy of the system, you must endeavour to strengthen your patient, and to repair his impoverished blood, by nutritious food and tonic medicine, and especially by the administration of steel. I told you formerly that preparations of iron have an exceedingly good effect, oftentimes, even in cases of *organic* disease of the heart, consisting in dilatation and tenuity, and consequently weakness, of its muscular parietes.

On the other hand, if there be violent palpitations of the heart, with a strong heaving impulse, you may appease the excessive action, and afford sensible comfort to the patient, by applying leeches, from time to time, to the præcordia.

Of the *renal* variety of chronic general dropsy, whether pure or mixed, the treatment is less accurately ascertained. The uncertainty which has perplexed men's minds respecting the nature of the renal disorder, has extended, in some measure, to their choice of remedies for it.

Whenever (in renal dropsy) acute symptoms and febrile disturbance occur, much relief may be expected from the abstraction of blood. When drawn from a vein, it usually shows the buffy coat. The existence at the same time of pain in the loins, would indicate the propriety of applying cupping glasses to that part. Nevertheless, the impoverishing effect of the disease itself upon the blood, and the probable dependence of some of the more distressful and alarming symptoms upon the thin serous condition of the circulating fluid, as well as the increased facility with which the dilute blood may transude outwards—these are circumstances which should induce every cautious practitioner to have recourse to this heroic remedy, only when it is clearly *demanded*. It is chiefly adapted to those cases in which the renal change occurs in connexion with acute or chronic inflammation.

One definite object, in the renal as well as in the cardiac variety, is to remove the dropsical fluid; from which the danger and the suffering often chiefly proceed. But it is a more nice question, when the *kidney* is involved in the disease, how this is to be accomplished. Can we, with the same safety as in cardiac cases, employ diuretics? It has been thought that we cannot. As the primary morbid state of the kidney is certainly often a state of active or inflammatory congestion, it has been feared that direct diuretics, such as are calculated to cause, keep up, or augment such congestion of the kidney, or to stimulate and irritate that organ, would be likely to accelerate the disorganizing process of which it is already the seat.

These views can scarcely now be considered theoretical. Their justness has been established, in respect at least to the nephritic kinds of renal dropsy, by those interesting results of modern inquiry which I yesterday brought under your notice. You had better, therefore, when you can, observe the caution which they suggest. You had better endeavour to empty the distended cavities, and to relieve the loaded areolar tissue, through the bowels, or through the skin. Sometimes, however,—more often indeed than in cardiac dropsy—we have the untoward complication of irritable bowels,

or habitual diarrhœa : and then drastic cathartics are inadmissible. But when this complication is not present, they are eminently useful.

Much benefit is sometimes derived from measures that act powerfully or steadily upon the cutaneous transpiration; and especially from warm, or hot-air, baths. The hot-air bath is, in many respects, preferable to the common warm-water bath, and even to the vapour bath. Upon the principle of heterogeneous attraction, the escape of the liquid from the surface of the body is more promoted by a dry heat, than by water artificially raised to a high temperature, and even than by an atmosphere made moist, as well as hot, by vapour. The risk, moreover, of exposure to cold, and the inconvenience, and hazard of fatigue, are much less : for the hot air can be brought, with but little trouble or expense, to the patient as he lies in bed. I have often applied the *sudatorium* to patients labouring under renal dropsy, and they have obtained much relief from it : but the relief is seldom of itself sufficient, or of more than temporary duration. Still it is an expedient that should never be neglected ; and in pursuance of the same indication, diaphoretic medicines should at the same time be diligently exhibited. Dr. Osborne states that when the renal disease has been uncomplicated with other organic mischief, he has always found the dropsy to disappear upon the re-establishment of the functions of the skin.

These measures failing—as fail they often will ; and diarrhœa forbidding the use of drastic purgatives ; or drastic purgatives and diaphoretics together proving insufficient ; we *must*, even in renal dropsy, of whichever variety, choose the least of two evils ; or rather we must incur the risk of one possible and contingent evil, for the chance of obtaining what, if obtained, is a certain and positive benefit : we must endeavour to remove the dropsical accumulation by means of *diuretics*, whether these accelerate the progress of the disease in the kidney or not.

Such diuretics, therefore, are, in the first instance, to be selected, as seen the least likely to stimulate the kidneys injuriously. The bitartrate of potass has been found one of the most certain and useful. Digitalis also is esteemed safer, and therefore more proper for this purpose, than many others : and the simultaneous exhibition of these two has, perhaps, the surest effect of all.

When diuretic medicines act as we intend them to do, and increase the quantity of urine, they are commonly of great service by reducing the dropsical swelling. Sometimes, however, no impression is made upon the anasarca, although the discharge of urine becomes plentiful. This is a very discouraging prognostic circumstance. And the drugs that we employ are apt to be very capricious and disappointing. In some unhappy cases of renal dropsy, I have tried every known form and combination of diuretics, without augmenting the secretion from the kidneys. Probably their secreting power was destroyed or arrested by the physical disorganization of the secreting apparatus. Could we be sure of this, it would become our duty carefully to abstain from all such medicinal substances as the healthy kidney is prompt to excrete. Urea appears to be the main natural diuretic ; and in this disease it already exists in the blood in too great abundance. On the other hand, the watery ingredients of the blood are sometimes transmitted readily enough through the diseased gland. The advanced stages, or certain forms, of the malady are liable to be attended with diuresis. We may even suppose that the obstructed tubules may sometimes be washed out by the descending streams of aqueous liquid ; and Dr. Johnson's suggestion is worth your attention, that, in these circumstances, "and more especially, perhaps, in cases of chronic nephritis," the safest and most useful diuretic is pure water.

It is yet an unsettled question whether mercury be advisable or even admissible, in these cases. The current of medical opinion sets against it : perhaps too strongly. It has been observed that salivation is apt to be produced by a small quantity of this drug ; and to be unusually troublesome and severe, without bringing any commensurate advantage. I mentioned to you formerly Dr. Farre's opinion, that mercury has the property of rapidly destroying red blood : if so, it must be regarded rather as an ally, than as an antagonist of this malady. On the other hand, some patients have appeared to recover altogether, after passing through a furious salivation. One of the reputed virtues of the mineral is that it promotes interstitial absorption—a property which the changes apparent in the kidney in some forms of renal dropsy would seem to render valuable.

When internal remedies prove ineffectual, and outward applications to procure

sweating miss their aim, we turn to those mechanical expedients which (in either variety of general dropsy) often afford ease, and prolong life, and may even, sometimes, perhaps, achieve a cure.

I have told you that the tense and stretched integuments occasionally give way; the areolar tissue sloughs, and from the breach thus made, water wells copiously forth, and great relief ensues. Sometimes, though very rarely, the whole of the accumulated liquid has so escaped, and the dropsy has not reappeared. The sore has healed, and the natural cure has been complete.

This spontaneous mode of draining away the liquid has been imitated by art. For the unwieldy legs become painful as well as cumbrous; the integuments threaten to inflame, or to mortify; and if we can diminish the tension by removing a portion of the included fluid, we avert or lessen this danger. The penis and scrotum also in the male, and the labia pudendi in the female, become, in many cases, enormously swollen, and hinder the exit of the urine, which is therefore spilled upon the tumid parts, rendering them erythematous and raw, to the grievous aggravation of the patient's sufferings.

Now seeing that vesications sometimes form upon the dropsical limbs, and give vent, in some degree, to the fluid, practitioners have been induced to follow that indication by exciting artificial blisters. But they are highly dangerous; leading often to gangrene of the surface thus inflamed. Within my recollection it was the custom to make sundry incisions with a lancet, in the cedematous legs. These gashes seldom healed again; but degenerated into sloughing sores; and not unfrequently they hastened the dissolution of the patient.

A vast improvement upon these expedients is the modern practice of acupuncture, which consists in perforating the integuments here and there with a fine needle.

It is surprising how much fluid may be let out in this way; and how much relief may be bestowed by this trifling operation. The liquid trickles rapidly forth; and I have known it soak through the patient's bed, and form a pool on the floor of the room. In one instance, a physician being my patient, the limpid fluid which thus oozed from a puncture in his thigh was caught, and collected in a glass, by means of a little gutter of oiled silk. It was found that ninety minims, or a fluid drachm and a half, escaped in a minute; which is at the rate of eleven ounces and a quarter in an hour: and this drain went on for upwards of four hours.

The surface on which punctures of this kind have been made sometimes becomes red: erysipelas supervenes, which it is difficult to arrest, and the patient sinks. In some of these cases the same event would probably have occurred, even although no punctures had been made, from mere tension of the integuments, and the progress of the disease. When such appearances present themselves, the affected limb should be kept in a horizontal position; and strips of linen, wetted with a solution of Goulard, should be applied to the inflamed surface.

Under the old system of incisions it was found (and reason would teach us to expect this) that there was more hazard of sloughing when they were made on the legs, than on the thighs. The risk is much less when needles are used. But even these punctures are not to be made without attending to certain precautions. They should not be too near each other: an inch and half, at least, should intervene between them. Neither should they be too numerous, nor too deep. The depth must depend upon the circumstances of the case; and especially upon the place where the puncture is to be made. The needle must not be pushed so deep as to penetrate or wound any fascia; for the danger of subsequent inflammation would thereby be increased.

Mead's plan, of making one incision, and no more, in either leg, on its inside, two fingers' breadth above the ankle, and through the areolar tissue, has been revived of late upon the continent, and commended here. The danger of sloughing is certainly less than when many cuts are made: and Dr. Todd points out, as advantages of this method, the rapidity with which the fluid escapes, and the avoiding "of that sloppy and soaking condition which inevitably ensues when the acupunctures are allowed to ooze all day."

The peritoneum may, at the same time, require to be emptied in the same mechanical way, by help of a trocar. I am always loath to recommend tapping, until the symptoms actually call for it, and until all other means of dispersing the water

have been tried in vain. But the circumstances that warrant or demand the performance of the operation, the dangers that attend it, and the means of obviating those dangers, were fully pointed out when we were on the subject of ascites.

By whatever means we may succeed in getting rid of the dropsy there will still remain (except in the comparatively few cases that are unconnected with organic disease, and depend simply upon debility and anæmia) the necessity for guarding against the re-accumulation of the water, by remedial measures addressed to the faulty organs. You may sometimes keep the disease of these organs in check, even when you cannot cure it.

In cardiac dropsies, besides the medicines already specified, undeviating temperance and regularity of life must be enjoined; and the patient should carefully, and always, avoid all active motion or exertion of the body, and all strong emotion of the mind; whatever, in short, might tend to hurry the circulation. You will scarcely be able to enforce this prudence, without plainly showing the patient the risk he will incur by its neglect.

In the renal variety of the disorder, in addition to the appropriate remedies already enumerated, particular attention must be paid to the avoidance of all exposure to cold and vicissitudes of the weather, and to keeping the surface of the body warm. Such patients should be constantly clothed in flannel from head to foot. To those who are able to choose their place of abode, I should strongly recommend resort to a warm climate. Some benefit may also be hoped for from counter-irritation—blisters or issues to the loins.

The diet in the chronic forms of the disease should be nutritive, but unstimulating. M. Solon suggests that if, in the renal cases, urea be detected in the blood, the patient should be restrained from too animalized a diet. Dr. Budd has had the same thought, and has put to the test, I believe, in the Hospital-ship Dreadnought, the utility of withholding all articles of food that contain azote. I have found this restriction entirely useless in one painful case, in which it was fairly enforced. In fact, the principle of such restriction appears to be wrong: the urea is furnished to the blood, not in the primary assimilative process, but in that which is secondary and destructive.

The discovery of the fatty character of one of these forms of renal degeneration has led to another plausible suggestion; viz., that the patient should be instructed rigidly to abstain from every kind of fat. But if the mottled and fatty kidney really be, as it probably is, one of the numerous issues of the scrofulous habit; this advice would seem to be of doubtful propriety, when we call to mind the confessed efficacy of some of the fixed oils, and especially of the cod's liver oil, in the treatment of strumous disorders. It is indeed the opinion of Dr. Williams, who bears strong testimony to this efficacy, that it results from the solvent power of the thin fish-oils upon the more concrete molecules of fat, by the morbid accumulation of which, the peculiar degeneration in question is constituted.

One more point, and I have done. Much unnecessary penance used to be inflicted upon dropsical persons, by stinting their allowance of drink. It was natural to suppose that the accumulation would increase in proportion to the quantity of liquid swallowed. But experience has shown this opinion to be erroneous, and

“crescit indulgens sibi dirus hydrops”

has ceased to be more than a poetical doctrine. The patient may safely be allowed to exercise his own discretion in this respect. When the peritoneum is full, distress is apt to ensue upon the distension of the stomach by drinks; but this source of suffering is soon discovered and avoided. The sick man is better able than his physician to judge which evil is the greater; the torment of unslaked thirst, or the discomfort that may be produced by its immoderate indulgence.

LECTURE LXXX.

Chylous Urine. Hæmaturia; its diagnosis, general and particular; Local disorders of the Urinary Organs on which it depends; Treatment. Disease of the suprarenal capsules; Bronzed Skin. Abdominal Tumours.

I HOPE I have convinced you that the morbid conditions of the urine are worth studying. You have heard, probably, of the quacks who call themselves "water-doctors," and who pretend that, by mere inspection of the urine of a patient living at a distance, they can tell what is the matter with him, and how he may be cured. This skill, which looks like conjuring, the scientific physician of the present day does really possess. Of some very important forms of constitutional disorder, and of some specific local maladies, he reads the sure evidence in the sensible and chemical qualities of the secretion from the kidneys. And I do not hesitate to say, that a rightly instructed person might form a more accurate opinion respecting a sick man fifty miles off, and prescribe for him more judiciously, upon being furnished with a vial of his urine, than some practitioners whom I have known could do, if they had the patient bodily before them. You may learn much (and so, no doubt, you ought) by prying into the arcana of the night-chair: but you may learn more, I am persuaded, by the habitual perusal of the chamber-pot.

I have not quite done with the subject.

Sometimes urine is voided, which appears to contain *chyle*. It looks white and milky, and stiffens as it cools into a tremulous jelly, like *blanc manger*, and takes the shape of the vessel into which it was passed. The coagulum gradually separates again into two portions: one of which is liquid and whitish, and when left at rest for a few hours throws up to the surface a sort of creamy matter, containing (as cream does) a butyraceous or oily principle; the other is a delicate fibrinous mass, of flesh-like appearance, having a red tinge from the presence of some of the colouring matter of the blood. This is the character of the urine passed a few hours after a full meal. When the patient has long been fasting, the urine is simply opalescent, and the coagulum small and partial. It never contains any casts of the urinary tubules.

Dr. Prout attributes this curious deviation from the natural qualities of the urine to a double fault; first, in the organs of assimilation; secondly, in the functions of the kidney. The chyle, from some derangement of the assimilative process, is not raised to the blood standard; and being unfit for its purpose, is ejected through the kidneys; and these organs, instead of converting it, as usual, into the lithate of ammonia, suffer it to pass through them unchanged.

Of this rare disorder I have not met with an instance. Dr. Prout had seen more or less of fourteen cases. Five of the patients were males, nine females. Two of the males, and one of the females, were below the age of puberty. Eight of the cases occurred in natives of the East or West Indies, or in persons who had lived for many years in hot climates. Mr. Thomas informs me, that during a residence of ten years in Barbadoes, he saw at least a dozen well-marked examples of chylous urine in negroes. It would seem, therefore, that a tropical climate predisposes certain individuals to this affection. In one case, drinking cold water while the body was warm seems to have been the exciting cause; and exposure to cold was thought to have had something to do with the attacks in other cases.

The general health suffers less than you might suppose. Two of the females, while labouring under the complaint, became pregnant, and brought forth healthy children; and one of the two lived nearly twenty years, with the disorder upon her all the time. Another of his female patients, who since his death has been seen by Dr. Bence Jones, first noticed that her urine was chylous in the year 1827. It continued so, with occasional intervals, till July, 1853, a period of twenty-six years, when the lady was (as she may be still) alive, and, this state of the urine excepted, in good health.

In the slighter cases there is usually some degree of feverishness, some uneasiness

in the back and loins, some thirst, a dry skin, and torpid bowels. When the malady is more severe, the symptoms approach to those of diabetes; the thirst is more urgent, the appetite unnaturally craving: and there is some degree of emaciation and debility. In this severer variety the urine is apt to coagulate before it leaves the bladder; and the patient experiences difficulty in passing it, the urethra being blocked up by the clots. Dr. Prout states that he has known this to constitute the most troublesome symptom of the disease. In one of the fatal cases the body was examined, and the kidneys found perfectly healthy. Occasionally the complaint ceases of its own accord, even for years, and then recurs without any apparent cause. We may conclude from these circumstances that it is a purely functional disorder.

With respect to treatment, little can be said. Dr. Prout has found several things of temporary service, in the chronic state of the affection. The mineral acids; astringents, such as alum, and the acetate of lead; opium; counter-irritation. But the suspended symptoms have always sooner or later returned. Dr. Bence Jones has put several drugs fairly to the test, and found the gallic acid more useful than anything else, though not a specific, in this disorder. In the case of a man, who also had formerly consulted Dr. Prout, the urine had been chylous, constantly, for more than a year. It ceased to be so two days after he began to take the gallic acid, and it remained free from the fatty admixture for 232 days, the acid having been continued for nearly two months. This looked like a cure. But the complaint recurred, and went, again and again. Twice subsequently the chylous condition ceased, after administration of the gallic acid, for the respective spaces of 237 and 242 days. The acid was frequently given to the large amount of two drachms daily, for weeks together.

Tannic acid was tried in this case: but it could not be taken in doses so large; and it caused much more nausea, and much more headache than the gallic acid. The chylous state was always abated by rest, and aggravated by exercise.

Urine which contains albumen exhibits it in the solid form under the tests of heat and nitric acid.

Yet I have met with one remarkable case in which those tests did not produce the usual precipitate, although an enormous quantity of an albuminous substance was passing out of the body in the urine.

This urine was of a high specific gravity. It became thick with heat, from a deposit of phosphates, but cleared again with a drop of acid. It gave no precipitate with an excess of nitric acid, unless left to stand, or unless heated and left to cool, when it became solid. This solid was redissolved by heat, and formed again as the liquid again cooled.

The case was one of *mollities ossium*. A full analysis of the urine has been published, in the *Philosophical Transactions* for the present year (1848) by Dr. Bence Jones, who found the peculiar substance thus secreted to be hydrated deutoxide of albumen.

There was as much of this albuminous substance in the urine, as there is of ordinary albumen in healthy blood. So far therefore as the albumen only was concerned, each ounce of urine passed was equivalent to an ounce of blood lost.

Dr. Bence Jones informs me that the same substance, in small quantity, is contained in pus: and has received the name of *pyine*. He thinks it probable that the substance which characterizes chylous urine, is closely related to this.

Lastly, the urine may contain *blood* itself: and I proceed to consider some of the phenomena that occur in connexion with *hæmaturia*; under which term I would comprehend every kind of bleeding from the urinary organs.

Blood alters, of course, the colour of urine with which it is mixed: giving it, in some instances, a bright red tinge, and causing it, in others, to assume a dark hue; to become brown, like coffee, or even to approach to blackness. Hence we are sometimes too ready to conclude that urine of a distinctly red colour, or so very dark as to appear almost black, derives its peculiar tint from blood that has somehow mingled with it.

But, in truth, urine may be perfectly red, or nearly black, and yet be quite free from blood. There are certain substances which, when taken as food, invariably

impart a red colour to the urine. One of these is the prickly pear, or Indian fig as it is sometimes called, the *cactus opuntia* of botanists. When the Spaniards first took possession of America, many of them were alarmed by observing that they passed what they supposed to be bloody urine; but it was soon discovered that the red colour of the secretion was owing to the liberal use they made of that fruit. Dr. Hennen, in his book on *Military Surgery*, quotes a precisely similar example from Elliot's Journal of his Travels for determining the boundary of the United States. He says that his "people ate very plentifully of this substance at an island of the Mississippi (Kayo-ani), and were not a little surprised the next morning at finding their urine appear as if it had been highly tinged with cochineal. No inconvenience resulted from it. It would seem (he continues) that the juice of this plant may be analyzed into a crimson dye by other processes beside that of the cochineal insect."

Another vegetable substance with which, in this country, we are more familiar, and which will produce the same effect, is *beet-root*. Desault relates the case of a person who noticed that he every morning voided urine of a deep red colour; exactly such as would result from adding fresh blood to that liquid, except that no deposit took place. The man became frightened at this, and consulted M. Roux, who, after some examination, began to suspect that the water owed its red appearance to some other cause than an admixture of blood. It turned out that his patient was in the habit of supping every night upon the red beet-root; and as soon as, by M. Roux's advice, he relinquished this article of diet, he was freed at once from his supposed bloody urine, and from his fears.

A similar tinge is said to be given to the urine by the use of *madder* as food, by some species of strawberries, and by drinks made of sorrel. *Logwood*, which we sometimes use in medicine, has the same effect. Rhubarb also, and senna, give to the urine, *if it chance to be alkaline*, a blood-red colour.

It is right that you should know these facts: for by swallowing large quantities of such substances, and by complaining of sensations which they do not really feel, impostors may endeavour, without any difficulty, or pain, or danger to themselves, to deceive others into a belief that they are suffering under some serious and disqualifying disease, and are proper objects of charity. Moreover, a knowledge of the effects of these vegetable matters may sometimes enable you, as in the case mentioned by Desault, to remove unfounded anxiety and alarm from the minds of persons who are innocently and unconsciously giving themselves red urine.

The natural tint of the urine inclines towards redness, independently of any admixture of blood, in many instances of fever, and of acute inflammation. Occasionally urine of a pink colour is passed by persons who are subject to obstinate dyspepsia connected with organic disease. This pink tint is most apparent when the water is contained in an opaque, shallow, white vessel.

Again, urine of so deep a colour as to be called, in common parlance, *black*, may or may not owe that hue to the presence of blood. When blood is the cause of the unnatural colour, the blackness must be ascribed to the chemical action of some free acid upon the blood: as I showed you formerly to be the case with blood that is vomited, in hæmatemesis.

I also pointed out to you some little while since, that the urine, in jaundice, sometimes *seems* to be black, when it is collected in large quantity, and in a deep vessel. This colour is merely a concentration of yellowness, as appears at once upon diluting the urine with water. It then assumes a bright yellow colour. The aspect of the skin in icterus will always secure you against mistaking or overlooking this cause of black urine.

There has been observed, also, though very rarely, a form of black urine, depending upon the presence in that secretion of a peculiar principle to which Dr. Marcet gave the name of melanic acid. The only specimen of really black urine that I ever saw, was shown to me by Dr. Prout; who knew nothing, however, of the circumstances under which it was voided. It appeared to me to be full of coal-dust.

With these two exceptions, almost all urine that is of a very dark or blackish colour owes that quality to the circumstance of its containing blood, which has been more or less altered, by various causes, from its original appearance.

When blood is present in any considerable quantity, a portion of it subsides to the bottom of the vessel, and may be recognized without any difficulty. And even when

there is not enough blood to give a marked and characteristic deposit, a very small admixture of it will be found to disturb the natural transparency of the urine, rendering it of a smoke-brown, or dull cherry colour: whereas the reddish or pink urine which contains no blood is clear and untroubled; and if, on cooling, it throw down a sediment, that sediment may be redissolved by heating the urine—a result which does not take place when a portion of blood has been deposited. Another rough test is, that a mixture of urine and blood tinges a piece of white linen dipped into it, of a red colour. A better criterion is afforded by gradually raising the suspected urine to the boiling temperature. If it contain blood, a greyish brown flocculent precipitate, consisting of coagulated albumen tinged with the colouring matter of the blood, will form, and gradually subside, and leave the supernatant liquid clear, and with its natural tint. But if you use the microscope, that will furnish you with the best evidence upon this point. If there be blood in the urine; there will be blood-corpuscles, turgid or collapsed, diffused through the urine, or collected at the bottom of the vessel: and whatever modifications they may present, they may always, Dr. Bird says, be identified by “their non-granular surface, uniform size, and yellow colour under the microscope.”

We have by no means accomplished the diagnosis when we have merely ascertained that there is blood in the urine; and that the case is a case of hæmaturia. The question remains, of what is such hæmaturia a sign? The blood emerges from the urethra, but it may have been poured out at any point of a long and somewhat complex tract of mucous membrane. It may have proceeded from one or both of the kidneys, from each or either ureter; from the bladder; from the prostate gland; or from the urethra.

Hæmaturia strictly idiopathic must be very rare. Cullen remarks that neither he nor any of his friends had ever met with an instance of it. I shall mention presently the only example of hæmorrhage from the urinary organs, apparently idiopathic, that has fallen under my own notice.

Blood in small quantities is excreted with the urine in that acute affection of the kidney which I have already spoken of as lying at the root of most, if not of all cases of febrile dropsy. Respecting its origin in such cases we need have no doubt: for besides the colouring matter and corpuscles of the blood, the urine is found to contain blood-casts, which have obviously been moulded in the urinary tubules, and in which epithelial cells, detached from those tubules, are often entangled. The same phenomena present themselves sometimes—though much more rarely—in the chronic forms of Bright’s disease. Blood-casts, without epithelial cells, have been seen by Dr. Johnson in a case of strangury produced by oil of turpentine; and it is probable that the appearance and conditions of the urine are the same in strangury from cantharides. Hæmaturia is occasionally, I believe, vicarious of some other hæmorrhage, and especially of bleeding from the hæmorrhoidal vessels: so that it is always right, in obscure cases, to inquire whether the patient has been habitually subject to hæmorrhage from the rectum; and if so, whether that hæmorrhage is suspended. These cases have even been called *hæmorrhoides vesicæ*.

Hæmaturia occurs also, independently of any strictly local complaint, in the course of certain disorders which affect the system at large; especially in scurvy and purpura hæmorrhagica. Bloody urine is sometimes a symptom, and one of the most fatal augury, in typhus fever, small-pox, measles, and the plague.

But setting aside these more general forms of hæmaturia, let us inquire what local affections of the urinary organs, besides those already referred to, may give rise to hæmorrhage; and how, under different circumstances, we are to interpret this symptom.

One very common source of hæmorrhage from the urinary passages, is the presence within them of calculous matter. The pressure occasioned by the aggregation of the earthy mass, when it is formed in the kidney, or by its accidental change of position, lacerates, or lays open by ulceration, some of the smaller vessels with which it is in contact. And in those cases in which a calculus descends into the bladder, and is ultimately voided, it may, in succession, give rise to hæmorrhage, first from the kidney from which it is separated; secondly, from the narrow tube of the ureter through which it is forced; thirdly, from the bladder which it enters, and wounds,

or irritates; and fourthly, from the urethra in the last stage of its progress out of the body.

There will be the same liability to hæmaturia, if the concretion, instead of coming down from the kidney, be formed originally in the bladder. The appearance of blood in the urine suggests therefore, in many cases, the fearful suspicion, that there is, or is likely to be, a stone in the bladder. Dr. Heberden, in his *Commentaries*, says, "urine made of a deep coffee colour, or manifestly mixed with a large quantity of blood, has within my experience been very rarely the effect of any thing but a stone in the urinary passages. I therefore suppose a strong probability of this cause, whenever I see this appearance."

Again, blood may proceed from the kidney, or from the bladder, in consequence of malignant fungous growths, to which those parts are liable: a disease which, though more surely fatal than the stone, is scarcely, to the unhappy subject of it, so appalling.

Hæmorrhage may take place from the surface of the bladder from chronic disease, not cancerous, of that membrane. Mr. Howship has recorded an instance of this kind which occurred in Mr. Heaviside's practice. An old East Indian, who had long been subject to nephritic complaints, was suddenly seized with what was thought to be retention of urine. A catheter was passed, but as no water flowed it was supposed that it had not entered the bladder, in the situation of which there was a manifest tumour. The patient died the next day; and the bladder was found distended by a very large coagulum of blood which had come from the diseased mucous membrane. There was no trace of hæmorrhage in the kidneys, nor in the ureters.

I hold in my hand a preparation showing disease of the prostate gland, which had been accompanied by hæmaturia.

Now we judge of the exact seat of the hæmorrhage, and of its cause, partly by the nature and appearance of the effused blood, and partly by the symptoms that precede or accompany the bleeding.

Dr. Prout states that "when blood is derived from the *kidney*, it is in general equally diffused throughout the whole urine: on the contrary, when derived from the bladder, the blood for the most part comes away in greater or less quantity at the termination of the discharge, the urine having previously flowed off nearly pure."

There are also certain modifications of the sensible qualities of the excreted blood, by means of which the same eminent physician thought he could pronounce, with considerable confidence, that the hæmorrhage was owing to malignant disease. "The red particles of the blood (he says), discharged in the earlier stages of fungoid disease, have often a remarkable appearance, and *appear to the eye* larger than natural; so that after they have subsided to the bottom of the urine, they at first sight somewhat resemble grains of lithic acid gravel, and, like that substance, when the vessel is inclined, may be distinctly seen to roll along the bottom. From this peculiar appearance of the red particles of the blood, the presence of malignant disease may be often suspected before the symptoms assume a decided character." In a more advanced stage of the disease, there is often a dark-coloured offensive bloody sanies in the urine, and more or less of mechanical impediment in passing it. I should conceive that the microscope might aid the diagnosis of such cases.

There is one phenomenon which, whenever it occurs, is very characteristic of hæmorrhage from the kidney, or from the commencement of the ureter. I mean the expulsion, with the urine, of slender cylindrical pieces of fibrin, which have evidently been moulded in the ureter, and subsequently washed down into the bladder by the descending urine. These little coagula are commonly of a whitish colour, the red particles of the blood having been removed; and they look like slim maggots, or small worms. They denote, with much certainty, that the hæmorrhage which they accompany is renal.

Such, then, are some of the points of diagnosis furnished by the qualities of the excreted fluid itself.

The bleeding may be presumed to come from the kidney, or from the upper part of the ureter, when it is accompanied or preceded by a sensation of heat, or of weight, or by some degree of pain, in the situation of the kidney; especially if these uneasy feelings are confined to one side of the body. This presumption will of course be strengthened if calculi have been known to descend from the kidney; and converted

into certainty if the patient suffer, together with the hæmaturia, a fit of the gravel; and if there be no symptom of stone, or of disease, in the bladder. As the blood comes, in these cases, from the calices or the pelvis of the kidney, the urine contains no blood-moulds of the renal tubes.

On the other hand, when no symptoms referable to the kidney or to the ureter are present, while there are signs of stone, or of disease of the bladder, or of a diseased prostate—a mixture of mucus with blood; occasional retention, or a sudden stop in the stream, of urine; pain referred to the glans penis immediately after the bladder is emptied—then we conclude that the blood proceeds originally from that receptacle.

When pure blood comes away, either *guttatim*, or in a stream, unmixed with urine, and neither preceded nor accompanied by any desire to make water, it is probable that the *urethra* is the locus of the hæmorrhage.

Bleeding from the surface of the urethra doubtless may, and commonly does, proceed from some mechanical injury done to that channel: as in the passage outwards of a fragment of stone, or inwards of a surgical instrument. But it is probable that blood may sometimes exude in considerable quantity from ruptured capillaries of the same membrane, under circumstances which favour or produce a strong determination of blood to the genital organs. A young man came to the Middlesex Hospital with hæmorrhage from the urethra, and said that he had lost a considerable quantity of blood in this way, within a few hours. The hæmorrhage appeared to have been the consequence of excessive indulgence in sexual intercourse. His own account of the matter was that he had passed the night with a female, in whom the monthly period had just returned; and he ignorantly fancied that the hæmorrhage from his own person was the result of a sort of contagion. However, the bleeding was permanently arrested by the introduction of a bougie, which was allowed to remain for a short time in the urethra. This was the solitary instance to which I alluded just now, of (perhaps) idiopathic hæmorrhage, occurring within my own knowledge. When the hæmorrhage comes originally from the urethra, the blood may regurgitate into the bladder, and coagulate there; and mislead an observer into the belief that the hæmorrhage was vesical.

It appears then, that, in many instances, certain local symptoms are associated with hæmaturia, and point distinctly to the part of the urinary apparatus whence the blood proceeds.

But many cases are very obscure. Blood sometimes appears, mixed in greater or less quantity with the urine, when there is no pain, nor any other sign which would lead us to fix upon one part rather than another as the source of the hæmorrhage. Now I believe that hæmaturia, bearing this indeterminate character, will generally turn out to be *renal*, and to depend upon concretions in the kidney. This conclusion will be strengthened if (as often happens) there are more or fewer *pus* globules excreted with the blood. It is true that the hæmorrhage which results from cancerous disorganization, whether of the kidneys or of the bladder, may also be painless. But cancerous disease of these organs (unless it extends from parts in the neighbourhood, as from the rectum, or from the uterus, to the bladder) is very rare; and when it does occur, the nature of the case may usually be ascertained from those peculiar qualities of the effused blood which I have mentioned as being characteristic of malignant growths.

A calculus can seldom remain long in the *bladder*, at any rate will seldom cause bloody urine, without giving some other notice of its presence there: but concretions form in the kidney, sometimes in great numbers, and reach a considerable size, and remain there long, without furnishing any signal from which we might suspect their existence; except (perhaps) the occurrence of hæmaturia. We know this, because calculi are frequently met with in the kidneys of persons who have never suffered any pain or obvious derangement of the urinary organs during life; and because, in other persons, in whom such calculi pass down from the kidney towards the bladder, the first notice of their existence is often given by the acute suffering they inflict during their transit through the narrow ureter.

Yet though calculi may lodge in the infundibula, or in the pelvis, of the kidney, without manifesting their presence by exciting pain, it is very conceivable that, by progressive enlargement they may lay open, or, by accidental change of position they may wound, some of the smaller blood-vessels of the part, and so give rise to painless

hæmaturia. It will strengthen the presumption that such is the source of the bleeding, if it have succeeded (as hæmorrhage from the urinary passages often does succeed) to a fall; a shock, or jar of the body; or jolting on horseback, or in a carriage. Similar movements may occasion bloody urine when there is stone in the bladder; but then the irritation will be *felt* in that sensible part; the hæmaturia will *not* be painless; the bleeding will not be the only symptom.

It is then, I say, my belief that very many of the obscure cases of hæmaturia may be referred to renal calculi: and if this view of the subject be correct, it will render it probable that the alleged instances of idiopathic hæmorrhage from the kidneys ought thereby to be reduced in number.

The expulsion of the blood in hæmaturia, whether it be painful or not, is sometimes attended with severe rigors. I mentioned before that, in some persons, almost any irritation of the urethra, the passing of a bougie for example, will bring on a shivering fit. I had some time ago a patient under my care in the hospital, who had hæmaturia of an obscure kind, and the discharge of blood was always marked by a smart rigor. Dr. Prout speaks of an instance of obstinate hæmaturia in which a shaking fit constantly preceded the hæmorrhage. Dr. Elliotson, too, in one of his lectures, gives an account of a case of intermittent hæmaturia. The patient was under his care in St. Thomas's Hospital, and had formerly had the Walcheren fever. He was admitted for ague, and every time that the cold stage of his attack came on, he voided a quantity of pure blood from the urethra. He was cured, by quina, both of his ague, and of his hæmorrhage.

If we may trust to the records of physic, instances of periodic hæmaturia are not uncommon.

One circumstance yet remains, worth noticing, in respect to hæmaturia; and it depends upon the hæmorrhage itself rather than upon the disease of which the bleeding is a sign. I allude to the coagulation of the effused blood in the bladder, however it may have got there. This circumstance is sometimes the source of much inconvenience, and suffering, and even of danger, to the patient. It may cause retention of urine, and all its evil consequences; and a still worse event is, that the coagulum sometimes supplies a nucleus, around which calculous matter is deposited, and thus lays the foundation of that horrible malady, "the stone."

You will perceive, from what I have said, that the *treatment* of hæmaturia resolves itself, in most cases, into the treatment of the disorder, or bodily condition, with which the hæmorrhage is associated, and of which it is merely a symptom.

Sometimes, however, the bleeding itself is so profuse, or so long-continued, as to require direct efforts on our part towards its restraint.

"When (says Dr. Prout) the bladder becomes distended with blood, and complete retention of urine in consequence takes place, recourse must be had to a large-eyed catheter, and an exhausting syringe, by the aid of which, and the occasional injection of cold water, the coagula may be broken down and removed. If the hæmorrhage be so profuse that the bladder becomes again distended with blood in a very short time, the injection of cold water into the rectum or bladder is sometimes of great use; and should these means fail, from twenty to forty grains of alum may be dissolved in each pint of water injected into the bladder; a remedy that seldom fails to check the bleeding, even when the cause is malignant disease. I have never known any unpleasant consequences follow the use of this expedient, and have seen it immediately arrest the most formidable hæmorrhage, when all other means had failed; and when the bladder had repeatedly become distended with blood, almost immediately after its removal."

Among remedies given by the mouth, the same physician thought highly of the acetate of lead. I have mentioned before, in these lectures, a nostrum called, after the name of its inventor, *Ruspini's styptic*. This has often been known to put a stop to hæmorrhage which had resisted other remedies. I will read you one example of this from Sir Benjamin Brodie's published lectures. Speaking of hæmaturia, dependent upon disease of the prostate gland, he says: "Those medicines which operate as styptics when taken internally, and which are useful in cases of hæmorrhage from the lungs, are also useful in hæmorrhage from the prostate. I had a patient with very diseased prostate. A frightful hæmorrhage took place. The usual methods of treatment were adopted, but were of no avail. The skin became pale, the pulse

became weak, and the patient was exhausted; yet the bleeding continued. Large quantities of blood were drawn off with the catheter: nevertheless the bladder continued to become more and more distended with blood, and was felt prominent in the belly as high as the navel. All other remedies having failed, I gave the patient a dose of the nostrum known by the name of Ruspini's styptic, and repeated the dose two or three times in the course of the next twelve hours. In about half-an-hour after the first dose was taken the hæmorrhage ceased; and it never returned. The patient lived a year and a half afterwards, and there was no reason to believe that any ultimate harm arose from the bleeding."

For a long while this nostrum seems to have baffled analysis. The late Dr. Maton told me that Dr. Wollaston had examined it, and had arrived at the negative conclusion, that it contained no metallic substance. D. A. T. Thomson afterwards announced that it mainly consists of a solution of *gallic acid* in alcohol, diluted with rose-water.

There is no substance more highly spoken of as a remedy for internal hæmorrhages by foreigners, and especially by the French, than the extract of *Rhatany* root, the *Krameria* of our Pharmacopœia. A woman was sent to me by my colleague, Mr. Arnott, complaining that for some weeks she had been passing bloody urine. She had gone through the ordinary routine of treatment without benefit. There were no symptoms present which threw any light on the precise source or cause of the hæmorrhage. I recommended a trial of the rhatany, and she began to take a scruple of the extract, mixed with water, three times a day. As in Sir Benjamin Brodie's case, the hæmaturia ceased after the first dose, and it did not return for many months. I mention this instance the rather, because the *gallic acid* enters into the composition of this vegetable extract also.

Now the gallic acid is one of those substances which, when introduced from the digestive organs into the blood, passes through the round of the circulation unchanged, and reappears in the urine. We may conceive, therefore, that it stays internal hæmorrhage by exerting its astringent property upon the ultimate capillary blood-vessels in its passage through them. It certainly is applied, in solution, after its elimination from the blood, to the urinary passages: and thus, in hæmaturia, it may be presumed to produce its styptic effect upon the bleeding surface.

To the same principle are owing, I believe, the astringent and styptic virtues of uva ursi, bistort, tormentil, the pomegranate, kino, catechu, and the several preparations of gall nuts. But as the efficacy of the gallic acid in restraining internal hæmorrhage is now well established, I would recommend you to employ it in its separate and simple state; since it may thus be prescribed in quantities more definite and precise, than would be possible in its natural combinations with so many different vegetable matters. It may be given three or four times a day, in doses of five or ten grains each, suspended in water by means of mucilage.

In cases of vesical hæmorrhage, with an alkalescent state of the urine, much benefit has been obtained from a steady use of the muriated tincture of iron.

In former courses of these lectures I have said nothing—for I knew nothing—of the morbid conditions of the *supra-renal capsules*. I was ignorant alike of their physiology and of their pathology; of their uses and of their diseases. A pathology, however, they have, which vindicates the importance of these little organs in the bodily economy, although it does not disclose their purpose. The suspension of their function (the necessary result of the destruction, or the extensive impairment of their structure) is fatal to life: and this mortal inward change is revealed before death by one very conspicuous signal—unregarded, indeed, till our own day, and reserved for the sagacity of our distinguished countryman, Dr. Addison, to discern and to interpret. It is very remarkable that two physicians living at the same time, in the same town, and attached to the same hospital, should have brought to light two most serious forms of disease, utterly unknown before, in parts of the body so contiguous: and have thereby won for themselves an enduring place in the records of medical science. Henceforward the names of Bright and of Addison must be held in honourable remembrance whenever mention is made of renal and of supra-renal pathology.

Without dwelling upon the steps which led Dr. Addison onwards to his remarkable discovery, I will describe the group and succession of symptoms that ordinarily result from the spoiling of these internal organs.

It is not uncommon to meet with a sick person whose history is of this kind. He knows not how, nor precisely when, his illness commenced, but he has gradually fallen from his usual state of health: has become weak, pale, thinner, but generally not emaciated, languid, spiritless, unequal to bodily or to mental exertion, with flabby muscles, and a soft pulse, which commonly is very feeble also.

For this deteriorated and anæmic condition you can trace no intelligible cause. There has been no exhausting profluvium; no loss of blood, morbid or artificial; no diarrhœa, diuresis, or other drain upon the strength; no wasting excess or indulgence; no mental shock or anxiety. No fault is discoverable in the lungs, in the heart, in the kidneys, in the digestive organs; no direct evidence of malignant disease in any part. Yet the diminution of strength is progressive—faintness and vomiting sometimes supervene—the mind becomes confused, the pulse grows weaker and weaker, until at length the flickering lamp of life goes out, or is extinguished in a sudden convulsion.

This unexplained train of symptoms should suggest the suspicion of disease in the supra-renal capsules; and the suspicion will be turned into something like certainty, if, during the progress of the case, a change of colour begin to be perceptible in the patient's skin, first and chiefly in parts that are uncovered by the dress, as the face and hands, but elsewhere also. The colour which thus takes the place of the natural hue, is a brownish yellow. You fancy, while it is yet slight, that the hands and face may have been tanned by the sun: or that the patient may be labouring under a dirty-coloured jaundice. This latter notion is, however, at once refuted by the pearly whiteness of the conjunctiva, the paleness of the finger-nails, and the absence of any bile-tinge in the urine. Dr. Addison speaks of this colour, which deepens with the advance of the disorder, as being dingy, smoky, as presenting various tints or shades of amber, or of a chestnut brown. It is somewhat like the stain produced by the juice of walnuts, or by the tincture of iodine; or, finally, the skin looks, in the affected parts, as though it had been *bronzed*. And the name of *bronzed skin*, familiarly given to the complaint, is preferable, while we are still so ignorant of its real nature, to any more formal appellation which might be derived from the Greek or Latin.

This hue is often partial, and occupies principally the front of the body and of the limbs, especially of the thigh. When it is spread over the whole surface, it varies in intensity, being darkest in the flexures of the body, in the armpits, and groins, round the navel, and upon the scrotum. Sometimes it is deeper here and there, and gives a mottled appearance to the surface; and sometimes it is diversified with white patches, in which the skin is blanched, is more white and colourless than healthy skin, like the nails and the conjunctivæ; and the hair on the head and on the pubes, corresponding to these spots, becomes perfectly white.

In several instances the blood, when examined under the microscope, has been found to contain an unusual number of white corpuscles.

The body and breath of the patient have exhaled, in two or three recorded cases, a peculiarly offensive and penetrating smell. In one report this odour is spoken of as being somewhat like that of a negro.

Though the disease is mostly a fatal disease, the state of the renal capsules is not always the same. Sometimes they are seen to be cancerous, sometimes enlarged, sometimes dwindled and atrophied. Most frequently of all they have undergone a calcareous degeneration, or have been converted into a softer putty-like substance. It is not, apparently, upon the character of the change, so much as upon its extent and completeness, upon the utter destruction of the natural texture of the capsules, and the consequent abolition of their functions, that the occurrence and march of the specific symptoms depend. The rapidity of the morbid change may also modify the outward manifestations of the disorder.

Disease of the supra-renal capsules has, in a few examples, been met with when there had been none of the characteristic bronzing. This is believed to have occurred only when the alteration of structure was *partial* or *recent*. One of them may be affected, the other sound; or each, being unsound, may yet retain enough of its proper structure to enable it to fulfil its office, whatever that may be. Again though both capsules may be pervaded throughout by the same morbid process, there may not

have been time for the production of the peculiar colour of the skin, which is not one of the earliest symptoms, and which augments as the malady makes progress.

That profound disorganization of the supra-renal bodies, and bronzing of the surface, stand to each other in the relation of cause and effect, is rendered the more probable by the negative evidence which has been collected on this subject. In 500 inspections of the dead body conducted by Drs. Wilks and Habershon, in all of which the renal capsules were examined, disease of those organs was never found without its having been predicted during life, except in two instances. In one of these the skin *had* shown a slight dinginess, and a large mass of cancerous disease involved the whole of *one* of the capsules. In the other case also, one only of the capsules was affected; a few malignant tubercles grew from its surface.

Once only, so far as I know, has this peculiar bronzing of the skin been observed, with a perfectly healthy condition of the supra-renal capsules. The capsules themselves, and portions of the altered skin were shown by Mr. Hutchinson at a recent meeting (March 17, 1857) of the Pathological Society of London. This occasional dissociation of the two phenomena does not disprove the law which in so many more instances connects them as cause and consequence: it only shows that the bronze-like skin may be due to some other cause than disease of the capsules. You will do well to guard yourselves against mistaking some forms of *pityriasis* for this bronzing disorder. *Pityriasis* is a vegetable parasite of the human skin; and its vegetable character may be readily detected by the microscope.

I have said that the disease is *mostly* fatal, by which I meant that perhaps it is not invariably so. In not more, however, than in one or two recorded instances, has the characteristic colour disappeared, with the rest of the symptoms, and health been restored. Our knowledge of the subject is yet very crude and imperfect: but, admitting the correctness of the diagnosis, it has been reasonably conjectured that in these cases the whole of both capsules might have suffered some remedial change—some congestion or moderate inflammation for example—which at length receded and ceased, without ultimate damage to the integrity of the parts affected.

Again, to meet the opposite difficulty, of general disease of the capsules unattended by bronzed skin, it has been supposed, as I just now stated, that the whole of each capsule may undergo rapid softening and disorganization, under *acute* inflammation or some other spoiling process, and so the disorder prove fatal, before there has been time enough for the skin to assume the tawny hue.

What the functions of these little bodies may be, Physiology has not yet pronounced. They are amply supplied with blood; they have a cortical and a medullary structure analogous somewhat to those of the nervous centres; and they have direct and numerous relations with the great solar plexus. That they are concerned in the preparation and maintenance of the blood: that they influence the functions of the sympathetic system of nerves: that they regulate the formation, or the destruction, or the distribution of the animal pigments: these are among the guesses at their purpose. In support of this last guess is the fact that the colouring matters of the body are, to a certain extent, misplaced. The dark bronze-like hue, which more than one observer has described as giving to the patient the complexion of a mulatto, has been ascertained to be produced by dark granules, or cells, in the rete mucosum. Indeed the aspect of the rete mucosum, as seen under a microscope, approaches closely to that of the negro. It is said that the renal capsules are very large in the negro, and very small in the white or albino rabbit. How this may be I cannot say, but it would seem that according to this view of the disorder, these conditions should be just exactly reversed. Certainly, in the disease, the less there is of efficient renal capsule, the more there is of the dark pigment beneath the skin. In one of Dr. Addison's published cases, black spots were sprinkled over many of the abdominal viscera.

My own experience of this formidable disease has been but small. The barrister whose case (the 6th in the book) is described by Dr. Addison, was a patient of mine; and it was a vast comfort to me to have his advice and help under circumstances so distressing.

In January, 1856, I was consulted at my own house by a surgeon from the country, thirty-four years old, who was evidently drooping under the same malady. His face had a tanned appearance, his hands were bronzy, with pink contrasting fingernails, his scrotum was very dark, and a few soot-like spots were scattered here and

there upon his skin. His urine was light coloured. His stools had never wanted bile. There was no tinge of yellow in his eyes.

He told me that he first noticed the dark colour of his scrotum in 1853. He died in April, 1857. The disorder existed therefore for four years at least. He began to perceive that he was unwell—to feel weak, to waste, to suffer fits of languor and depression, and to change in complexion—in December, 1854. Cold weather always distressed him.

The course of this gentleman's disease was marked by fluctuations. He regained flesh and strength at Hastings, the bronze-like hue varied in depth, his spirits were less depressed, and he resumed very active duties, as a coroner, and in his calling as a surgeon; but he sank at last under some acute affection of his larynx.

The supra-renal capsules were found large, and infiltrated with crude and suppurating tubercular matter. In the year 1847 Mr. S. was near dying from an abscess in the loin. From the cicatrix of this abscess a closed sinus was distinctly traced to the right capsule.

This case was remarkable for its duration, and for the energy which the patient displayed even in a late stage of the disease, and between occasional periods of most distressing prostration, palpitation, and sickness. During his last fatal attack it was noticed that a very foetid smell proceeded from his body.

Casting my memory backwards, I cannot but suspect that similar instances, transiently seen among my home patients, may heretofore have been erroneously set down as obscure cases of jaundice.

It would be idle to speak to you of any *cure* for this disorder. A strengthening plan, both of diet and of drugs, would naturally suggest itself. Mr. S. believed that he improved greatly for a time under quinine, and Margate ale. The pallor and the feebleness invite to the use of steel: but the malady which has, perhaps, occasionally yielded to time, and to the restorative force of nature, has been obedient to no special effort of our art that I am aware of.

I am unwilling to take leave of the cavity of the abdomen, without saying a few words (very few they must be) respecting the various kinds of *tumours* to which it is obnoxious. It may seem strange that the diagnosis of abdominal tumours, which manifest themselves to the touch and to the sight, should be so difficult and puzzling as it often is. I mentioned some reasons for this before: the loose and shifting manner in which some of the viscera of the belly are packed and fastened; their liability to enlarge beyond their natural limits; their accidental dislocations under disease. It would be vain to attempt even a sketch of the infinite variety of these deviations from the healthy state. Every case of abdominal tumour forms a separate object of study, and must be judged of by its proper circumstances. All that I can profess to do, is to offer you some rough hints on this interesting subject.

Some kinds of tumour result from morbid growths; such are all the varieties of cancer: some from the presence and multiplication of parasites; of which we have examples in collections of hydatids: some are produced by the distension of hollow organs; as when concretions, or faecal matters, or gases, lodge in the intestines; or when urine accumulates and is pent up in the bladder: some consist in the mere enlargement of parts.

Let us enumerate the principal of these; that you may know what chiefly to expect.

1. There are, I say, tumours from lodgments in the *bowels*; and these are more hopeful than most kinds of abdominal tumours. Sometimes the stomach, or some part of the intestinal canal, is distended in consequence of a mechanical impediment to the course of its contents: and this impediment may be invincible.

2. *Ovarian* tumours are very common. Of these I spoke at some length in a former lecture.

3. The *liver* is very liable to enlargement: either from simple congestion of blood; or from the interstitial deposit of adipous or of lardaceous matter; or from the intrusion of malignant growth; or from colonies of hydatids.

4. So also the *spleen* swells, from fulness of blood, or from specific deposits in its substance.

5. The *kidneys* sometimes attain a vast size; being occupied by malignant disease, or swollen by pus, or by urine, that finds no vent.

6. Enlargements of the *mesenteric glands*; cancerous degeneration of the peritoneum, especially where it forms the *omentum*; tumours connected with the *uterus*; aneurisms of the *aorta*; vast distension of the *gall-bladder*; constitute other species of abdominal swelling, which I simply mention without further comment.

Now our judgment of the character of a given tumour is naturally influenced by its *place*. In the right hypochondrium, we suspect the liver; in the left, the spleen; in the epigastric region, the stomach; in the hypogastric, the womb, or the bladder; in either flank, an ovary, or perhaps a kidney; in the track of the colon, we guess at fecal collections.

But sometimes the situation of the tumour fits more than one, or than two, suppositions. Between the ribs and the ilium on the right side we may have an enlarged ovary, a tumid kidney, a distended cæcum. A prominence in the epigastrium may be due to enlargement of the left lobe of the liver, to cancer of the stomach, to an infarcted transverse colon, to a ventral aneurism. Above the pubes, the distended bladder, or the enlarged uterus, may equally project. The sigmoid flexure of the colon loaded with feces, the left kidney exaggerated by disease, a bulky ovary, may either of them occupy the same sinistral space.

Moreover, the colon deviates strangely, and not seldom, from its natural course and position: and the magnified viscera may invade, by their displacement, or by their irregular expansion, the regions that are proper to other organs.

Our conjectures are assisted by the associated symptoms, and by observation of the regular performance, or of the disturbance, of particular functions. Yet here, also, we meet with continual sources of fallacy. Pressure from a tumour without, as well as infarction within, may impede the passage of alimentary matters through the bowels, of urine through the ureters; and cause, in the one case, flatulence and tormina, in the other, retention or suppression of urine. Growths foreign to the liver may, nevertheless, press upon its excretory ducts, and occasion jaundice. And so of other parts and functions. I mean, that the functions prominently deranged are not always the functions of the part occupied by the tumour, but of organs which are secondarily and accidentally subjected to its disturbing influence. Your sagacity will be abundantly tried in balancing the evidence of different symptoms in these obscure, yet palpable, forms of disease: and after all you will often doubt; and often, when you do not doubt, you will mistake.

Enlargement of the liver may usually be distinguished from other tumours of the right hypochondrium, by *percussion*. Try from the clavicle downwards. At first, you get a hollow sound. Then, a little below the nipple perhaps (for the spot varies much in different subjects) the sound begins to grow dull. If this dulness be traceable, without change or interruption, to the tumour, the inference is strong that the tumour is hepatic. Any other tumour there situate leaves, most commonly, when the patient is recumbent, a palpable sulcus above it; or a space in which the sound, upon percussion, is different from that which is yielded by the liver.

Percussion helps us to discriminate an ovarian from a renal tumour. When the swelling is large, the intestines lie behind the one, in front of the other: and the sound is affected accordingly.

Tumours that are readily *moveable*, are generally intestinal, omental, or ovarian.

A pulsating tumour is not necessarily an aneurism. The healthy artery will lift almost any sort of hard swelling that happens to lie directly over it.

The occurrence of hæmatemesis or of melæna would corroborate your belief that a tumour in the right hypochondrium was hepatic—the left, was splenic.

Even when you are satisfied as to the organ affected, there comes another question, scarcely, in some cases, less difficult than the first—What is the *nature* of the tumour?

Suppose, for the sake of illustration, that your inquiry relates to the liver. If the tumour be large, prominent, smooth, roundish, of slow growth, and the general health be not materially deranged, it is most likely a *hydatid* tumour. If along the edge and upon the surface of the augmented liver, you can feel large inequalities and projections, and if the complexion and general state of the patient are expressive of failing health, the enlargement is, in all probability, *cancerous*; and if there be other

traces of carcinoma in the system, this conclusion becomes almost certain. Small hard irregularities betoken the *hobnail* liver; which is, sooner or later, accompanied by ascites. When, without pain or jaundice, the liver of a phthisical patient transgresses its natural boundaries, it is, presumably, a *fatty* liver, or a *waxy* liver.

By applying a similar method of investigation to other ventral enlargements, you may frequently hit the right scent, and trace the mischief to its true source. To treat the subject in detail would require a volume. I may refer you to a series of papers by Dr. Bright, in the *Guy's Hospital Reports*; where you will find a host of examples, and much valuable information, concerning the most common and the most important kinds of "abdominal tumours and intumescence."

LECTURE LXXXI.

Acute Rheumatism; Symptoms; Varieties: Treatment. Chronic Rheumatism; Phenomena; Plan of Cure.

Gout: Description of a Paroxysm; Progress of the Disease; general state of Health in Gouty Persons; Causes of the Disease; Diagnosis between Gout and Rheumatism.

I PROCEED, this afternoon, to the consideration of that very common, very painful, and sometimes very perilous disease, *rheumatism*. There are two species of it, the acute and the chronic. They graduate, however, insensibly into each other; and the chronic is often a sequel of the acute form. Yet this is not necessarily so. Chronic rheumatism occurs in persons who have had no preceding attack of the disorder in its acute stage or degree.

Rheumatism implies inflammation: but, as I mentioned in an early part of the course, it is inflammation of a peculiar or specific kind. In the first place, it is inflammation of a particular tissue—the *fibrous tissue*: and it may therefore manifest itself wherever that tissue is employed in the fabric of the body. No doubt the inflammation does involve other tissues also: but it is always, probably, by extending to them, through what has been called *contiguous sympathy*. Thus we have the *synovial* membrane of a joint inflamed in many cases, the inflammatory action having spread from the fibrous textures around the joint; or, as I formerly pointed out to you more in detail, the serous surface of the pericardium, and the serous surface (or what is analogous to a serous surface) of the inside of the heart, and especially that part of it which is carried over the valves—each and all of these serous membranes are extremely liable to be affected with inflammation in the acute form of rheumatism; but in all of them it is probable that the *fibrous* texture was the first to suffer. The pericardium is, as you know, a fibrous-serous membrane; and fibrous tissue is interposed between the folds of the serous membrane, in the cardiac valves.

Rheumatism, therefore, is essentially inflammation of the fibrous tissue: and it most commonly seizes upon the fibrous parts that lie around the *larger joints*: the ligaments and the tendons: and in respect to this disease you may almost consider the perpetually moving *heart* as one of the large joints. Yet this inflammation, when confined to the fibrous tissues, is not *common* inflammation. At any rate, it does not reckon among its events (as common inflammation does) either the effusion of plastic lymph, or suppuration, or gangrene. If suppuration sometimes occur (and it certainly occurs very rarely) it is because the rheumatismal inflammation has extended to contiguous textures, and then and there has run the ordinary course of inflammation. The *areolar* tissue around a joint may thus inflame and suppurate. The inflammation of the synovial membrane *may* be of sufficient intensity to give rise to the formation of pus. When, however, the inflammation extends to the serous tissues within and

around the heart, the products of the inflammation are just the same as when inflammation of the same textures, of the common kind, is anyhow produced.

Acute rheumatism, then, consists in redness, heat, pain, and swelling (that is to say, in inflammation) of the parts lying around, or entering into the composition of, one or more of the larger joints of the body; generally of several at the same time, or in succession; with a disposition to shift from one joint to another, or to certain internal organs, and especially to the membranes of the heart; and with fever.

This tendency to shift its place — to what is usually called metastasis — is a very remarkable feature of the disease. The inflammation will appear, in one joint, suddenly, and as suddenly subside in another which it previously occupied: and then, perhaps, it will jump back again to its old quarters. In very many instances, however, it invades fresh joints without wholly ceasing, and sometimes even without diminishing at all, in those formerly affected. It may visit in this way every large joint in the body, and even seize upon some of the smaller ones; or it may possess nearly all of them at once. It is most commonly seen to affect the ankles and knees, the knuckles, wrists, and elbows. Less often it is seated in the shoulders also; and in the hips. The joints of the fingers frequently suffer; and I have seen more than one instance in which the joints of the jaws were manifestly implicated in the rheumatic inflammation. But by far the most serious and dangerous leap which the diseased action is apt to take, is to the membranes of the heart.

In truth, acute rheumatism is a blood disease. The circulating blood carries with it a poisonous material, which by virtue of some mutual or elective affinity, falls upon the fibrous tissues in particular, visiting and quitting them with a variableness that resembles caprice, but is ruled, no doubt, by definite laws, to us, as yet, unknown.

I pointed out to you, in a former lecture, the symptoms of rheumatic carditis, its consequences, the various ways in which it connects itself with the joint affection, and the treatment by which we are to attempt to moderate it. I dismiss, therefore, this the most momentous complication of acute rheumatism, from the present discussion; and shall confine myself solely to the disease as it manifests itself externally.

The shifting and migratory inflammation of the textures lying around or composing the larger joints, is ushered and attended with high inflammatory fever; with a remarkably full, bounding pulse; with flushed cheeks; head-ache; profuse, drenching, sour-smelling perspirations, which distress and weaken the patient, but bring no present relief to his pain; with a white-coloured, dirtyish, thick fur on the tongue, which is red, however, at its tip and edges; with turbid acid and urine. But this severe and inflammatory fever (*synocha* Cullen calls it) has no tendency to degenerate into a typhus-like form: and *that* is a striking feature in the complaint. Neither is the intellect affected, except when carditis takes place: and then, as I stated formerly, violent delirium is apt to ensue, misleading the practitioner, drawing his attention away from the chest, where grave and often fatal changes are in progress, and fixing it upon the head, where no inflammation at all exists, but which is disturbed through sympathy with the cardiac disorder. With this exception, we do not find patients in acute rheumatism *delirious*. Throughout all this febrile disturbance, there is no coma, no dulness or bewilderment of aspect, no marked trouble of the stomach or of the bowels, no vomiting, no diarrhoea, no petechiæ, no aphthæ, no sordes about the mouth, all which are of ordinary occurrence in the course of common continued fevers. The producing poisons, different in origin and in kind, differ no less in their effects upon the animal economy.

The joints are exquisitely *tender*, as well as painful. The fibrous tissues, which are endowed with but little sensibility in their sound and healthy state, become acutely painful when occupied by inflammation. The pain is increased by pressure; and therefore by whatever implies pressure; by movements of the joints consequently. The patients are reduced to perfect helplessness by the pain; and lie fixed in one position, from which they are afraid to stir. Their common phrase is, that they have entirely lost the use of their limbs: and this is true enough in fact, but not true in the medical sense of those words. They have not lost the *power* of moving them; there is no *palsy*; but they *dare not* move them, because the effort gives them so much torment. They dread the touch of the physician, the handling of the nurse, the shaking of their bed by the footstep of an approaching friend.

As in other disorders which depend upon the presence of some imported or inbred

poison within the body, so also in this—the constitutional symptoms generally precede the local. Nay, I believe that the fever may sometimes run its whole course without any manifest affection of the joints. I have never seen this; but Dr. Graves declares that he had known several instances of persons who, having undergone attacks of genuine acute rheumatism, did afterwards experience febrile symptoms exactly resembling in character, in intensity, and in duration, those which they had previously suffered, although from first to last not a single joint was inflamed.

Pain in the affected joints is more constant than swelling; and swelling more constant than redness. The swelling differs also in different cases in rather a remarkable manner. In fact, two varieties have been made of acute rheumatism. The distinction was first drawn by Dr. Chambers, at St. George's Hospital; and afterwards made public by Dr. Francis Hawkins in his Gulstonian lectures. The varieties are spoken of under the names of *fibrous* or *diffused* rheumatism; and *synovial* rheumatism. I will briefly state their distinctive characters.

In the one, then, the inflammation commences in the immediate neighbourhood of one of the larger joints: not *in* the joint, but *near* it. It attacks the tendons, fasciæ, ligaments, and possibly also the muscles. At first there is not much redness, or swelling: but after the pain has been of some duration, there is a puffiness around the parts affected, caused apparently by turgescence of the blood-vessels, and at length slight pitting, or *œdema*, may supervene, from effusion into the surrounding areolar tissue; and what redness is present is disposed in streaks, following the course of the tendons.

On the other hand, in the synovial variety, which shows itself more frequently and more plainly in the knee than anywhere else, the pain which marks the onset of the complaint does not last long before some degree of swelling is perceptible, together, in most instances, with slight redness of the skin; and this swelling is not due so much to turgescence of the blood-vessels, or to œdema of the areolar tissue, as to fluid poured into the *cavity* of the joint. And the form and character of the swelling indicate that it results from the fulness and distension of the synovial membrane. It is tight and elastic, and protrudes, as it were, through the spaces that intervene between the tendons and ligaments by which it is in other parts bound down and restrained: and *fluctuation* is often distinctly perceptible in the superficial joints, when both hands are applied to them.

These are the *local* differences between the two forms of the disease. And there are differences equally well marked between the constitutional symptoms that attend them.

It is in that form which *κατ' ἐξοχήν* is called *fibrous* rheumatism, that the inflammatory fever runs so high; that the tongue is so thickly furred; that the round, full, bounding pulse occurs; that the profuse, spontaneous, acid perspirations break out, which exhaust the patient's strength without alleviating his sufferings; that the urine is high-coloured, and deposits a copious sediment like brick-dust.

In the *synovial* form, the fever is either less intense from the beginning, or soon moderates after the joints begin to swell; the tongue is less foul; the patient sweats much less. It is to this form that the term *rheumatic gout* is often applied. And growing experience has led me to believe, that in this popular appellation the real nature of the complaint is most truly expressed. Gout and rheumatism are very similar in kind: and what has been called synovial rheumatism, while it forms a connecting link between the two, and partakes of the characters of both, is more nearly allied to gout than it is to rheumatism.

Conformably with this belief, it has been noticed that the tendency of the inflammation to settle upon the cardiac membranes is much greater in the fibrous than in the synovial disease. This is a most important difference.

I know of no other exciting cause of acute rheumatism than exposure to cold, and especially to cold combined with moisture. And this is the reason why the disease is very common among the poorer classes of society, who are more in the way of that cause, and cannot guard against it so effectually as their wealthier brethren; among whom it is comparatively rare. The cold probably exercises its injurious influence by checking the elimination, through the skin and other emunctories, of the poisonous principle as it forms, and by thus accumulating it in the blood.

This poison in the blood constitutes that predisposition to the disease, without

which it would never occur. In the absence of the poisonous material, no exposure to wet and cold will produce acute rheumatism; but out of its abundant presence acute rheumatism may and often does arise, independently of exposure to atmospheric vicissitudes, or of any other possible exciting cause. We see it break out in hospital patients who have for some time been protected from such exposure. The predisposition appears to be sometimes inherited. Dr. Fuller tells us that he traced this hereditary character in twenty-nine per cent. of the rheumatic patients admitted into St. George's Hospital. The poison itself is probably a product of unhealthy assimilation.

Acute rheumatism is principally a disease of youth: prevailing most, I believe, from the age of puberty to that of thirty-five or forty. I have repeatedly, however, seen it in children; sometimes as early as the third or fourth year: and I stated to you some time ago, that the chance of the joint affection being complicated with rheumatic carditis is the greater in proportion as the patient is the younger. With, perhaps, two exceptions, I never knew the disease occur in an unequivocal form before puberty, without its being attended with inflammation of the lining or of the investing membrane of the heart.

I have already told you what I believe to be the proper plan of treatment to pursue when rheumatic carditis is present: in that case the affection of the limbs is of secondary consequence. But what are we to do when there is no complication of the joint disease; no invasion of any of the viscera?

Why, if you seek for instruction upon this matter in books, or even among practical men, you will meet with a very perplexing diversity of opinion. Apart from the cardiac affection, acute rheumatism has no *danger* about it; and the articular inflammation usually terminates, sooner or later, in recovery, whether the heart be implicated or not. And most persons who have been for any considerable time in practice have their own favourite method of conducting the disorder to its termination. While many employ free blood-letting, and other active antiphlogistic remedies, some, on the contrary, even in the present day, put their trust in bark. Some give large doses of calomel in the outset of the disease, such as half a scruple or a scruple, with or without a grain or two of opium; and they repeat the dose daily, or oftener, with purgatives perhaps intervening, till the urgent symptoms give way; and in this manner I have seen the disease apparently cut short. But I have also known many instances in which the disease was painful, and protracted, and obstinate, although this practice was adopted early and fairly prosecuted. Some physicians, again, give smaller and more frequent doses of calomel and opium: and some think opium alone to be as useful as this combination. Others depend mainly upon colchicum: others upon large and repeated doses of conium: and some attempt the cure of acute rheumatism through sweating the patient by means of guaiacum, and similar stimulant medicines, and a profusion of bed-clothes.

Now you may be sure—when men's opinions concerning the treatment of a disease which is of common occurrence and easy recognition, are thus unsettled and diverse—you may be sure, first, that no specific for that disease has yet been discovered; and secondly, that the disease is not very obedient, or not steadily obedient, to any remedial plan. When first I began to practise, I pleased myself, now and then, with the belief that I had ascertained the best cure for acute rheumatism; so rapidly and decidedly did the disorder recede and cease upon the administration of such or such a remedy. But, on the next trial of it perhaps, my expectations have been miserably disappointed. This marked improvement has happened under the use of colchicum, of conium, of calomel with opium, of alkalies. I did not, in the prosperous cases, mistake spontaneous recovery for cure. The change was too great and immediate, and the instances of success were too numerous, to admit of that explanation. Whether it be (as I suspect), that slighter diagnostic marks have been overlooked, and that sometimes *gout* has in reality been cured under the semblance of rheumatism—whether bodily idiosyncrasies have withstood the influence of remedies—or whether atmospheric agencies have kept up the disorder in spite of proper treatment—I cannot tell: certain it is that we are occasionally baffled, and the patient continues to suffer, notwithstanding the diligent enforcement of all the approved remedies and plans of treatment, one after the other. I am far, however, from thinking that remedies are useless: and I do by no means assent to the dictum of the first Dr.

Warren, who, when asked what was good for acute rheumatism, replied "six weeks."

One principle to be kept in view in these cases may be laid down broadly and decidedly. That practice will be the best which tends most surely to obviate or to lessen the risk of cardiac complication. Now I believe it to be quite true that the plan of treatment which is most calculated to moderate and to bring to an end the uncomplicated disease, is also the plan of treatment which is the best adapted to the paramount object of keeping it uncomplicated.

In the fibrous or genuine form of the disorder, with the bounding pulse, the flushed face, the high inflammatory fever, you may bleed your patients from the arm; especially if they are young and robust. They will bear to lose a large quantity of blood without fainting; and you will always find the blood drawn to be remarkably buffed and cupped. The pain and inflammation are local; but the case is not adopted to local remedies. We might, by cold applications, be able perhaps to subdue the inflammation in a knee or an elbow: but from the migratory character of the disorder, we should incur the risk, by the use of such topical measures, of repelling the poison into the circulating blood, and of thus giving it afterwards a new and a more serious location.

Venesection will, almost always, afford a marked degree of relief to the sufferings of the patient: but the relief will seldom be complete or permanent: and we may sometimes with propriety repeat the bleeding. In very few instances, however, can we hope to extinguish the disease, or to reduce its noxious seminal principle within harmless limits, by blood-letting. And this is one of the cases in which you must not take the state of the blood already drawn as a criterion of the propriety of abstracting more. The blood will remain ready to show the buffy coat long after the use of the lancet has ceased to be available or safe.

The advantages of blood-letting in this complaint are, first, the partial relief which it bestows. It is seldom that the pain is not sensibly mitigated, and the fever calmed, provided the bleeding has been carried to a proper extent, sufficiently soon. Secondly, free and early venesection may sometimes perchance (but not often) cut short the attack. This, however, is a favourable effect which you must not reckon upon, and which I would not advise you to aim at. But, in the third place, blood-letting may be serviceable, by preparing the body for other remedies: for calomel, opium, purgatives, colchicum.

Yet, looking to my own practice in acute rheumatism, I find that although I am in the almost daily habit of treating it—for it is a disorder from which our wards are never perhaps entirely free—I rarely prescribe phlebotomy. Many of the patients come under our care at an advanced period of the disease; when the time for bleeding, with any prospect of benefit, has gone by. Some have been bled before their admission. Others, in whom the rheumatic diathesis has become inveterate, are suffering recurrences of the malady: and neither does the recurrent disease require, nor would the system bear, active and repeated depletion. In many instances, again, the constitution has been battered and broken by a London life: while in others there is no great intensity of febrile disturbance from the first. So that the cases in which I am tempted to employ the lancet are really few and far between. At the same time I can well believe—what I have often heard—that in the country, in young persons of unimpaired vigour, in the first attacks of the disorder, and when the inflammatory symptoms run high, venesection may be of much service in mitigating the sufferings of the patient, and in facilitating his cure.

In all diseases in which pain is a prominent symptom, opium is a most serviceable and precious remedy. In rheumatism pain is a very prominent symptom. It is severe, it is multiplied by its occupancy of many parts at once, it is apt to continue long without any natural pause or abatement, it prevents sleep. You will find it right, and even necessary, to administer opium in almost every case of acute fibrous rheumatism. What should be the dose, and how frequently it should be repeated, must be regulated by its observed effect. Enough must be given to quell, or to assuage the pain. Dr. Corrigan, of Dublin, trusts to opium alone for the cure. He begins with one grain, and repeats that quantity at short intervals, or increases it, until sensible relief is obtained: and he then continues the dose thus reached, up to the time when the disease is manifestly departing. Dr. Corrigan found twelve grains in the twenty-

four hours to be the average requisite amount: but less than even one half of this quantity will generally suffice when other remedies are combined with the opium.

I believe few persons now adopt the plan of forced perspiration for the cure of acute rheumatism. Formerly it was the fashion to give powerful sudorifics: Dover's powder, or antimony, in large doses: and the patient was "accinctus ad sudorem;" covered up in bed between thick blankets, with a hot bottle or brick at his feet. But in the severe, unequivocal fibrous form of rheumatism, the perspiration is profuse without any artificial means being used to excite it, and it is not accompanied by the smallest alleviation of the pain: nay sometimes the patients will tell you that they are worse, in that respect, while the sweating continues.

The preparations of colchicum have sometimes, whether venæsection has been premised or not, an almost magical effect in subduing the disease. Frequently, when most successful (though that is by no means a necessary condition of their success) they exercise some marked influence upon the stomach and bowels. Colchicum is very apt to occasion deadly nausea and vomiting, griping, and diarrhœa, and when these consequences ensue from its use, the inflammation of the joints often subsides entirely. At any rate, if the rheumatism do not give way when the stomach and bowels become thus affected, you may be certain that to push the colchicum further would be useless.

Our wishes, however, and our expectations, from colchicum, are often doomed to be defeated. I believe that in proportion as the synovial symptoms predominate, or mix themselves distinctly with the fibrous—in proportion as the disease approaches in its characters to gout—you may expect to be successful with colchicum. Large doses are not requisite. Twenty minims of the wine or of the tincture may be given every six hours, until some result is obtained. Or a grain of the inspissated juice, or of the acetous extract of colchicum, every four hours. Under this treatment the disease sometimes vanishes within three or four days; the medicine producing sickness and purging, and the rheumatism, or the rheumatic gout, rapidly declining. Occasionally the same favourable event takes place, although there has been no disturbance of the stomach or bowels.

Much and prompt amendment may often be effected, at the outset of the disease, through full and free purging, exacted from day to day by means of calomel and black draughts. From five grains to a scruple of calomel given every night, and followed every morning by a draught of senna and salts, will sometimes obtain and dislodge a vast amount of dark and foul secretions from the liver and bowels, and bring sensible relief to the patient's sufferings. And these doses, adjusted to the particular circumstances of the case, may be repeated for three or four days in succession, provided that they continue to procure dark evacuations, and that their operation is unattended with griping pains, a scalding of the rectum, or the discharge of mucus or of blood.

This practice was first followed and taught by Dr. Chambers; and its beneficial results, when it is beneficial, are so early and so marked, that it has become a common and a favourite practice. It is not, however, always an eligible practice. Persons of a weakly habit of body—persons in whom the disease has already run on for some time—do not bear this course of hard purging. There is also a practical evil in the exaction of four or five motions from the bowels of these rheumatic patients every day; I mean the inconvenience, the increase of pain, and the exposure to cold, occasioned by frequently going to stool; and this is a disadvantage likely to be particularly felt in *hospitals*.

Sometimes it is expedient, in the prosecution of this plan, to combine with the calomel a grain or more of opium; both with the view of quieting pain, and of preventing the calomel from running too rapidly through the bowels. For, besides its operation as a purgative, the full influence of the calomel upon the biliary and intestinal secretions is essential to its curative effect upon the disorder.

Again, there are cases of acute rheumatism which proceed kindly, or yield readily, under moderate doses of calomel and opium, repeated every six or eight hours, with occasional purgatives of the milder kind.

The acid properties of the perspiration, as manifested by its peculiar odour—of the saliva, as tested by litmus paper—of the urine, as shown by its deposits—warrant

the belief that the peculiar poison, which the whole disorder would seem to be an effort to discharge from the blood, is some sort of *acid*. And reason suggests the likelihood of benefit from attempting to facilitate the escape of this poison, to draw it forth from the system, or to neutralize its noxious qualities. Now the bloodletting plan, and the purgative plan, both tend to aid the removal of the poisonous element; but when carried beyond a certain point, or when applied inopportunistly, they bring evils of their own. *Alkaline* drugs would appear to be chemical antagonists of the poison; and they have the further good effect of hastening its ejection through a natural emunctory, inasmuch as they are most of them diuretic. Accordingly the treatment of acute rheumatism by alkalis is also, and long has been, a common and a favourite treatment among physicians. Nitre in large doses has been much used and commended; and the alkaline carbonates are very generally prescribed. The bicarbonate of potash in solution has been largely and fairly tried by Dr. Garrod, who has administered it in the average quantity of two scruples, repeated every two hours, by night and by day, for several days together. Of 51 cases so treated, the average duration under treatment was between six and seven days, and the average duration of the whole disease was between thirteen and fourteen days. The medicine soon rendered the urine alkaline. It had no injurious influence on the bowels, or on the bladder. It seemed rapidly to calm the pulse, and to allay the febrile heat. In no instance did any affection of the heart arise after the patient had been forty-eight hours under its influence.

It is an additional recommendation of alkaline remedies in this disease, that they hinder (if we may trust the medical chemists) the deposit of the fibrin of the blood, and may therefore be regarded as so far tending to prevent the formation of endocardial vegetations. The alkaline carbonates may be prescribed in the pleasant form of an effervescing draught, containing an excess of 30 or 40 grains of the carbonate; the dose being repeated every three or four hours while the fever runs high, and the joints are swollen and painful.

The favourite drug of the late Dr. Golding Bird in this and in some other blood-diseases, was the *acetate of potass*. This intelligent physician was himself more than once the subject of acute rheumatism, and his testimony to the efficacy of the acetate is so strong and direct that I cannot forbear quoting it. "I would not willingly (he says) use language which was not completely compatible with experience, but I do not still hesitate to declare that I have never seen the disease in question yield with so much facility to any other remedy. In the severest cases which have been admitted into the hospital under my care, I have seen the cure to be more rapid, and the immediate relief to the patient more marked, by the use of the acetate of potass in quantities of half an ounce, administered, largely diluted, in divided doses, in twenty-four hours, than by any other treatment. In three days I have repeatedly found the exquisite pain of the joints nearly absent, the patient comparatively comfortable, and able to bear with greater ease the helpless state in which the still swollen joints place him. In no case has any ill effect followed the use of the remedy, and whilst the cure has been far more expeditious, the ill effects of colchicum and mercury have been avoided. The pain remarkably and suddenly lessens as soon as the urine becomes alkaline and rises in specific gravity. I can indeed unhappily attest my experience in my own person on the marked alleviation and rapid cessation of the pains of rheumatic fever from the use of the drug, and can gratefully compare its influence with the tedious and painful results of mercurial treatment in a former attack. It is difficult to decide on the comparative immunity from pericarditis in acute rheumatism under particular modes of treatment, but the impression on my mind is very deep, that the tendency to this fearful complication is very much lessened as soon as the urine is rendered alkaline by the acetate." He was in the habit of administering it "in some aromatic water, or what is far more grateful, in plain water, to which a few drops of oil of lemons have been added."

Alkalis, then, or the alkaline salts, are always, in my opinion, fit remedies to be employed in the treatment of acute rheumatism. They may be added largely to the common effervescing saline draught, or they may be simply dissolved in water. Together with them, blood-letting, calomel, purgatives, opium, colchicum, may be variously combined, according to the special circumstances of the case. And what those spe-

cial guiding circumstances are, may be gathered, I trust, from what has been already said respecting these remedies separately.

You will take into account the age and strength of the patient, the period of the disease, the degree of heat and fever, the condition of the pulse, the preponderance of pain, the state of the abdominal secretions. You will bear also in mind that in proportion as the disease assumes the synovial form, the nearer it approximates in its characters to those of gout, the less energetic should be the treatment, and the greater is the likelihood that the preparations of colchicum may prove a useful part of it.

In the tenth and eleventh chapters of his *Lectures on Diseases of the Heart*, Dr. Latham has given a masterly analysis of the purpose, and the effects, of the main remedies for acute rheumatism, both in their separate use, and in their various combinations. He shows how, in one case, venæsection may suffice, by subduing high vascular action and febrile heat; how, in another, opium may solve the disease, by allaying nervous disquietude and pain; how calomel and purgatives, in a third, may cleanse the liver and bowels, and the whole system, of a colluvies of morbid secretions, and so set the sufferer free; and finally, how, in most instances, these three remedial measures may together, or with the occasional interjection of colchicum, achieve the desired end more certainly, and with less of distress, and of expenditure of vital power, than could either of them singly. The skill to deal practically with the varying exigencies of particular cases can be learned nowhere but by the bedside of the sick, under the guidance of competent teachers; but I am sure that a careful study of these admirable lessons will greatly help your choice of remedies, and simplify your conceptions of their proper aim, and contribute to form your judgment of their real efficacy.

Bark used to be highly thought of for the cure of acute rheumatism; but it is beneficial, or safe, only during the convalescence. Where the system has been much reduced and broken by the complaint, or by the discipline the patient has undergone, I think that the decoction of cinchona is of service in fortifying the body against the renewed agency of those causes on which the original attack depended.

Respecting guaiacum, the iodide of potassium, lemon-juice, all of which have been lauded as curative of acute rheumatism, I can tell you nothing from my own experience.

Of external remedies in relief of the articular *pains*, less use has been made than might well have been made. The affected joints rarely sustain any permanent damage. The inflammation is apt to leave them of its own accord, even suddenly, and sometimes as suddenly to return. Leeches, or cupping-glasses, would seem therefore needless, or superfluous. Repulsion of the poison which is making its exit through a joint, back again into the circulating blood by cold applications—to settle possibly upon some internal part, and especially upon the heart—would be positively hazardous. No peril, however, of this sort belongs to *warm* fomentations, which often afford exceeding comfort. Both Dr. Basham and Dr. Fuller have fairly ascertained their safety and their value. Theory suggested that the lurking or emerging poison might be neutralized and rendered innocuous by making these fomentations *alkaline*, and experience appears to confirm this notion. I may quote Dr. Fuller's remarks on this subject. Warm fomentations "soothe the parts, promote perspiration, and thereby favour the elimination of the poison. As the object of the application is to allay the pain, and to counteract the extreme acidity which always accompanies if it be not the cause of rheumatic inflammation, it is manifest, theoretically at least, that an alkaline and opiate solution should prove the most effectual remedy. And so in practice it is found to be. I have tried hot water; I have tried a warm solution of nitrate of potash, as recommended by Dr. Basham; I have tried a simple alkaline solution; and I have tried a mixed alkaline and opiate solution, and the latter has proved far the most powerful in allaying the pain of rheumatic inflammation. In every instance in which it has been employed, the relief obtained has been almost immediate. In order to guard against any source of fallacy, I selected fourteen instances in which corresponding joints were affected, and applied a fomentation of warm water to the one joint, and an alkaline and opiate solution to the other, and almost uniformly the pain and inflammation continued in the former, and speedily subsided in the latter."

The solution usually employed by Dr. Fuller is made by dissolving half an ounce

or six drachms, of the carbonate of potass, or of soda, in nine ounces of hot water, and adding six fluid drachms of Battley's *liquor opii sedativus*. Thin flannel, soaked in this hot lotion, is applied to the inflamed joints, and the whole is wrapped in a covering of thin gutta percha.

Cases frequently occur which are neither absolutely acute, nor absolutely chronic. The inflammation, without being intense in any one joint, lingers in many; and the fever, though not entirely absent, is moderate. The joints are hot and painful, and the skin is dry, and there is some thirst. To this intermediate character the term *sub-acute* is sometimes applied; and it will serve our purpose of distinction.

In this form of the disease, as well as in the more active form, the urine is almost always loaded with lateritious matters, and strongly acid. Looking upon this as an indication of treatment, I have prescribed alkalies, and with much success: the liquor potassæ, for instance, to the amount of a drachm daily, for several days together; keeping the bowels free by laxative medicine. Under this plan, more surely in my experience than under any other, the urine clears, the pain abates, and the joints are liberated. Nitre, I repeat, has been praised of late as being eminently successful against rheumatism: I have not had much opportunity of trying it, but I should think it likely to do good in these subacute cases.

There are two kinds of *chronic* rheumatism: one attended with local heat and swelling, although the constitution at large sympathizes very little or not at all with the topical inflammation; the other characterized rather by coldness and stiffness of the painful joints. In the former of these the pains are increased by pressure, and by movements of the limbs, and by external warmth; the warmth of a bed, for example; and there may be even some slight degree of pyrexia at night. In truth this form of chronic rheumatism claims a near relationship with the acute, *into* which it sometimes passes, and *of* which it is frequently the sequel. It accordingly requires antiphlogistic remedies, only less vigorously applied. It is important for you to know that, in these cases, you may, with less hesitation, less fear I mean of driving the inflammation to some more vital part, apply leeches, and cold washes, to the painful joints. Otherwise, the principle of treatment remains unchanged. The complaint is, however, often obstinate and lingering, and prone to recur. It frequently involves and cripples the smaller joints, especially those of the knuckles and fingers; rendering them knobby, and distorting their form and position. The fingers take a permanently oblique direction, slanting outwards towards the ulna: and Dr. William Budd has drawn attention to the curious fact that the corresponding joints of the two sides of the body are always affected exactly in the same manner. To use a paradoxical expression, the deformity is symmetrical. One crooked joint is just the copy of its fellow. Surely this indicates the *constitutional* origin of the disorder; the infection of the *blood*.

In the other form of chronic rheumatism, what some call *passive*, the remedies that answer best are of a different kind. The pain is alleviated by friction of the joint, and the patients are most comfortable when they are warm in bed, and especially when moderate perspiration is present. They are singularly benefited also by summer weather. Persons who are much troubled by this wearing complaint, and who can afford to live where they please, would do well to take up their residence in a warm climate. Wherever they may be, such patients should be protected against atmospheric vicissitudes by warm clothing: they should be cased in flannel from the neck downwards. Warm bathing is of great service; and especially baths of salt water, of a temperature not less than 100°, that they may act as a stimulus to the cutaneous circulation; warm douches; the vapour bath; or the hot-air bath, of which, as I said before, the patient may receive the benefit as he lies in bed. And to warm clothing, and warm bathing, may be added friction, with some stimulating liniment, and what is called shampooing. It is in these cases that stimulating internal medicines are often of use. Turpentine; some of the animal oils, the cod's-liver oil for instance; guaiacum. Opiates, too, are frequently remedial of the pain; and there can be no better form for their administration than that presented to us in the celebrated Dover's powder; the *pulvis ipecacuanhæ compositus* of the Pharmacopœia.

Whatever its value may be as a remedy for acute rheumatism, the iodide of potassium is certainly available for the relief of the chronic disorder. It is *most* sure to

act beneficially when that fibrous part, the *periosteum*, is principally affected. Its virtues in the case of venereal nodes (*i. e.* in venereal inflammation of the periosteum) were first distinctly pointed out by Dr. Robert Williams, of St. Thomas's Hospital. I believe it is equally effectual, upon whatever cause chronic inflammation of the same part, with nodes and thickenings, may depend.

Some of you probably saw a woman who was lately my patient in the hospital, and who had been worn down to a skeleton by the pain she had endured from chronic periostitis giving rise to nodes, which did not appear to be traceable to syphilis. She had been in the habit of lulling the pain by large opiates at night, and begged to have them after her admission. I gave only the iodide in the ordinary dose (five grains thrice daily), and she slept without opium; and in a week or two lost her nodes, and was perfectly well.

Closely allied to acute *rheumatism*, and yet distinct from it, is the singular disease which in this country is popularly called the *gout*: which Cullen, in the first instance, was disposed to term arthritis; but as arthritis would imply inflammation of all or any of the joints, he afterwards adopted the ancient name of *podagra* (foot-pain).

The same author has given, in his *First Lines*, an excellent account of the phenomena which constitute a paroxysm of gout. It is copied from Sydenham, who drew from nature; for he had himself suffered frequent and severe visitations of the disease during a period of thirty-four years.

The attack begins, most commonly, an hour or two after midnight. The patient, who had gone to bed and to sleep in his usual health, and without suspecting what was about to happen, is awakened by a pain in one of his feet, mostly in the first joint or *ball* of the great toe; but sometimes in other parts of the foot—the heel, the instep, the ankle. With the coming on of this pain there is generally more or less of a cold shivering, which gradually ceases as the pain gets worse, and is succeeded by heat. The pain grows more and more violent and intolerable; and is spoken of by those who suffer it as amounting to torture. It is a grinding, crushing, wrenching pain; or a burning sensation as if a hot iron were pressed into the joint. Some humorous Frenchman described it in this way. "Place (said he) your joint in a vice, and screw the vice up until you can endure it no longer. That may represent rheumatism. Then give the instrument another twist, and you will obtain a notion of the gout." The pain is attended with great restlessness and misery, and exquisite tenderness. The patient cannot bear the weight of the bed-clothes upon the affected limb; nor the jar of a heavy foot-fall in his chamber. In a vain search after comfort he is perpetually shifting his foot from place to place, and from posture to posture. At length, about the ensuing midnight, the pain remits; sometimes gradually, sometimes so suddenly that the patient attributes the relief to his having at last found an easy position. He falls asleep in a gentle perspiration, and when he awakes the next morning he finds the part, which had been so painful, to be red, swelled, tense, and shining, surrounded by more or less œdema, and by turgid veins. The same series of symptoms recur, in a mitigated degree, for some days and nights; and then the disease often goes entirely off, not to return till after a long interval.

As the œdema subsides, and the redness fades, the cuticle of the part that has been inflamed peels off; and this process of desquamation is generally attended with troublesome itching.

Such is a picture of an attack of gout, occurring in an adult subject, for the first time, and in its most regular and genuine form.

Attacks of this kind are preceded, in most instances, by some marked disorder of the functions of the stomach; diminished appetite, flatulence, heartburn, nausea perhaps. And during the paroxysm the urine is often dark-coloured, and acid, and turbid; depositing copiously a pink, or brick-dust sediment. The stools, also, are unnatural; pale, or of a dark green, and very offensive. After the fit, when the complaint has ceased entirely, it generally (says Cullen) "leaves the person in very perfect health; enjoying greater ease and alacrity in the functions of both body and mind than he had for a long time before experienced."

But the disorder, which has thus departed, is very apt—nay, unless extreme care be taken to prevent it, and even in spite of all care, it is almost sure—to return. At first, perhaps, it recurs not oftener than once in every three or four years; but after

some time the intervals are shorter, and the attacks become annual, happening about the same time of the year : afterwards they come twice every year ; and at length they return several times during the course of the autumn, winter, and spring. And as the fits are more frequent, so also are they more protracted, till, in the advanced state of the disease, the patient is hardly ever free from it, except perhaps for two or three months in summer. I do not mean that all this occurs invariably in all cases alike ; but this is a sketch of the general course of the complaint.

There are other phenomena also to be noticed as time advances, and as the disease is repeated. At first, I say, it commonly appears in one foot only ; afterwards every fit includes both feet, the one after the other ; and as the disease continues to recur, it not only attacks both feet in succession, but after having ceased in the foot which was secondly visited, it will return again into the foot first affected, and perhaps a second time also into the other. It passes, too, into other joints, both of the upper and lower extremities, large as well as small ; so that there is scarcely a joint that may not, at one time or another, be seized upon. But as the disease proceeds, and the fits get to be more numerous, the pains are commonly less violent than they were at first ; the patient is, however, more affected with sickness, and suffers more in his general health.

Again, after the earlier attacks, the joints usually recover entirely their former strength and pliancy ; but when the disorder has returned again and again, they are not so readily nor so completely restored to their previous condition, but remain weak and stiff : and sometimes they lose at length their capacity of motion altogether.

Also, in many gouty persons, but not in all, after the disease has frequently recurred, what are called *chalk-stones* form ; concretions, that look exactly like chalk, collect around and outside the joint, filling up the areolar tissue, and lying, in general, immediately beneath the skin. The material of these curious concretions is deposited at first in a half fluid state, and resembles soft mortar ; but the more watery ingredients being afterwards absorbed, it becomes dry and hard. Of course when this stuff is deposited in any quantity on the outside of a joint, it must to some extent limit, or it may entirely prevent, the motion of that joint. The concretions consist mainly of the urate of soda, in a crystalline form. The cartilages *within* the joints are also sometimes encrusted with this salt ; and Dr. William Budd has ascertained that the crystalline deposit is not merely spread superficially over the free surface of the cartilage, but penetrates a little way into its substance. Another, and an odd place, in which these deposits are extremely common in gouty persons, is the cartilage of the external ear.

Gout is a disease that was well known, and well observed, by the ancients. In its genuine form it could neither be overlooked nor mistaken. Many very interesting facts relative to this painful disorder have accordingly been ascertained : and I proceed to notice the chief of these ; but I must do so with as much brevity as I can.

First, then, gout is a *hereditary* disease. I do not mean to say that the disposition to it is always a transmitted disposition ; but that the complaint is much more likely to occur in persons in whose pedigree it can be traced, than it is in other persons. It may, I believe, be generated by certain habits of life ; and, on the other hand, in spite of an inherited predisposition, the disease may be staved off and averted. Let the son of a rich and gouty nobleman change places with the son of a farm-servant, and earn his temperate meal by the daily sweat of his brow ; and the chance of his being visited with gout will be very small. Granting this, we see reason, independent of the general analogy of hereditary disorders, why the gout may be expected sometimes to leap over a generation, just as family likenesses are known to intermit ; while yet the *disposition* may descend to the children of those who, in their own persons, have never suffered the *disease*. Among 522 gouty persons, concerning whom Sir Charles Scudamore had collected information, 332 could trace their disease to the father, mother, grandfather, grandmother, uncle, or aunt. In the remaining 190 the disease was not known to have existed in either upward branch of the family-tree.

2. There is a pattern of body which is believed to be favourable to the acquisition of gout. "It attacks (says Cullen) especially men of robust and large bodies, men of large heads, of full and corpulent habits, and men whose skins are covered with a thicker *rete mucosum*, which gives a coarser surface."

3. Whether, in a given individual, there be an inherited tendency to the disorder

or not, its access is promoted in a remarkable manner by a full and luxurious mode of life, and by sedentary or inactive habits.

4. It is observed of gouty persons, that they are usually subject to nephritic complaints also, to fits of the gravel, to renal and vesical calculi. These disorders of the urinary organs commonly begin to manifest themselves after the gout has plagued the patient for some time. They do not coincide with the paroxysms of gout, but the two happen alternately: or (what is equally expressive of the connexion between the two forms of disease) the children of gouty and nephritic parents inherit often the one or the other of these maladies; but "whichever may have been the principal disease of the parent, some of the children have the one, and some the other. In some of them the nephritic affection occurs alone, without any gout; and this frequently happens in the *female* offspring of gouty ancestors."

The urinary concretions to which gouty people are so subject, and the morbid states of their urine generally, belong to the *lithic* diathesis. Dr. Prout holds that the lithic or uric acid, developed principally during the mal-assimilation of the albuminous textures, may be considered as the characteristic feature in gout." And the chemical composition of the *chalk-stones* which sometimes accompany gout, is in accordance with this statement; and illustrates strongly the connexion between gout and gravel. The so-called chalk-stones are chiefly composed, as I have said before, of uric acid combined with soda; of the urate of soda. Sometimes this very urate of soda, perfectly white, is deposited in large quantities in the *urine*. Dr. Prout says that he has seen it copiously secreted of the consistence of mortar, so as to block up the urethra in its passage outwards. Now this is just the stuff which is deposited around, and sometimes within, the joints, and which hardens and crystallizes as it collects. I may mention here again that many persons have the gout long, and severely, without having any of these concretions. They are incidental to the more chronic forms of the disorder, in which the pain and the fever, though of long duration and frequent recurrence, are slight in degree. The cuticle at length gives way, and the earthy matter lies bare. A namesake of mine, Mr. Henry Watson, describes in the first volume of the *Medical Communications*, the case of a Mr. Middleton, who was accustomed, when playing at cards, to chalk or score the game upon the table with his gouty knuckles.

5. Gout attacks especially the male *sex*. Some few women, however, suffer it, in its regular and decided form; and generally these women are robust and plethoric. Cullen noticed its occurrence in "several females whose menstrual evacuations were more abundant than usual." But in women the disease chiefly happens after the catamenia have ceased to appear. Heberden knew a female who had numerous sores from chalk-stones.

6. Cullen observes that the gout does not usually come on till after the *age* of five and thirty. Heberden, who in his long and extensive practice among the higher classes of society in this town saw as much, perhaps, of this disease as any physician ever did, says that he never met with a case which he could decidedly pronounce to be gout, before the age of puberty. Sir Charles Scudamore has collected a statistical account of 515 examples of gout, in which the period of the first assault had been noted. Of these 142 began between the ages of 20 and 30; 194 between 30 and 40; and 118 between 40 and 50. The greater number, you will observe, was between 30 and 40.

However, I believe that where the inherited disposition is strong, and the habits of living are such as to foster that disposition, gout may show itself, occasionally, even prior to the age of puberty; but this is, certainly, the exception to a very general rule.

7. Gouty persons are subject to various ailments, which spring from the same fountain as the well-marked paroxysm: derangements in the functions of the digestive organs, of the heart and lungs, of the brain and nerves.

The most familiar of these ailments is indigestion, with its various circumstances of impaired appetite, sickness, vomiting, flatulency, heartburn, acid eructations, gastrodynia. Pains and cramps occur in several parts of the trunk, and shoot thence into the upper extremities, and are relieved upon the extrication of wind from the stomach. The bowels are irregular; colicky diarrhoea being sometimes the prevailing fault, but more commonly costiveness. With all this the patient is apt to be exces-

sively dejected and hypochondriacal, morbidly attentive to every bodily feeling, disposed to exaggerate his sufferings, and apprehensive of the worst event.

When the viscera of the thorax are affected, the patient has palpitations, fits of dyspnoea, faintings, or even pangs like those of angina.

In the head occur pain, giddiness, transient affections of the vision and of the hearing, threatenings of palsy and apoplexy.

All these, you may say, are feelings and ailments to which any and all persons are liable. True: but the remarkable peculiarity which connects them, in some men, with gout is this:—that they often all clear away and disappear upon the breaking out of a paroxysm of that disease in the foot.

Hence such symptoms are regarded as indicating one variety of *irregular* gout. Cullen, led by a questionable theory, classes them under the head of *atonic* gout. Sometimes the patient so affected is said to have *lurking* gout; or *masked* gout.

In another variety of irregular gout, the complaint commences, in the ordinary way, in a joint; but the pain and inflammation do not reach the ordinary degree of intensity, or at any rate do not continue for the usual time and then recede gradually in the accustomed manner, but they disappear abruptly and entirely, while symptoms of severe and alarming disorder arise, as suddenly, in some internal part. This Cullen names *retrocedent* gout. It affords an example, as I conceive, of true metastasis. The internal part most commonly attacked is the stomach. It becomes affected with a peculiar feeling of anxiety and distress; with sickness, vomiting, or violent pain which the patient calls spasm, and which perhaps is of a spasmodic character. More rarely the retrocession is to the heart, when syncope or urgent dyspnoea ensue; or to the head, when it may terminate in a stroke of apoplexy, or of paralysis.

In a few cases the disorder, thus alighting on some other part than a joint, is plainly inflammatory. The most common example of this is gouty inflammation of the urethra, with scalding and a puriform discharge; simulating very exactly an attack of gonorrhœa. So also there is a gouty form of ophthalmia, or gout in the eye; gout in the testicle; and a year or two ago an eminent physician of my acquaintance suffered a violent and dangerous attack of what was considered to be gout in the throat. Dr. Cullen speaks of these inflammatory affections under the title of *misplaced* gout: but they may well enough be ranked under one of the preceding heads of masked, or of retrocedent gout.

The disposition to gout may be engendered, and when inherited will infallibly be strengthened and developed, as I have told you already, by certain habits of life: by sensual indulgences, and (but in a less degree, I believe) by want of bodily exercise. Of this we have the strongest negative evidence in the remarkable immunity from the disease enjoyed by the working poor in our rural districts. One never hears of the gout among agricultural labourers. Sir Gilbert Blane states that, during ten years in which he was physician to St. Thomas's Hospital, although in his private practice he reckoned 130 patients who had gout, being about one in twenty-six of the whole number, he had not a single case of it among 2406 patients in the hospital. This I think strange, for in the London hospitals it is not very uncommon for us to meet with gout; but then it is in persons who have lived fully and inactive: in the servants of wealthy families for instance, butlers, coachmen, porters—men who often live more luxuriously, and more idly a great deal, than their masters. And among the rich, those who are most subject to gout are notoriously those who indulge most in what are called the pleasure of the table; who eat largely of animal food, and drink much wine: especially if they are indolent withal. Such men generate for themselves the lithic acid diathesis; and if the gouty tendency happen to have been born with them, they incur the disease, under these habits, with more or less readiness, according to the degree of that innate disposition. Strong exercise certainly *remedies*, in some measure, the evil effects of this mode of life, by promoting the excretions of the body: but gout used to be exceedingly common in the old-fashioned fox-hunter, who “rode hard,” while he also “lived hard.” Mere sedentary habits do not produce gout, as we learn from the comparative exemption of females; and of the poor, who, following sedentary employments, are yet compelled by their poverty, which is so far a blessing to them, to be temperate. Men who eat much meat, generally indulge themselves in drinking also: the two causes go together, and it is diffi-

cult to estimate their separate influence. Butchers, who live fully upon animal diet, are said to be rarely affected with gout, but then they necessarily take a great deal of exercise. It appears that the use of wine, and of malt liquors, fosters the disposition to gout much more than the abuse of distilled spirits. The paucity of gouty patients among the lower classes in this gin-drinking town suffices to show this. I have been told that gout is very little known in Glasgow, where the commercial men live richly, and lead sedentary lives, but do not drink much wine, their favourite beverage being rum-punch, of which they are not at all sparing. Dr. William Budd says that the disease is common among the "ballasters" on the Thames; that, although they are not a numerous body, many are admitted with gout every year into the *Dreadnought*. Now these men being much exposed to inclemencies of weather, and using great bodily exertion, which is attended with profuse sweating and much exhaustion, think themselves warranted in drinking (besides spirits) two or three gallons of porter daily. This shows the effect of malt liquor in producing the gouty habit of body.

On the other hand, the inbred gouty tendency may be so strong, as to be scarcely kept in check by the most abstemious regimen.

A fit of the gout may be *brought on* by various circumstances: in other words, the possible, *exciting* causes of gout are many. A paroxysm has been frequently known to follow immediately upon an unusually severe debauch. Strong mental emotion has sometimes the same consequence, especially emotion of a depressing kind. Excessive fatigue—more particularly fatigue produced by too much walking exercise on any one day—is another exciting cause. And this is unlucky, for it often discourages a patient from again making use of a proper and even a necessary amount of exercise of that kind. Another exciting cause which frequently operates is external injury. The first attack of gout often fixes upon the seat of an old hurt: and a very slight recent injury is sometimes enough to determine a paroxysm—a trifling bruise, or sprain, the pressure of a tight shoe; nay, Dr. Heberden tells us that he verily believes he has seen an attack of gout brought on by the bite of a flea; showing how easily the disease may be excited, when there is a strong predisposition to it. This it is which makes us so often doubt the accuracy of gouty persons, when they tell us that they are lame from a mere sprain.

Dr. Cullen enumerates sundry debilitating circumstances, which, as such, appear to operate in calling into action the gouty disposition. And there can be no doubt that a state of weakness does often favour the eruption of the malady. A friend of my own had lately a most serious attack of continued fever, in the course of which he became hemiplegic, and his life was despaired of. Soon after the fever had left him, and while he was yet extremely feeble, he had three attacks of gout in quick succession.

Pains have been taken by several writers, especially by Heberden, to lay down the distinguishing characters between gout and rheumatism. A first assault of gout can scarcely be confounded with an attack of acute rheumatism. The limitation of the inflammatory redness to one foot, and the restless distress of the gouty patient, contrast strongly with the helpless and motionless condition of the rheumatic, who is pinioned, so to speak, in many limbs. There may be more room for doubt and mistake in the advanced state of gout, when many joints have at length become involved; but even then you may generally decide by inquiring into the history of the patient, and learning the circumstances of his early attacks.

The main points of distinction may be broadly and generally stated thus.

In gout the small joints are first and chiefly affected, especially the joint of the great toe: in rheumatism, the large. The redness of the gouty inflammation is more bright and vivid than that of the rheumatic; and the fluctuations between agony and ease are more complete and more frequent. Gout usually affects one joint only at a time: rheumatism usually several at once. The inflammation in gout is attended with turgid veins, and with more œdema than in rheumatism; and is followed, in the majority of instances, by desquamation and itching, phenomena which we do not notice at the close of rheumatic inflammation. Gout is not attended with those drenching acid sweats which are so characteristic of acute fibrous rheumatism. The gout is decidedly hereditary: rheumatism, though probably hereditary too, is much less distinctly so. The gout occurs rarely or never, whereas rheumatism is not very

uncommon, before the age of puberty. In gout, though many functions suffer, and especially the digestive functions, there is no tendency to carditis: in rheumatism, with far less general disturbance, but more fever, that tendency is very marked. Gout is often, rheumatism is never, associated with chalk-stones: and conformably with this distinction, Dr. Garrod has taught us that uric acid in excess is present in the blood of gouty, and not present in that of rheumatic patients. Gout is the punishment (some have thought it the privilege) of the rich, of persons who live fully, luxuriously, and indolently: rheumatism is most frequently the appanage of the poor, and of those who toil.

LECTURE LXXXII.

Pathology of Gout. Prognosis. Prejudices respecting the disease. Treatment: during the paroxysms; during the intervals. Cutaneous Diseases.

I YESTERDAY described the phenomena of gout, from its primary outbreak to its crippling consummation. I told you what observation has collected concerning its causes; and I pointed out the circumstances which distinguish it from rheumatism. Let us look a little closer into the essence of this curious malady.

The pathology of gout has been the theme of endless controversy. Humoralists and solidists contend alike for the triumph of including the disease within the pale of their respective theories. The very name, *gout*, derived through the French *goutte* from the Latin *gutta*, expresses summarily the doctrine of those who imposed it: and we trace the same, or a similar idea, in the appellation of the kindred disorder, *rheumatism*.

"The opinion (says Cullen) which has generally prevailed, is, that gout depends upon a certain morbid matter, always present in the body; and that this matter, by certain causes thrown upon the joints or other parts, produces the several phenomena of the disease."

You will find this doctrine at the bottom of all Sydenham's speculations on the subject. But Cullen doubted it, and even endeavoured, in an elaborate argument which you may read in his *First Lines*, to disprove it. He held gout to be an affection of the nervous system. I shall not trouble you by detailing his argument, for I consider it an utter failure. I am satisfied that the ancient doctrine, which asserts the *humoral* origin of the disease, is the true one. "Morbific matter" (it may well be called a *poison*) is generated, or detained, under certain circumstances, within the body, and silently collects in the blood; until, after obscure threats, perhaps, and prelusive mutterings, it explodes in the foot; and then the bodily economy, like the atmosphere after a thunder-storm, is, for a while unusually pure and tranquil. To some such conclusion as this the result of all modern research seems clearly and unfailingly to tend. Sir Henry Holland, for example, in his thoughtful and thought-exciting volume, recently published, expresses his belief in "a *materies morbi*, which, whatever its nature, is capable of accumulation in the system, of change of place within the body, and of removal from it." In this, and in several other propositions relative to gout, enunciated in distinct terms by this learned writer, I fully concur. Some speculations still more lately put forth by Dr. William Budd in a communication to the Medical and Chirurgical Society, throw a strong light upon this perplexed subject; and bring the phenomena, not only of gout, but also of many other important complaints, within the operation of one general, comprehensive, and intelligible law. I shall take leave to refer, in a very cursory manner, to some of Dr. Budd's positions.

I need not remind you of the various ways in which extraneous matters find entrance into the blood. Poisons, under their proper shape and name; medicines, which misapplied become poisons; our natural food and drink, which the folly of man

converts into poison; the products or dregs of the secondary assimilative process; these are common sources of impurities, more or less hurtful, which mix and circulate with the vital fluid. Some of these extraneous matters escape harmlessly by one or more of the waste-pipes and emunctories of the body. Some are entangled in its solids: but not indiscriminately; for different substances have their special or their favourite resting-places. All this is well known to persons conversant with toxicological researches.

Now this doctrine, of the elective affinity between certain tissues or parts of the body, and certain morbid principles conveyed to them by the blood, is applied by Dr. Budd to elucidate the very curious fact of the symmetrical local manifestations of many disorders; which disorders are themselves so far general that they derive their origin from the circulating fluids. This symmetry he finds the most exact in chronic constitutional complaints, wherein the local morbid changes are effected in a manner which approximate closely to the processes of healthy nutrition. He shows good reason for believing (what, if the whole theory be true, we should expect) that the same symmetrical phenomena are modified by the *amount* of the poison collected in the system. If there be a certain quantity only, it may settle in some favourite or congenial spot, on one side of the body. If there be more than enough to saturate that part, it goes next to the corresponding spot upon the opposite side; or, perhaps, to an analogous part of the other limb of the same side. If there be more still of the poisonous material, it flies to, and occupies, other parts also. He further shows that the elective affinity is more exclusive, and the bond of union stronger, in respect of some morbid principles, than of others: and in proportion as the affinity is weak, so is the local manifestation of the disease apt to shift, by metastasis, from place to place. When the matter which has thus entered, or combined with, a certain tissue or organ, is anyhow loosened and released from that union, or repelled from the part, it is again set afloat in the blood, to "break out" elsewhere; to tease various organs, perhaps, or to derange the whole economy. The alternation so often to be noticed between certain cutaneous eruptions and internal disorders of function, is a striking and familiar example of this. The eruption afforded presumptive evidence of the detention of some peculiar morbid principle in that part; and the internal affection which succeeds the disappearance of the eruption, denotes that the morbid principle has re-entered the blood. Some of these peccant or poisonous matters fix permanently in the affected spot or spots; and some of them may even be recovered in substance from the dead tissue by chemical means: the poison of lead, for example, from the symmetrically palsied muscles. Others appear to be expended gradually in the part, and so eliminated from the system. Dr. Budd observes, that the regular arrangement of these local tokens, whether they be outward or internal, is disturbed by the presence of fever. Probably the febrile tumult may itself be owing to the quantity of the noxious matter in circulation within the body. He states, also, that, *cæteris paribus*, this morbid matter is most apt to pounce, in the first instance, upon parts which have been previously hurt, or which are mechanically irritated at the time. For which reason a part that has once been affected by it is more likely than other parts to suffer again.

Now, see how thoroughly the ascertained phenomena of gout accord with this theory. Certain habits of life produce fulness, and richness, and impurity of blood; the same habits which breed the lithic acid diathesis. We may even conjecture this acid, or some of its compounds, to be the actual *materies morbi*.¹ Lithates are poured forth with the urine, and sometimes deposited in vast masses around and within the gouty joints. At first, after obscurer intimations of the presence of the poison in the system, it thunders in the foot; and there, perhaps, is all discharged and spent. The chemist Berthollet found that the skin of a part affected with gouty

¹ This conjecture, or rather this inference from the fact stated in the next sentence, has been verified, within the present year (1848) by Dr. Garrod, who succeeds with ease in demonstrating the presence of urate of soda in excess, in the blood of gouty patients.

The same able chemist detects also urate of soda, and urea, in *minute quantities*, in healthy blood. It seems probable that various blood diseases result from different kinds or degrees of imperfection in the eliminating powers of the kidney. Thus perhaps in gout, the uric acid is but scantily excreted—the urea being separated in its natural proportion: while in some cases of "Bright's disease" the reverse of this may occur; the urea escaping with difficulty, and the uric acid coming away with the urine in its ordinary and proper quantity.

inflammation communicated instantly to litmus paper a deep red colour: a large quantity of acid was evidently passing off by exhalation from the inflamed surface. If the poison be too copious to find sufficient vent in one joint, it attacks another, or more than one other. "Quandoque etiam primis morbi diebus, cum materia peccans adeo exuberat ut ei capiendæ pes unus impar sit, utrosque simul pari vehementiâ fatigat: sed ut plurimum pedes successivè, uti diximus, adgreditur." These are Sydenham's words. A chain of repeated paroxysms at length purifies the blood: "donec tandem materiâ peccante prorsus absumtâ, æger pristinam obtinuerit sanitatem." The descent of the disorder upon a particular joint is often determined by a recent blow or sprain, or by the chronic weakness consequent upon an ancient hurt. If the inflammation, after thus settling, be repelled from the foot, the poison, being driven again into the blood, may light upon some vital organ, and place the patient's life in immediate jeopardy. The late Dr. Parry, of Bath, had at one time under his care two patients who had attempted to cut short, or to ease a paroxysm of gout, by plunging the affected foot into cold water. This gave instant relief to the pain, and in both instances the inflammation presently abated; but in both, also, *hemiplegia* occurred a few hours afterwards.

If these views respecting the pathology of gout be true, it can scarcely be doubted that they are applicable, *mutatis mutandis*, to the cognate disorder—acute rheumatism. Dr. Prout, indeed, considered *lactic* acid to have the same relation to rheumatism which *lithic* acid has to gout. Some results of a singularly interesting experimental inquiry which Dr. Richardson is at the present time pursuing, fall in with, and seem to support, this notion. Into the peritoneal cavity of a healthy cat he introduced a solution of lactic acid in water. In two hours the action of the cat's heart became irregular. The next morning the animal was found dead. There was no peritoneal inflammation; but marked endocarditis of the left chambers of the heart. The mitral valve was inflamed and thickened, and covered on its free borders with firm fibrinous deposits. The whole inner surface of the ventricle was highly vascular.

A dog, on which a similar experiment was tried, died in two days. Unequivocal evidence of endocarditis was disclosed upon examination of the heart. The tricuspid valve was swollen to twice its ordinary size. The aortic valves, inflamed and enlarged, presented fibrinous beads along their edges: and the entire endocardial surface was red. The pericardium was simply dry. There was no affection of the joints.

The prognosis of gout may be gathered, without much further suggestion on my part, from what has already been said. The inflammation which befalls the joints has no worse *event* than the thickening, or, perhaps, the chalk-like deposit, which it produces; so that *gout in the extremities* is not a mortal disease. But as it is not always confined to the extremities, the life of a gouty person is justly held to be insecure. "La goutte articulaire (says some French author) est celle dont on est *malade*; et la goutte interne est celle dont on *meurt*." You will find that all insurance companies exact, *cæteris paribus*, a larger premium from those who have had the gout. When it proves fatal, it is by translation of the disease, or rather of the gouty virus, to some vital part; to the stomach, the heart, the lungs, the brain.

As the early visits of gout are generally followed by a striking change for the better in the health and feelings of the patient, it is not to be wondered at that the disease, in its genuine and decided form, should have sometimes been wished for, and even courted. It is commonly thought that a fit of the gout clears the system of all other disorders. It does, indeed, clear it, for the time, of those disorders which resulted from the poison of gout. But this fact has led to great practical mistakes. First, to the error of looking on inertly, and doing nothing to remedy the ailments which are supposed (often very wrongly) to depend upon lurking gout, and to require a fit of the gout for their cure; and, secondly, to the more dangerous experiment of endeavouring to force on such a fit by excess and intemperance. Men forget, or do not know, that the enemy thus reinforced, instead of evacuating the fortress by its outposts, may retreat triumphant into the citadel. To drop metaphor, such a course of living may, indeed, determine an attack of the disease in the extremities, but it involves the fearful peril of some fatal internal seizure. Besides, the benefits expected from external gout belong to its earlier returns alone. The more numerous the fits, the faster does the general health break, and the more stub-

bornly do the associated symptoms cling to the patient: and many persons linger on, martyrs (as they say) to the disease, long after they have ceased to be fit for any of the business of life, or capable of any of its pleasures. Nevertheless, as Heberden observes, "people are neither ashamed nor afraid of it; but solace themselves with the hope that they shall one day have the gout; or, if they have already suffered it, impute all their other ills, not to having had too much of that disease, but to wanting more. The gout, far from being blamed as the cause, is looked up to as the expected deliverer from these evils."

And this mistaken ambition is heightened, no doubt, by the notion, still more absurd and ridiculous, yet very generally prevalent, that it is a *creditable* thing to have the gout: a notion which evidently originated in the fact of its being peculiarly incidental to the wealthy and the great, to men of cultivated minds, and intellectual distinction. Nothing can show more strongly the power of fashion than this desire to be thought to possess, not only the tone and manners of the higher orders of society, not their follies merely and pleasant vices, but their very pains and aches, their bodily imperfections and infirmities. All this is more than sufficiently ludicrous and lamentable: but so it is. Even the philosophic Sydenham consoles himself, under his sufferings from gout, with the reflection that it destroys more rich men than poor — more wise men than fools. "At vero (quod mihi aliisque licet, tam fortunæ quam ingenii dotibus mediocriter instructis, hoc morbo laborantibus solatio esse possit) ita vixerunt atque ita tandem mortem obierunt magni Reges, Dynastæ, exercituum classiumque Duces, Philosophi, alique his similes haud pauci. Verbo dicam, articularis hiece morbus (quod vix de quovis alio adfirmaveris) divites plures interemit quam pauperes, plures sapientes quam fatuos."

The treatment of a gouty patient naturally divides itself into that which is proper during the paroxysm, and that which is proper during the intervals between the paroxysms.

It was maintained by the great physician whose words I have just been quoting, that all artificial evacuations during a fit of the gout are useless or hurtful. He therefore discountenanced blood-letting, purging, and the use of diaphoretic medicines. It was nature's prerogative, he said, to exterminate the peccant matter in her own way; namely, by depositing it in the joints, whence it might be dispersed by insensible transpiration. Evacuant remedies had no other effect than that of recalling into the blood this peccant matter, which nature had already thrust forth to the extremities of the body; whereby it happened that the virus, which should have been eliminated through the joints, fell upon some of the viscera; and so the patient who was in no danger before, became in peril of his life. I mention all this to show you how entirely identical was Sydenham's theory of the gout with that which is now rapidly regaining its lost ground in this country, and which I firmly believe to be the true one. After his time, and upon his authority, the treatment of gout lapsed into an inert expectancy. Even Cullen came to the conclusion that the best thing to be done is commit the sick man to "patience and flannel alone." Here and there, indeed, an advocate of more active measures sprang up. Dr. Rush thought that venæsection was always safe, and generally serviceable: and some persons, following the bad example of the illustrious Harvey, were for extinguishing the inflammation by immersing the affected joint in cold water. Heberden, however, had clearer and juster views upon the subject. He perceived that one reason why physicians did nothing to check the paroxysm was, that they did not know what would check it. He agreed with Cullen in thinking "that no medicine for curing the gout had yet been found;" but he did not partake of his belief in "the impossibility of a cure by medicines."

"The itch (he observes) is supposed to be wholesome in some countries, where it is endemical; and an ague has been considered as a minister of health, whose presence and stay ought by all means to be courted. These opinions are now pretty generally exploded in England; and I hope the time will come when a specific for the gout, as certain as those which have been discovered for these two disorders, will ascertain the equal safety and advantage of immediately stopping its career, and preventing its return."

That time has come: for the colchicum, judiciously employed, may fairly be accounted a specific for the gouty paroxysm. And it is remarkable how long this truth has been seen, though not distinctly or steadily. The hermodactyl of the

ancients is the modern colchicum, and was in high estimation among them for its efficacy in the same forms of disease as are benefited by the colchicum now. It bore, with some, the name of *anima articularum*, the soul of the joints, because (as Quincey states) it prevented "the lodgment of such gritty matter as occasions the gout and arthritic complaints." And I think there can be no doubt that the active principle of the quack medicine so much in vogue for the cure of gout some years ago — I mean the *eau médicinale* — either was the same with that of the meadow-saffron, or was derived from the same family of plants which Decandolle has associated together under the title of "Colchicaceæ."

This drug has certainly the property of easing, in an almost magical manner, the pain of gout. How it operates is not so clear. It is apt to produce nausea, faintness, and diarrhoea; but its curative influence is not conditional upon the occurrence of these symptoms. Sometimes the rapid disappearance of the gouty inflammation is its only perceptible effect. The patient may be in helpless agony, with a tumified red joint, to-day; and walking about, quite well, to-morrow. The colchicum is therefore plainly an anodyne. It also sensibly modifies the condition of the urine, rendering it less acid, and even alkaline; and increasing its quantity. These effects are consequent, I presume, upon changes in the blood wrought by this substance, which thus, and there, proves somehow an antidote to the poison of gout.

There are, as you are aware, various preparations of colchicum in use: the wine of the bulb; the wine of the seeds; the vinegar of colchicum; the acetous extract, made by evaporating that vinegar; the inspissated juice of the plant itself. These are all of them active and valuable medicines; and I should pretend to more knowledge than I possess, if I undertook to tell you which of them is the best.

The mode of administering the remedy, in a regular fit of the gout, is simple enough. For example, you may give forty or sixty minims of the *vinum colchici*, in a saline draught, at bed-time; and half a drachm more, in a warm black dose, the next morning; and you may repeat this sequence if the gout continue. Some persons give twenty minims every six hours, with a drachm of Epsom salts, and a drachm of syrup of poppies in the draught, till the symptoms yield; but I prefer the other plan. In this way the pain is usually calmed, and the swelling reduced in a few days; or even, as if by a charm, in a few hours.

But you must not be satisfied with thus quelling the pain and inflammation. A strong prejudice at one time existed, and still exists among some practitioners, against the colchicum. It was said that it had indeed the power of cutting short the paroxysms, but that it cut short the patient's life also: that they who trusted to it for getting rid of the gout, very seldom lived more than two or three years afterwards. How far this was true I cannot tell; but even admitting it to be true, it was not, I conceive, so much the fault of the medicine as of the patient, or of the physician who did not properly admonish the patient. Men were very glad to get rid of their gout on such easy terms; and they will sometimes say to us now: "I have, as you see, got the gout. This is Monday. I must be in the House of Commons, or attend such and such a meeting, or be at the head of my regiment, on Wednesday: and I expect that you will enable me to do so." Or even sometimes the reason may be that they are engaged to some good dinner two or three days afterwards. Now if patients are content, and are suffered to be content, with expelling the gout from their toe, without observing abstinence more than a day or two, and without any depletion or further medication at the time, we can easily perceive the probability of their being soon attacked by some formidable internal complaint. I apprehend that the proper way to eradicate the lurking residue of the mischief is to continue to give small doses of the colchicum; five minims of the wine, for instance, two or three times a day, for a while. Moreover, purgatives must be employed, if that remedy do not prove aperient. Not violent purgatives, however, which, by weakening the patient, seem to strengthen the power of the gouty virus. With mild cathartics, moderate doses of mercury will generally be advisable, to correct the subsisting disorder of the hepatic functions; and the patient must adopt and pursue abstinent, or at any rate strictly temperate habits, in respect to meat and wine.

And as I think that the dregs, if I may so speak, left behind it by a gouty paroxysm, may be dispersed by the continued use of what, in the usual acceptation of the word, I may call *alterative* doses of colchicum (doses, that is, which produce the desired

purpose gradually, and by insensible operation) so I think it probable that many a fit of the gout might be averted, if the remedy were given in the same way upon the first occurrence of the ordinary premonitory troubles. Many of those troubles never appear to reach the crisis of a fit. There are headaches, attacks of asthma, derangements of the digestive organs, which, occurring in a gouty person, are presumed to be fainter intimations of the presence of the gouty poison in the blood; and if such symptoms yield (as unquestionably they often do) to colchicum, the presumption draws near to proof. Sir H. Holland has well remarked that the meadow-saffron, by its curative effects, may bring sundry maladies, hitherto thought anomalous, under the same law of morbid *gouty* action; just as the Peruvian bark has reduced many complaints, that were previously vague in their nature, within the same category of *aguish* distempers. The same author conjectures that as hypochondriasis is certainly often symptomatic of the gouty poison in the male, so may sometimes the kindred disease, hysteria, be in the female.

Strange stories are recorded—strange, but I believe true—of instantaneous cures of the gout by strong mental emotion; by sudden terror, by violent wrath. Dr. Rush relates an instance of this. An old man who for several years had suffered an annual attack of gout, was lying in one of these paroxysms, when his son, by some accident, drove the shaft of a wagon through the window of his room, with vast noise, and a great smashing and destruction of glass. The old man leaped out of bed, forgetting his crutches; and his wife on entering the apartment, was surprised to see him walking up and down, and exclaiming angrily against the author of the mischief. The late Professor Gregory, of Edinburgh, was in the habit of mentioning another example to the same effect, authenticated to him by a naval surgeon. It occurred in the person of an officer who was freed from an attack of gout, when at sea, by an alarm of fire. Southey again, in his autobiography, speaking of a Mr. Bradford, says, “By that time he had become a victim to the gout. An odd incident happened to him during one of his severe fits, at a time when no persuasions could have induced him to put his feet to the ground, or to believe it possible that he could walk. He was sitting with his legs up, in the full costume of that respectable and orthodox disease, when the ceiling being somewhat old, part of it gave way, and down came a fine nest of rats, old and young together, plump upon him. He had what is called an antipathy to these creatures, and forgetting the gout in the horror which their visitation excited, sprang from his easy chair, and fairly ran down stairs.” Whether this influence of certain states of the mind be rightly alleged or not, it is clear that we can never hope to make any practical use of such a remedy. Indeed, a fit of the gout has been sometimes *brought on* by a mental shock.

The treatment of a gouty patient in the intervals between his attacks of gout, whether regular or irregular, must be chiefly regimenal. The instances are not few of men of good sense, and masters of themselves, who, being warned by one visitation of the gout, have thenceforward resolutely abstained from rich living, and from wine and strong drinks of all kinds, and who have been rewarded for their prudence and self-denial by complete immunity from any return of the disease; or upon whom, at any rate, its future assaults have been few and feeble. On the other hand, many who are liable to gout are taught by sharp experience that a single debauch, a casual glass or two of champagne, even an unusual indulgence in the use of animal food, may suffice to bring their enemy suddenly upon them. I am sure it is worth any *young* man's while, who has had the gout, to become a teetotaller. But the case is different with the *old*, and with those whose health has been broken by the inveterate disease. They must be allowed a certain quantity of their accustomed good cheer, or they become an easier prey to the disease. In such cases you must trim, as well as you can, between opposite dangers; between the Scylla of excess, and the Charybdis of debility.

It is the same with respect to exercise. The young and the hearty can scarcely take too much: the old and the dilapidated, by one act of over-exertion, may incur the penalty of an attack. Although I can do little more than point out general principles for your guidance, I may remark, in reference to exercise, that it should never be *violent*, lest it excite a paroxysm by straining any part, or by causing great fatigue: that it should be *habitual*, daily—not used by fits and starts, and interrupted by long periods of indolence or inaction: and that it should be *active* muscular exercise, as

distinguished from passive exercise or gestation. No mode of exercise is so good as that of walking; and with this may be agreeably and beneficially conjoined riding on horseback.

Early and regular hours are also of much importance; and the avoidance of severe mental application. Sydenham relates that one of the most atrocious attacks of gout he ever underwent was induced by intense thought and study, in the composition of his medical works.

The regimen which I have been recommending may require some power of self-control: yet in reality it implies no severity of mortification. It is perfectly compatible with life's best enjoyments: but to be effectual it must be adopted early, as soon as the disease threatens; and steadily persevered in. Gouty persons, however, do not like these restraints. They are ready to believe that an attack of gout will do them good; or if they are disabused of that error, they are desirous that some medicine may be found which will avert the disease, without their being obliged to forego their accustomed indulgences. "To gratify this desire (says Cullen) physicians have proposed, and to take advantage of it empirics have feigned, many remedies." One of these was the famous Portland powder, of which Heberden remarks: "*Unum est ex multis quæ vocantur remediis specificis, quorum ortum, et splendorem, et occasum vidi.*" It consisted chiefly of bitters and aromatics, and had descended, with some slight variations in its composition, from the times of Galen. Another preventive has recently been praised by Dr. Graves, of Dublin, as being highly serviceable, although (what is a suspicious circumstance) it had, like the Portland powder, fallen out of fashion. These are its ingredients:—Two ounces of orange peel, an ounce of powdered rhubarb, and two ounces of the *pulvis aloës cum canellâ* of the Dublin Pharmacopœia, steeped for a week in a quart of brandy. A table-spoonful of the strained effusion is to be taken, mixed with two or three spoonfuls of water, night and morning. Sir Henry Hallford recommends what I think a better form of prophylactic remedy; viz. a few grains of rhubarb, with double the quantity of magnesia, every day: or some light bitter infusion, with tincture of rhubarb, and about fifteen grains of the bicarbonate of potass.

Now what has been observed respecting preventive remedies of this kind is, not so much that they are inefficacious, as that, *when exclusively trusted to*, they are unsafe. I believe that they are often useful by improving the digestive process; but they are dangerous substitutes for a course of temperance and exercise.

When gout attacks the stomach, either by retrocession or primarily, it often proves rapidly fatal. The gastric affection is not, in general, inflammatory: so we judge, at least, from the *juvantia*. The attack, which consists of violent pain, and a sense of weight or of constriction in the epigastrium, with sickness, vomiting, and a disposition to faint, is often relieved by the employment of stimulants. But such remedies would be likely to aggravate inflammation. It will always be well when symptoms like these occur, to inquire whether any indigestible food has been lately taken: for *gout* (so called) *in the stomach* has sometimes turned out, under the test of an emetic, to have been nothing more than *pork in the stomach*. In the true gouty seizure, antacids will frequently remove the pain; magnesia, in full doses, with rhubarb. If this do not succeed, opium may be resorted to; and if it should be vomited, opiate enemata may be injected. Dr. Heberden thought that opium, and hot spices, were more efficacious and less inconvenient, in these cases, than wine and spirits; but when they fail, a glass of brandy will often allay the pain completely. The mustard poultice, or the turpentine stupe, applied over the epigastrium, has been followed by strikingly good effects. And it is in these emergencies; contingent upon retrocedent or misplaced gout, that we are justified in the endeavour to induce gout in the extremities; not however by internal stimuli, but by enveloping the feet in a mustard poultice, and so enticing or provoking the foe to quit his hold of the interior, and to appear in the outposts. And this expedient should be practised, whatever may be the internal organ upon which the gouty disorder has settled.

Sometimes, but much less commonly, actual gastritis does seem to ensue; and therefore all these cases are anxious and alarming cases. I do not know how the inflammatory affection can be discriminated from the non-inflammatory, unless it be by the occurrence of *tenderness* with the pain, and of fever. You must treat such

cases as you would treat an ordinary case of gastritis, taking no further heed of the gout, except by the application of stimulating cataplasms to the feet.

This concludes what I proposed to say respecting gout and rheumatism : diseases of which the local seat is not exactly external, nor yet do they belong strictly to the interior of the body, except in their accidental complications. They form a link of connexion between the internal and external disorders which fall to the care of the physician ; and I proceed, in the next and last place, to speak of those complaints which either concern the integuments alone ; or which, at any rate, are attended with some notable affection of the skin.

Under the general head of *cutaneous* diseases, are included maladies of very different kinds, and of very different degrees of importance. Some are attended with fever, and run a definite course, and are often dangerous to life. Others are chronic, irregular in their progress, troublesome perhaps, and obstinate, and disfiguring, yet implying no peril to the existence of the patient. Some again are contagious, while many are not so. But before I enter upon any further account of these diseases, I wish to make you acquainted with the names by which the various morbid appearances presented by the skin have been known, since the time of Dr. Willan.

That author—whose works have been augmented by Dr. Bateman, so that perhaps I ought to say *those* authors—divides cutaneous diseases into eight orders, distinguished from each other solely by the appearances upon the skin. I shall omit the last of these orders, the order of *maculæ*, such as freckles and congenital spots and discolorations, because in fact these are not diseases at all.

The first, then, of the appearances described by Dr. Willan are *papulæ* ; pimples. These are little elevations of the cuticle, of a red colour, and solid ; not containing, I mean, any fluid. They are of uncertain duration, and often terminate in scurf. They are supposed to denote inflammation of the papillæ of the skin. If you wish for an example of a papular eruption, look at that of small-pox, at its very earliest outbreak.

The second are *squamæ* ; scales. These are small, hard, thickened, opaque, whitish patches of unhealthy cuticle. The subjacent surface is red. They are well seen in lepra and psoriasis ; and are very common in syphilitic eruptions.

The third are *exanthemata* ; rashes. They consist of superficial red patches on the skin, variously figured, and irregularly diffused, and of all sizes. We have examples of them in some of the most important febrile cutaneous diseases ; scarlet fever, measles, and others. Most commonly they are followed by a peeling off, or desquamation as it is called, of the cuticle.

It is a pity that some other technical denomination was not chosen to express these rashes ; for the term *exanthemata* has long been familiar to the profession as the title of an order of diseases in Cullen's Nosology.

The fourth are *bullæ* ; blebs, miniature blisters. Portions of cuticle, of considerable magnitude, are detached from the subjacent skin, by the interposition of a thin transparent liquid ; with inflammation beneath them. Such occur in erysipelas sometimes, and in pemphigus.

The fifth are *pustulæ* ; pustules. Circumscribed elevations of the cuticle, containing pus, and having red inflamed bases. We have instances of these in common boils ; and in the eruption of small-pox when at its height and maturity. They end in crusts, or scabs.

The sixth are *vesiculæ* ; vesicles. Small elevations of the cuticle, covering a fluid which is generally clear and colourless at first, but becomes afterwards whitish and opaque, or pearly. These are exemplified in the eruption of cow-pox, and in the chicken-pox. You will observe that these vesiculæ differ very little, except in size, from the bullæ or blebs. They often terminate in small scabs.

The seventh are *tubercula* ; tubercles. This also is an unlucky appellation, since the word tubercle is almost appropriated, in the present day, to the scrofulous deposits which infest the lungs and other parts of the body in pulmonary phthisis. However, these cutaneous tubercles are small, hard, superficial tumours, circumscribed and permanent ; or if they suppurate at all, the suppuration in them is partial. Sometimes they slowly ulcerate at the summit. The imperfectly suppurating pustules of the modified small-pox, and certain red spots which are apt to haunt the face, particularly of young persons, furnish examples.

Now it is very convenient, for the purpose of distinguishing different diseases, and of describing them, to know these outward marks when you see them, and to use these names. But they form a very unfit basis for the *classification* of diseases. Maladies may usefully be classed according to their causes; according to their intimate nature; according to the general plan of treatment they may require. But the superficial markings of disease have no definite relation to any of these heads. Besides, a complaint, which is papular to-day may be vesicular to-morrow, and pustular next Saturday. Yet the classification most commonly followed in this country, and in France, is that of Willan and Bateman. Here we find collected under one and the same division, maladies which nature has stamped with broad and obvious marks of distinction; the febrile with the non-febrile; contagious complaints with those which have not that property; ailments that are local and trivial, with diseases of grave import, and deeply-rooted in the system at large. And, on the other hand, distempers which nature has plainly brought together, and connected by striking analogies and resemblances, this methodical arrangement puts widely asunder. I point out, without professing to remedy, these imperfections. I cannot even undertake to give you any full or systematic account of the many disorders comprised in this classification. There is, however, one group, so remarkable, so important, and so highly interesting, that I shall consider it as much in detail as I can. I allude to the group which Cullen comprehends under the title *exanthemata*. With this exception, the advancing year warns me that I must contract what I have to say respecting diseases of the skin within very narrow limits.

LECTURE LXXXIII.

Exanthemata. They are contagious; sometimes epidemic. Period of the eruption; period of incubation; Theory of contagious Febrile Diseases. Continued Fevers.

Of the numerous complaints which are ranked among the diseases of the skin, some, I observed in my last lecture, are attended with fever, and some are not.

Among the former there is a highly interesting group, distinguished by other and more important characters than the mere presence of fever, or peculiar marks upon the skin; characters that enabled Cullen to collect these diseases into a separate order, to which he gave the name of *exanthemata*. This is his description of them. “*Morbi contagiosi, semel tantum in decursu vitæ aliquem afficientes; cum febre incipientes; definito tempore apparent phlogoses, sæpe plures, exiguæ, per cutem sparsæ.*” Contagious diseases; attacking a person once only in his life; beginning with fever. At a definite period small inflammations appear, often numerous, scattered over the skin.—These you will allow, are very remarkable characters. They are not all strictly and universally true, perhaps, of all the forms of disease which I propose to bring now under your notice; but they apply with more or less exactness to the several species of continued fever, to the plague, to small-pox, chicken-pox, measles, scarlet fever, and erysipelas.

Whooping-cough, and the mumps, might be placed in the same catalogue, although in them there is no specific eruption on the skin: but I have already spoken of these two disorders.

Before I take up the consideration of any one of these diseases in particular, I shall premise a brief survey of certain circumstances that are more or less common to them all. The preliminary examination of the *exanthemata* as a class, will give you, I trust, clearer ideas respecting each of them: at any rate it will enable me to dispense with much needless repetition afterwards, and so to save both your time and my own; a matter of some consequence at this advanced period of the session.

In the first place, then, the diseases comprehended in this group are *contagious*

diseases. You will hear persons disputing about the *term* contagion; but such disputes can only arise from the want of a distinct definition of the sense in which it is employed. I understand a disorder to be contagious, when it is in any way *communicable* from one person to another. Some would restrict the word contagion to the cases in which there must be absolute *contact* of the healthy body with the sick body, or with its visible offscourings. When the disease can be conveyed through the medium of the atmosphere, or by means of other intermediate substances called fomites, they would call it *infectious*. And there is no objection to such a distinction, provided it is understood by the reader, or hearer, as well as by the writer or speaker. But since in all cases the disease is conveyed to the person of the recipient by particles of matter proceeding from the person of the sick, and since it seems very unimportant whether those particles are in a solid or in a gaseous form, whether they are imparted by direct contact of the two human bodies, or by being wafted through the air, or carried upon articles of clothing, I shall include both and all these modes of communication under the single term contagion. This, in fact, is what is done in common discourse: all disorders that are "catching," I shall take leave to consider *contagious*.

In this sense I believe that all the diseases just now enumerated *are* contagious; some, no doubt, much more strongly and distinctly so than others. Some of them, indeed, are undeniably contagious. For example, we are privy to, and sometimes willing agents in, the communication of small-pox from one individual to another. There are others concerning the contagious nature of which medical opinion is less settled and unanimous. Many persons deny that continued fevers are communicable from person to person. The evidence from which I conclude that they are so, I will lay before you when I have described those disorders. Even they who admit that they are contagious, are of opinion, many of them, that they sometimes break out spontaneously, without the intervention of any specific virus. No one questions, I fancy, the contagious properties of measles; or of scarlet fever. Whether the plague, and whether erysipelas, be always or ever so produced, has been thought more doubtful. There is every reason for believing that the small-pox, at least, has now no other source than contagion. How it first arose it may be difficult to conjecture; but it is never known to originate spontaneously now-a-days.

Small-pox may in truth be regarded as the *παράδειγμα*, or type, of this group of diseases. I shall therefore take, by anticipation, some well-ascertained facts in its history, for the sake of illustrating the general subject. It is a malady which could scarcely be mistaken for any other; and of which the horrible aspect and fatal tendency are so strongly marked, that its appearance has always been watched with affright by mankind in general, and with intense interest by the philosophic physician.

In the acme of this disease, when it is severe, the whole surface of the body is covered with innumerable little pustules. A minute portion of the matter contained in any one of these pustules, just so much as may suffice to moisten the point of a lancet, is inserted, we will suppose, beneath the cuticle of a healthy man, who has not been near the sick man. What follows this engrafting? Nothing, apparently, for several days: but then febrile symptoms burst forth: and by and by a crop of papulæ appear sprinkled over the skin; and these gradually ripen into pustules precisely resembling that from which the engrafted drop was taken.

The very same phenomena ensue, when a healthy man enters the chamber of a small-pox patient, and breathes, for a certain time, an atmosphere tainted with the emanations from his body.

The points to be noticed here are — 1, the manifest introduction of the virus into the system: 2, its dormancy for a while; in other words, the occurrence of a period of incubation: 3, the breaking out, at length, of a disease identical in its symptoms and in its character with that of the first patient: and 4 (most surprising of all), the enormous increase and multiplication of the poisonous matter.

I say the *history* of small-pox leads to the settled belief that this disorder, of which few persons are not readily susceptible, never occurs, except from contagion. It appears to have been unknown in Europe till the beginning of the eighth century. No mention of any such malady is to be found in the Greek or Roman authors of antiquity. Now whatever may have been the deficiencies of the ancient physicians, they were excellent observers, and capital describers of disease: and it seems to me

scarcely possible that a disorder so diffusive, and marked by characters so definite and conspicuous, should have escaped their notice, or have been *obscurely* portrayed (if known) in their writings.

On the other hand, Mr. Moore, in his learned and interesting *History of Small-pox*, has shown that it prevailed in China and Hindostan from a very early period; even more than 1000 years before the time of our Saviour. That it did not sooner extend westward into Persia, and thence into Greece, may be attributed partly to the horror which the complaint everywhere inspired, and the attempts that were consequently made to check its progress by prohibiting all communication with the sick, partly to the limited intercourse which then took place among the eastern nations, but principally to the peculiar situation of the regions through which the infection was diffused; separated as they were from the rest of the world by immense deserts and by the ocean.

The disease is said to have broken out in *Arabia* at the siege of Mecca, in the year in which Mahomet was born; *i. e.*, in the sixth century. It was widely propagated by his wars, and by those of the Arabs afterwards; and, as I said before, it is generally believed to have first found entrance into Europe at the time of the overthrow of the Gothic monarchy in Spain by the Moors; when to avenge the well-known outrage upon his daughter "Count Julian called the invaders." Whensoever and wheresoever it came, it spread with fearful rapidity and havoc.

What I wish you to remark is this: that while almost all men are prone to take the disorder, large portions of the world have remained for centuries entirely exempt from it, until at length it was imported; and that then it infallibly diffused and established itself in those parts.

Of the more modern history of the disease our knowledge is more precise and sure. It tends uniformly to the same conclusion.

There was no small-pox in the New World before its discovery by Columbus in 1492. In 1517 the disease was imported into St. Domingo. Three years later, in one of the Spanish expeditions from Cuba to Mexico, a negro covered with the pustules of small-pox was landed on the Mexican coast. From him the disease spread with such desolation, that within a very short time, according to Robertson, three millions and a half of people were destroyed in that kingdom alone. Small-pox was introduced into Iceland in 1707, when 16,000 persons were carried off by its ravages; more than a fourth part of the whole population of the island. It reached Greenland still later, appearing there for the first time in 1733, and spreading so fatally as almost to depopulate the country.

Evidence to the same effect is furnished by the results of vaccination in some countries. Take one instance. Vaccination was adopted in Denmark in 1801, and made compulsory in 1810. From that time small-pox disappeared altogether, for fifteen years; whereas during the twelve years preceding the introduction of the preventive disease, upwards of 3000 persons died of the small-pox in Copenhagen alone.

Now it is a very instructive fact respecting this disease, thus rankly contagious, and arising from no other source than contagion, that when it is epidemic in any place, many instances of it occur which we can by no means trace to contagion. Dr. Gregory tells us that of the numerous cases received into the Small-pox Hospital (to which he had long been physician) not one in twenty is capable of being referred to any known source of infection; the disease being ascribed by the patient to cold, fatigue, change of air, or some other innocent circumstance. A prisoner shut up in solitary confinement in the Penitentiary at Millbank was seized with small-pox. Surely this should warn us against inferring of analogous disorders (of continued fevers, for example) that they are necessarily not contagious, because we often fail to discover any way in which the poison could have been applied. If small-pox be produced by contagion alone, and yet the mode in which the contagious matter has been communicated eludes sometimes our closest scrutiny, then we must conclude that the same thing may happen in *other* contagious diseases, of which the contagious property may not be so strong or so obvious. Nay, the argument from analogy will lead us a step further. If once a disorder of this kind is decidedly proved to be sometimes the effect of contagion (and this I think I shall be able to prove to you of continued fevers)—we cannot help entertaining a doubt whether the disorder in question

really ever has any other cause. It is chiefly with a view to the light which they throw upon the obscurer subject of continued fever, that I am thus anticipating some points in the history of the contagious nature of small-pox.

Again, it is noticed of small-pox — and it is the same with the other diseases in this group — that the human body is not always equally susceptible of its contagious influence. Some individuals are more readily affected by it than others: and the same individual more so at one time than at another. There are even some who seem to be incapable of taking the small-pox — just as some, who are quite as much perhaps in the way of it as their neighbours, never become infected with the great pox. Mr. Cross, in his *History of a Variolous Epidemic* which occurred at Norwich in 1819, tells us that of 215 persons who had not been vaccinated, nor had the small-pox, and who were living at Norwich in the same houses with persons ill of that disease, fifteen did not become affected with it; and of these fifteen it was ascertained that ten had escaped under similar circumstances of exposure before. I mentioned, on a former occasion, the fact that a certain dog, in Paris, could not be made to take the contagion of rabies.

It is not at all uncommon for persons to resist the influence of contagion at one period, and to yield to it at another, even when the exposure has appeared to be less complete. Mr. Cross gives a striking example of this. A man, who believed that he had had the small-pox, lived for twelve years as a nurse in an establishment for the reception of persons inoculated with that disorder. At the end of that time he caught the small-pox, which proved fatal to him. Now this might have been, and probably was, as the man supposed, a *second* attack. The late Mr. Lockley told me an instance still more remarkable, as being free from that ambiguity. Nearly the first patient he ever attended, if not the very first, was an old woman, who for years had been in the habit of going from village to village as a nurse; and of nursing a great number of persons labouring under small-pox, which she had never had, and against which she (naturally enough) believed herself proof. At length she was taken ill, and died of small-pox, under Mr. Lockley's observation, at the age of eighty-four. Take one more illustration from another of these disorders. In 1845, a lady with whom I am acquainted went through an attack of measles, that disease being prevalent in the village where she was then residing. She had never had the measles previously: yet she had, long before, personally tended eleven of her twelve children, when ill of the same complaint.

In many cases we can assign no reason for these variations and differences. Age seems to have something to do with them. Infants are but little susceptible of the operation of contagions. Debility, howsoever produced, certainly augments the disposition to be affected by this, as by other causes of disease. The dose and strength of the poison must also be taken into account. As some men can drink a much larger quantity of wine (which is an alcoholic poison) than others, without being intoxicated, and are differently influenced by the same quantity at different times, so is it also with the animal poisons we are now considering; so is it, as I showed you before, with the mineral poison of mercury.

This fluctuating power to resist contagion is most conspicuous, perhaps, when viewed in reference to scarlet fever. After the very earliest periods of life, children catch infectious disorders of all kinds readily enough; more readily than in mature age. The poison of scarlet fever operates with less certainty upon adults than the poisons of small-pox, or of measles. Some medical men escape scarlet fever altogether, although brought much into contact with it by their vocation. I do not know that I ever had scarlet fever.

Another fact, well worthy of notice, is, that small-pox, which is so rankly contagious, and which has at present no other source besides contagion, has its alternate periods of slumber and of activity. This metropolis, and most of our large towns, are never entirely free from it. Scattered cases occur, here and there; and when thus thinly disseminated, the disease is said to be *sporadic*. But there are seasons in which it spreads rapidly and extensively, and assumes the form of an *epidemic* distemper. We are now living (1838) in the midst of one of these epidemics of small-pox. The same is equally true of the other complaints included in this group. Sometimes they are confined to single families; sometimes they pervade a whole district.

Hence you can never infer that any febrile disorder is not contagious, merely

because it prevails epidemically. Many epidemic diseases are not contagious. But the two properties may and do meet in the same malady. They are not to be set in opposition to each other, or regarded as incompatible properties, as they have been by some ingenious writers.

With respect to these epidemic visitations of the exanthemata, certain general facts have been ascertained, very useful and necessary to be known.

1. The strength of the contagion, and the severity and fatality of the disease, vary at different periods of an epidemic. In general the contagion is the most active, and the disorder the most fierce, at the outset of the epidemic. By degrees its violence slackens, and it ceases to spread. This is partly to be explained by the circumstance that the number of persons who are susceptible of the disease, and who have not yet been attacked, are fewer and fewer as the epidemic proceeds. The fire languishes for lack of fuel. But this does not seem to be all. The disease dies out before it has affected all those who are capable of receiving it. We might I think expect, prior to experience, that the earlier cases would usually be the severer; for the weak, who are less liable to struggle with the complaint, and those who, by peculiarity of constitution, are most susceptible of morbid influence, are likely to be the first to suffer.

2. There are great varieties also in the general character of the symptoms that occur in different epidemics of the same disorder. At one time, or in one place, inflammatory symptoms run high; in another place, or at another time, there is an early tendency to debility and sinking. One epidemic is more malign than another. And the practice varies accordingly: so that these are facts of the greatest importance. The prevailing character of the malady is attributed to what is called the *epidemic constitution of the season*. And when we have made out, by observation, what this epidemic constitution is, we have obtained a clue to the proper management of the disorder. Thus continued fever, as it has appeared in London during the last five-and-twenty years, has required and borne far less depletion than it did for the preceding twelve years or more.

You may learn from this how dangerous it is to apply indiscriminately in one epidemic the remedies that may have been found useful in another: and also how foolish and unfair it is to censure the practice employed and recommended by others, merely because it differs from that which we, in other epidemic visitations of the same disorder, have considered fitting and beneficial.

These differences in the prevalence of the disease, and in the character of its symptoms, are not to be explained by any variation in the exciting cause, which is a definite animal poison; nor can they be reasonably ascribed to any appreciable quality or agency of the weather *at the time*. They must depend upon changes that have been slowly wrought upon the human body: and those changes, constituting an acquired predisposition, are probably due to *previous* conditions of the atmosphere, which have exercised a long and gradual influence upon all the individuals of a community.

After stating, in the first clause of his definition, that the exanthemata are contagious diseases, Cullen announces, in the next place, the very curious fact, that they occur but once in a person's life. "*Semel tantum in decursu vitæ aliquem afficientes.*" In this they offer a remarkable contrast to inflammations, which having happened once, are, for that very reason, more apt to happen again.

You will take care to observe, that it is not the mere circumstance of the disease being *contagious* that makes the difference. Those disorders which shield the system against their own future recurrence are, all of them probably, contagious; but the converse does not hold. It is not true that all contagious disorders protect the constitution from their own return. Syphilis, purulent ophthalmia, the itch: these not only do not secure a patient from a repetition of the disease, but perhaps they even render him more liable to it in future.

Neither is the proposition absolutely and invariably true of any disease. Like most general rules, it admits of occasional exceptions. There is not one of the group enumerated in the beginning of this lecture, which has not been known to occur more than once in the same person. Small-pox has, in many instances, affected the same individual twice; even when the first attack had been so severe as to have engraved deep traces of its visit upon the skin. There are a few instances recorded of its *third*

occurrence. It was believed, at one time, that whenever the disease was thus repeated, it was *always*, in the first instance, severe, which, taken in conjunction with the repetition, was thought to indicate a strong natural susceptibility of the disorder. But it has since been noticed that the primary visit is sometimes unusually mild; and this fact (so powerful is the love of theory) has led to the supposition that the first attack was not sufficiently intense to affect the whole mass of blood, and to exhaust the inborn susceptibility. I believe that the two attacks have always been separated by a considerable interval of time. I have myself known two very striking instances, about which there could be no mistake, of the recurrence of measles in several children of the same parents. This proclivity to be again affected by the specific poison seems to run in families. It is less uncommon for scarlet fever to happen a second time in the same individual. No contagious disease therefore furnishes complete future protection against itself. But that this privilege belongs, as a general rule, to small-pox, to measles, and to scarlet fever, there can be no doubt: and the validity of the rule is applicable in the order in which I have here mentioned them. It applies also, though less uniformly, to the other exanthemata. A person who has suffered a well-marked attack of continued fever is far less liable than another, according to my experience, to have that same disease again. The plague is said to afford a temporary safeguard against itself. Very few persons have it twice in the same season. During one epidemic Dr. Russell found that, among 4,400 individuals who underwent the disease, only twenty-eight contracted it a second time. The singular property we are now considering is less plainly visible in erysipelas than in any other malady of the group.

The next clause in Cullen's definition asserts the supervention of the cutaneous marks, in technical language of the *eruption*, at fixed times (*definito tempore*) after the commencement of the general fever. It is clear, therefore, that the cutaneous inflammation cannot be the cause of the fever, but is itself an effect of the contagious poison. Here again we have a point of distinction between the febrile exanthemata and inflammatory fevers, or what Cullen calls the phlegmasiæ, in which the local inflammation commonly precedes the pyrexia.

In reality the circumstance to which I have just referred shows the impropriety of ranking these diseases under the head of *cutaneous* diseases. They would more rightly be called *blood* diseases. The disseminated cutaneous inflammation is a curious and an important circumstance; and it is the symptom which, in the majority of cases, is most *distinctive* of the disorder: but it is not an essential circumstance. Thus, although there are commonly distinct eruptions (quite independent of common petechiæ) in the earlier stages of typhus and of typhoid fever, a lenticular mottling, somewhat like that of measles, or a number of rose-coloured spots, yet these are often absent altogether. And the very same thing happens in other diseases of the same group—diseases which all the world considers and calls cutaneous. A febrile affection, often a fatal one, but attended with no rash, proceeds sometimes from the contagion of scarlet fever. The fever—or rather the patient—is *not* scarlet. The worst form of *cynanche maligna* is of this kind. It is just the same in measles. Authors speak of morbilli sine morbillis; of scarlatina sine scarlatinâ; and even of variolæ sine variolis. Catarrhal symptoms certainly occur sometimes in weakly children who have been fully exposed to the contagion of measles; and the complaint has proved fatal, without there having been any eruption at all. So in the plague: certain cases occur in which there are no buboes nor carbuncles, yet which undoubtedly originated from the common contagion of the prevailing epidemic.

The time at which the eruption comes out differs in the different diseases; and even in the same disease it is subject to occasional variation. The rule with respect to small-pox is, that the spots begin to be visible on the third day; that on which the sickness and fever commenced being reckoned the first. As far as I have observed, this rule is a very constant one. It has been noticed; however, that when the disease is confluent, and therefore severe, it occasionally shows itself in eruption on the second day; and when quite distinct and mild, sometimes not till the fourth.

The regular period for the outbreak of the cutaneous affection in measles is the fourth day: it scarcely ever begins sooner; but it is often later—on the fifth or the sixth day, or even later than that.

Cullen assigns the fourth day for the ordinary appearance of the rash in scarlet fever also. But in this he is decidedly wrong. Sometimes it is, I believe, perceptible on the first day; but its most general period is the second day. In severe and unfavourable cases it may be postponed till the fourth day, or longer.

The eruptions which are tolerably constant in some species of continued fever, observe less exact regularity in the time of their arrival.

The period which intervenes between the reception of the poison, and the super-vention of distinct symptoms—the period during which the virus, though doubtless at work, seems to lie dormant in the system—the period (in one word) of *incubation*, differs also in the different diseases of this group, and varies even in different cases of the same disease. Although no marked changes occur during this period, I believe that some slight deviations from the usual condition and feelings of the patient might often be observed, if they were expected and looked for.

The period of incubation in continued fevers is very uncertain. In a paper upon this curious subject, published in the ninth volume of the *Medical Gazette*, Dr. Gregory states it as his opinion, derived from much inquiry, that ten days is the *average* period. Dr. Haygarth reckoned the minimum period at seven; the maximum at seventy-two days. Sir William Burnett, in his *Account of a Contagious Fever at Chatham*, gives the history of a party of men belonging to the *St. George*, lying at Spithead, who were sent, on the 3d of January, 1811, to assist in navigating the *Dolphin* troop-ship; the crew of which were affected with typhus fever. On the 10th (seven days after exposure) fourteen of these men were sent to the Hospital-ship from the *St. George*, ill with the fever; and many subsequently, up to the 21st of January (the eighteenth from exposure); after which period no cases occurred.

The period of dormancy is more definite, yet still liable to some variation, in most of the other exanthemata. “At the Small-pox Hospital,” says Dr. Gregory, “abundant evidence has been afforded that the period of incubation is usually about twelve days.” It is a remarkable fact that, “when the small-pox is received into the system by inoculation, seven days only elapse between the insertion of the virus and the establishment of the fever.”

Dr. Bateman puts the period of incubation in measles at “from ten to fifteen days.” I have known several instances in which the date of a single short exposure was exactly ascertained, and in which the disease commenced precisely a fortnight afterwards. In scarlet fever the average period is shorter; not more than from four to six days. In the plague it is, I believe, more variable; but generally not exceeding a few days. Dr. Russell tells us that, among those inhabitants of Aleppo who shut themselves up after having been in the way of the contagion, no instance occurred of the appearance of the malady later than the ninth or tenth day.

To say that a febrile disorder is contagious, is the same thing as to say that it is produced by an *animal* poison. Now there are many poisons, very deadly poisons too, which cause diseases that are not communicable from person to person. That particular poison, the *malaria*, is of this kind.

Of the inorganic poisons some are taken into the blood and emerge again from the body, unaltered, with one or more of the ordinary secretions; chiefly with the urine. They may induce changes in the body as they pass; and if these changes be salutary, the substances so inducing them become medicaments. If the changes be destructive or injurious, they are strictly poisons.

Other of the inorganic poisonous substances do not find so ready an exit from the body. They enter into permanent chemical union with the constituent tissues of particular organs. In this way, to use the words of Liebig, they deprive the organs of the principal property which appertains to their vital condition, viz., that of suffering and of effecting transformations. If the organs of which the functions are thus destroyed, are vital organs, these poisons are fatal.

But the animal poisons, those at least with which we are now concerned, act in a totally different manner. They effect changes in the blood, whereby they are themselves abundantly multiplied or reproduced; and the eruptive disease that ensues seems to be the mode provided by nature for the escape or the expulsion of this newly-formed morbid matter from the system. This is the old-fashioned humoral pathology; founded on bold, unproven speculation: and it is most curious to see these very

doctrines, which had sunk into universal discredit and contempt, now again assuming their places, as scientific truths, upon the secure basis of organic chemistry. A wonderful specimen this of the sagacity of the older physicians — of the despised wisdom of our forefathers.

The ancients attributed various disorders to a fermentation of the animal fluids. The cause of fever, according to Hippocrates, was some morbid matter in the blood. This matter, by a process of concoction, was brought, in a certain number of days, into a state in which it was ready for expulsion from the body. It was then thrown off by hæmorrhage, by sweat, by alvine discharges; or deposited upon the surface in the form of abscess, or cutaneous eruption: and these eruptions or evacuations constituted the crisis of each fever.

The doctrine thus enunciated by the father of physic is very nearly the same with that which Liebig is teaching in the nineteenth century. This distinguished chemist ascribes the phenomena which succeed the introduction of certain animal poisons into the blood, to a process resembling fermentation. Let me try, in a few sentences, to expound to you his views on this deeply interesting subject.

You know that the brewer excites the fermentation of his *sweet-wort*, by adding to it a small quantity of *yeast*. Wort is an infusion of malt, and contains sugar and gluten, with other vegetable matters, in solution. Yeast, as I showed you in a former lecture, is a cryptogamous plant or fungus, which undergoes rapid change and development when placed in a solution of sugar. During this development, and apparently in consequence of it, alcoholic fermentation takes place; the elements of the sugar arrange themselves into new and simpler forms: namely, into alcohol and carbonic acid. If there were no gluten in the wort, this would be the whole of the process; during which the added yeast disappears.

But the presence of gluten makes a prodigious difference. The yeast increases enormously at the expense of the gluten, and mingling with the liberated carbonic acid, rises and floats upon the surface of the fermenting liquid. So that, when the process is completed, there has been produced thirty times as much yeast as was originally added to the wort.

Now this, according to Liebig, is but a type of what happens in other fluids under analogous circumstances. He maintains that (I use the words of his translator), “a substance in the act of decomposition, added to a mixed fluid in which its constituents are contained, can reproduce itself in that fluid, exactly in the same manner as new yeast is produced when yeast is added to liquids containing gluten.”

Thus the virus of small-pox (which virus is formed out of the blood) causes such a change within the blood as gives rise to the reproduction of the poison from certain constituents of that fluid: and whilst this process is going on, the natural working of the animal economy is disturbed: the person is ill. The transformation is not arrested until the whole of that ingredient in the blood which is susceptible of the decomposition has undergone the metamorphosis.

Liebig shows that similar processes may take place in mixed fluids (and therefore in the blood) without the regeneration of the added substance: just as the fermentation of a solution of sugar is effected by the addition of yeast, without any reproduction or multiplication of the yeast, if there be no gluten in the saccharine solution. In such cases, the disease, which accompanies, or results from, the transformations that occur in the blood, is not contagious: the poison is not renewed. It is thus that certain *miasms* produce disorders which are not communicable from person to person.

In order, then, that a specific animal poison should effect its own reproduction in the blood, and excite that commotion in the system which results from the formation and expulsion of the new virus, it is requisite that a certain ingredient (analogous to the gluten in the brewer's sweet-wort) should be present in the blood: and this ingredient must have a definite relation to the given poison.

If this ingredient be indispensably necessary to life, the poison, which transforms and destroys it, is inevitably a fatal poison. May not this be the *modus operandi* of the poison of hydrophobia?

Again, if this ingredient be wanting, no reproduction of the poison takes place; nor, of course, any of those symptoms which are consequent upon such reproduction. The poisonous qualities of the animal substance are not developed. It ceases to be a poison.

And this ingredient, if naturally present, is exhausted and destroyed, for a while at least, by the operation of the poison. Hence, for a while at least, the same disease cannot be again produced by the agency of that poison.

Supposing the ingredient to be one which is not essential to the composition of the blood, and to have been thus destroyed or exhausted, it may never be replaced. Or it may be replaced only after a long interval. In some persons it may never exist at all; or it may exist at certain periods only of their lives. It may even be acquired by unnatural or peculiar modes of living.

All this is possible and plausible. A certain number of peculiar substances do certainly exist in the blood of some men which are absent from the blood of others. In childhood and in youth the blood of the same individual contains variable quantities of substances, which are not to be found in it at other periods of life.

This theory of Liebig's offers, then, an intelligible explanation of the curious facts, that certain contagious disorders furnish a protection, temporary or permanent, against their own return; that they have a tolerably definite period of incubation, and run, for the most part, a determinate course; that some persons are less susceptible than others of the influence of these animal poisons, or not susceptible at all; and that the same individual may be capable of taking a contagious disease at one time, and not at another.

Mr. Simon, in his admirable *Lectures on Pathology*, which I earnestly commend to your diligent study—Mr. Simon, while he scouts the notion of any true fermentation in these cases (indeed Liebig could hardly have advanced the example of yeast in wort otherwise than analogically, just as we all speak, now-a-days, of *zymotic* diseases), adopts in full the hypothesis of some material which, pre-existing in the blood, but not being an essential part of it, combines somehow with the exciting virus from without, to cause both the febrile commotion, and, in consequence of the exhaustion of that material, the subsequent immunity from the same disorder. He even indicates the possible identity of the inbred material with certain "wastes of the tissues." "In infancy (he writes), in early age, and till puberty, there are certain waste materials which never afterwards occur: the temporary cartilages have to waste away, the thymus gland has to decay, peculiar changes referable to the sexual system have to be accomplished, and the effete products of these changes have to be eliminated from the system." He points to the fact that "the surfaces and organs most prone to affection in the diseases under consideration are those which are eliminative and defecating: those whose normal products can hardly be retained for any time within the body, much less out of it, without undergoing a foetid decomposition, which sufficiently stamps them with an excrementitious character. Bowels, skin, kidney, tonsils, are the favourite resorts of the several fever poisons, just as they are the surfaces by which naturally the organic waste of the several tissues is eliminated."

This curious subject is looked at in a different light by Mr. Paget. "*The maintenance of morbid structures* is (he says) so familiar a fact, that not only its wonder but its significance seems to be too much overlooked. What we see in scars and thickenings of parts appears to be only an example of a very large class of cases; for this exactness by which the formative process in a part maintains the change once produced by disease, offers a reasonable explanation of the fact that certain diseases usually occur only once in the same body. The poison of small-pox, or of scarlet fever being, for example, once inserted, soon by multiplication or otherwise affects the whole of the blood; alters its whole composition: the disease, in a definite form and order pursues its course; and finally the blood recovers, to all appearance, its former state. Yet it is not as it was: for now the same material, the same variolous poison, will not produce the same effect upon it; and the alteration thus made on the blood or the tissues is made once for all: for commonly, through all after life, the formative process assimilates, and never deviates from, the altered type, but reproduces materials exactly like those altered by the disease: the new ones therefore, like the old, are incapable of alteration by the same poison, and the individual is safe from the danger of infection.

"So it must be, I think, with all diseases which, as a general rule, attack the body only once. The most remarkable instance perhaps is that of the vaccine virus. Inserted once, in almost infinitely small quantity, yet, by multiplying itself, or otherwise affecting all the blood, it may alter it once for all. For, unsearchable as the

changes its effects may be; inconceivably minute as the difference must be between the blood before and the blood after vaccination; yet in some instances that difference is perpetuated; in nearly all it is long retained; by assimilation the altered model is precisely imitated, and all the blood thereafter formed is insusceptible of the action of the vaccine matter.

"But it will be said, the rule fails in every case (and they are not rare) in which a disease that usually occurs but once in the same body occurs twice or more. Nay, but these are examples of the operation of that inner yet not less certain law — that after a part has been changed by disease, it *tends* naturally to regain a perfect state. Most often the complete return is not effected: but sometimes it is, and the part at length becomes what it would have been if disease had never changed it."

Respecting points so interesting and so mysterious, it is scarcely possible to refrain from speculation altogether. I have laid before you some attempts of able and thoughtful men to explain the main facts of the case: namely, the production of the disease by an animal poison; the prodigious increase in quantity of the specific virus within the body during the progress of the malady; and the extinguishment of the susceptibility of its influence in that individual thereafter. Without adopting either theory with implicit credence in its truth, I hold my judgment in suspense, until evidence more convincing shall appear, or until some better theory than either shall be propounded.

It is very certain, and it is not inconsistent with these theories, that the diseases of this group, which are all of them blood-diseases, do often leave permanent traces of their agency upon the general health, even when no local damage is apparent. We hear men say, "I have never been so well since I had the measles, or the typhus fever, in such a year:" and we hear this without much wonder, knowing that local mischief may lurk within, unrevealed by any legible outward signs. It is more surprising, but it is equally true, that the influence is sometimes for good. The system is disencumbered of some previous impediment to its perfect welfare. Of this mysterious ameliorating influence, which is the rarer of the two, I may give you one example.

A servant of a gentleman living in Belgrave Square was constantly ailing and weakly. His master procured for him, without avail, the best medical advice that London could furnish; and at one time put him into St. George's Hospital, with very small benefit or change in his condition. At length the man caught small-pox, and had it most severely and dangerously under the care, in his master's house, of the late Drs. Nevins and Chambers. His life was despaired of. By and by a large mask of scab fell entire from his face, and he recovered, though frightfully seamed by the disorder. From that time for many years he lived in the enjoyment of thorough health and strength, such as he had not known before the attack of small-pox.

The subtle contaminating effluvia which proceed from the bodies of the sick enter the blood of those who catch the disorder, chiefly, I imagine, by being inhaled into the lungs in breathing. The poison may, perhaps, be capable of being spontaneously absorbed through the skin: and upon this supposition oil has been smeared over the surface with the view of shutting out the contagion of the plague. The virus may gain direct entrance into the blood; we know that it sometimes does so, for we ourselves insert it, in inoculation of the small-pox. Dr. Francis Home imparted measles by engrafting some of the blood of a person ill of that complaint; and subsequent attempts to excite the disease in that way have been equally successful. Some rash and unfortunate trials have proved that the plague is communicable by inoculation with matter from the buboes.

Endeavours have been made to estimate the distance to which the influence of different contagious emanations extends. The effluvia in small-pox, measles, and scarlet fever, are the most active; operate, I mean, at the greatest distance. In continued fevers they have a less range; and in the plague the diameter of the infectious circle is probably very small. Some have even supposed that the plague is communicable only by actual contact; but the opposite opinion seems the more likely, namely, that you may *touch* plague patients with impunity (as Bonaparte is known to have done

on a memorable occasion) if you avoid inhaling their breath, or the effluvia proceeding from their bodies.

The most important practical result of the experiments made by Dr. Haygarth and others, for determining the absolute distances to which the power of the contagion extends in different disorders was, that *where ventilation is complete*, in other words, where the gaseous poison is freely diluted with atmospheric air, the sphere of its operation is very limited.

It is an interesting subject of inquiry, worth glancing at for a moment, how far the power of different contagions is modified by differences of temperature. Small-pox is readily propagated either in hot or in cold regions; in Mexico near the Equator, in Greenland towards the Pole. The plague does not spread when the temperature is below 60° or above 90° Fahrenheit. The vaccine matter loses its property of producing the cow-pox if it be exposed for a certain time to extreme cold, or to a heat of 95°. Typhus fever, measles, and scarlet fever, are said to be of rare occurrence in the intertropical regions. Dr. Henry has turned these facts to useful account by proposing to decompose and destroy certain contagions lurking in fomites, by the operation of artificial heat.

Having thus pointed out many circumstances of interest, which are common to all, or nearly all, the diseases grouped together under the title of exanthemata, I may now proceed to a more particular account of those diseases in succession. And I shall begin with *continued fevers*; because, although they do not afford the best-marked examples of the collection of symptoms that compose Cullen's definition of the order, yet a right understanding of the practical points concerned in the management of these febrile diseases, will assist us materially towards a just conception of the modifications of treatment that may be required by the rest.

We hear continually, both in and out of the profession, many different species of fever spoken of. By the public, typhus fever, brain fever, bilious, putrid, low, nervous. And systematic writers are to the full as particular; mucous fever, ataxic, adynamic, gastro-enteric, typhoid, and so forth. Now this complexity of nomenclature is puzzling to the student, and misleads the public mind. Friends and parents ask anxiously what sort of fever the patient has: and medical men themselves often find it difficult to answer them. The subject is really more simple than at first it may seem to be; yet it is not quite so simple as I formerly supposed. For a long time I held, in common I believe with most English physicians, that no definite line of genuine distinction could be drawn between the various forms of continued fever met with in this country. I was indeed aware—it was impossible to be long engaged in hospital practice without becoming aware—that different epidemics presented very striking features of diversity and even of contrast—so as to suggest the frequent suspicion that the maladies composing them might be specifically distinct. Still, taking into account their strong general resemblance—finding that these like, yet varying forms were often more or less intermixed—noticing also that other distempers fluctuated in type, that within the last twenty years all acute disorders had assumed, in this town at least, an altered and a more asthenic character—I conceived, and I taught, that the differences to which I have adverted in the aspect and phenomena of continued fever, depended more upon what is called the epidemic constitution,—that is to say, more upon an acquired disposition of the human body produced by some obscure general influence, and therefore affecting the entire London community,—than upon any essential difference in the nature of the disease itself, or in the virus from which (as I believed) it sprang.

But I think so no longer. The Dr. Jenner of our time, with patience and sagacity worthy of the great name he bears, has traced out plain lines of division between two or three forms of continued fever, and especially between two forms which had been chiefly confounded together, and which we now call respectively *typhus* and *typhoid* fever. In the affinity of these names is still implied the similarity of the two disorders; but Dr. Jenner has shown, by evidence which quite satisfies my mind, that they differ notably and constantly in their symptoms and course, in their duration, in their comparative fatality, in the superficial markings which respectively belong to them and which warrant our classing them among the exanthemata, in the internal organic changes with which they are severally attended, and (what is the most im-

portant, the most conclusive, and the most difficult point to determine of all) in their exciting causes. For some reasons it may be regretted that names so much alike should have been given to diseases which are really distinct; yet even this may be not without its advantage, if it keep the attention of the observer on the alert to discriminate between the two kindred disorders. For my own part, I could have wished that typhoid fever had been named, as Dr. William Budd names it, *intestinal fever*.¹

A third form of continued fever, called the *relapsing fever*, is readily distinguishable by well-marked features of its own, when once its separate existence has been realized.

In sketching the main phenomena of continued fever I shall keep in view *typhus* as its typical form—and afterwards point out the characters which chiefly distinguish this from the *typhoid* disorder. In this way I hope to spare you the tiresomeness of listening to a detailed description of each of two diseases, which, after all, have very much in common.

Typhus fever does not not always commence in the same way. It may happen that for several days before the disease assumes its distinct and proper aspect, and before the patient is rendered unable to pursue his usual occupations, he is affected with certain morbid symptoms which may be considered premonitory of the fever; so that it is sometimes difficult to mark the precise beginning of the disease. These preliminary symptoms result apparently from an altered condition of the *nervous system*. The poison in the blood disturbs the functions of animal life before it causes any palpable derangement in the mechanism of the circulation. The expression of the patient's countenance alters; he becomes pale, languid, and abstracted. Those about him observe that he is looking very ill. He is feeble, and easily tired; reluctant to make any exertion of mind or body; listless, and apprehensive often of some impending evil. He loses his appetite; his tongue becomes white and inclined to tremble; his bowels are irregular, often confined, rarely affected with diarrhœa; his senses lose their natural delicacy. He has uneasiness or wandering pains in various parts of the body; and occasionally there is some giddiness: drowsiness perhaps during the day, and unsound and unrefreshing sleep at night. To collect all this into one expressive word, the patient evidently *droops*.

Much more commonly, however, these preliminary movements are altogether wanting: the disease sets in suddenly. Its regular onset is marked, very frequently indeed, by a *shivering fit*. Another common phenomenon at the period of the invasion is severe *headache*; pain or aching across the forehead, rarely in the temples, never at the back of the head. It is apt to begin during the night, or in the early morning. But this is not constantly the case. Sometimes there is a sense of heaviness and vertigo rather than headache. You will perceive also, even when there have been no premonitory circumstances, that symptoms arise, even thus early, which belong to the nervous system, and which denote some disturbance and alteration in the functions of sensation, thought, and voluntary motion. They are comprised under the general phrase "*febrile oppression*," and they are different from what we notice when pyrexia supervenes upon inflammation. You will obtain a clearer notion of what this term, *febrile oppression*, means, by watching at the bed-side of one patient in this disease, than by any description that I can give you. There is great inaptitude for the exertion of the power of thought, or of motion. The expression of the face is dull and heavy, absent, puzzled; its hue thick, and dusky. The patient presents very much the appearance of a person made stupid by drink; and he staggers a little when he attempts to walk. The muscular power is sensibly enfeebled: sometimes the patient will struggle against this; but in a few hours, or in a day or two at furthest, he takes to his bed.

These are the symptoms which mark the outset of the disease we are about to consider. They occur also, more or less distinctly, in the other disorders of the class in which I have placed continued fevers. In the plague, for example. The patients appear like people who are drunk. Now these symptoms result, no doubt, from

¹ It is a bare act of justice to record that so early as the spring of 1840, a paper was read before the Parisian Medical Society, by Dr. Alexander P. Stewart, in which the main distinctions between typhus and typhoid fever were clearly set forth. And in 1842 Dr. Bartlett's excellent book on Fever testifies to the same distinctions, as he had noticed them in the United States.

changes which are going on in the blood, and which make an early and a strong impression upon the nervous system. And there is another circumstance which, when it is observable, denotes a depressed state of the nervous power. Practitioners, sometimes, are in doubt whether the case may not be one of some visceral inflammation: or, perhaps, knowing it to be continued fever, they still think it expedient to *bleed* the patient. Now faintness or actual syncope is much more easily produced by the abstraction of blood, in *continued fevers*, that it is in *inflammations*: and this fact may occasionally be the means of distinguishing between incipient continued fever, attended, for instance, with catarrhal symptoms, and pure incipient pneumonia.

In order the more clearly to portray the course of continued fever, I shall divide it, as others have done, into periods: weekly periods. Not that there is any such period of seven days allotted to particular symptoms: but that in the simplest forms of the disease, when it runs its course most evenly and favourably, and therefore, we may suppose, the most *regularly* also, there is a succession of different *sets* of symptoms, which occupy each *about* that space of time: nearly enough to allow of my taking it as a help to the better *describing* the disease.

Many of the symptoms which occur during the first stage of the disorder—during the first week, we will say—are such as belong to the sanguiferous system. The pulse becomes more frequent than in health, there is increased heat of skin, and thirst; headache, and throbbing of the temples. The pulse varies considerably in different cases. Generally, I say, its frequency augments; but sometimes it is even slower than natural. The acceleration of the pulse is greatest (*cæteris paribus*) in those constitutions which are the most irritable. In young persons, in females, and in weak and delicate males, it will often rise, soon, to 120: while in stronger adults, it does not so early attain its maximum of frequency, and perhaps does not exceed 100 throughout the whole course of the disease. Should the pulse in any instance reach 130 or 140, the disease is severe: and the majority of such patients die. The absolute frequency of the pulse is not, however, of so much importance in typhus fever, as its steadiness. If it shift from one number to another, *that* affords a worse prognostic even than its being very frequent, provided it keeps at the same standard. It is almost always soft, soon becomes weak, and gradually gets weaker. Dr. Jenner affirms that in *typhus* fever, running an uncomplicated course, the pulse rises slowly in frequency to a certain point, preserves that rate of frequency for a variable period, and then as slowly falls: while in *typhoid* fever it rises and falls in a most irregular manner, to-day 120, to-morrow 90, the next day 120—without appreciable cause or consequence. The skin, during this period, is generally hot and dry, and it *feels* to a bystander very hot and *pungent*. The actual heat, however, is not so great as the sensation given to our hand might persuade us it was. Dr. Bateman found that, in a majority of the cases treated by him, the heat, as ascertained by the thermometer, was about 100°; and he never observed it higher than 104°. The thirst is usually troublesome for the first few days. The tongue becomes clammy or dry; sometimes it is clean and smooth; more often furred: its edges and tip will, perhaps, be red, then a white fur will begin, which either covers the central part of the tongue, or is divided by a straight brown streak which occupies its middle portion. This brown streak is often the first step to dryness and blackness of the tongue.

Slight and transient tenderness of the abdomen is not uncommon during the same period. Sometimes the belly is full and resonant, without being sensibly distended; sometimes it is even concave; most frequently of all it retains the natural qualities of health.

There is evidence, frequently, of a slight affection of the membrane lining the air-passages, from nearly the first: some notable quickness of respiration, and some diffused rhonchus and sibilus, audible through the stethoscope.

And among all the indications of increased action in the circulating system, the symptoms that relate to the nervous centres remain perceptible. The aspect of the patient is peculiar: the features are fixed and inexpressive; or expressive merely of apathy and indifference. If spoken to briskly, he responds; and although his sensibility seems blunted, his answers are, as yet, rational, and to the purpose. Delirium does not come on, in general, till towards the end of the first week. The muscular power is greatly depressed. The patient lies on his back, motionless; he sleeps but little, waking often; and the short snatches of repose which he seems to get, are

disturbed, apparently, by uneasy dreams; and he *fancies*, and says perhaps, that he does not sleep at all: or he lies with his eyes open, evidently awake, but insensible to all that is going on around him. To this condition Dr. Jenner applies the term *coma vigil*. It is much more common in typhus than in typhoid fever, if it be not peculiar to the former. Sometimes, even during the first stage of the disorder, the prostration of strength is so great, or the tendency to stupor and indifference is so marked, that the stools are passed under him as he lies in bed, without any apparent endeavour on the part of the patient to prevent it; and without any notice of his wants being made to his nurse. The urine, during the same stage, is scanty, and high-coloured, and ill-smelling often. Towards the very end of the first weekly period, the eruption which is peculiar to typhus fever commonly begins to show itself: but this is sometimes postponed to the next stage; and I shall describe it in connexion with the other symptoms that are apt to occur in the second week of the disorder.

It is seldom, except in very malignant forms of typhus fever, that death takes place during its primary stage.

LECTURE LXXXIV.

Typhus Fever, continued. Phenomena of the second week; Delirium, Mulberry Rash: of the third week; Recovery, or death in the way of Coma, of Apnœa, of Asthenia. Symptoms that precede and usher in those modes of dying. Typhoid Fever; points of distinction between it and Typhus in respect of symptoms, of modes of attack. Rose-coloured spots. Ulcerations of the Intestine.

IN the last lecture I commenced the consideration of that important disease, which has been best known, in this country, under the name of continued fever. I told you that I had been converted from my former belief in the unity of species of continued fever, by the researches of Dr. William Jenner, who has demonstrated the separate identity of at least three species, namely, typhus fever, typhoid fever, and relapsing fever. And I began to draw an outline of the general disease, keeping *typhus* fever before my mind, as its most formidable species. Sometimes this disorder is preceded by symptoms of a slighter disturbance of the system; more frequently it sets in suddenly, in the midst of apparent health. Whatever premonitory symptoms may take place, they indicate some alteration in the functions of the *nervous system*; upon which many pathologists have supposed that the first and most direct impression is made, by the exciting cause of the fever. But the exciting cause, in many cases, probably in all, is a specific poison received into the blood; and all analogy is in favour of the belief that the primary change is wrought upon the *blood* itself. The whole mass of the blood is gradually vitiated; and the first evidence of the circulation of this altered fluid, is depression of the powers and functions of animal life. Among the earlier symptoms of the declared disease, shivering, headache, and febrile oppression, take the lead.

For the convenience of description I divided the course of the disorder into three weekly stages: not that it necessarily *runs* its course in three weeks, but because the sets of symptoms which succeed each other while the disease is in progress, occupy, in the cases which seem to proceed the most regularly, *about* the space of seven days each.

Now the symptoms present during the first week are expressive of disorder both of the sanguiferous and of the nervous system. The patient is hot, flushed perhaps, and thirsty, and he has a frequent and hard pulse. Besides this he manifests indifference, and stupor; his senses are blunted, his intelligence is diminished. His muscular strength is reduced in a remarkable manner; so that he cannot sit up; in many cases he cannot even lie on his side, or turn himself about well in bed, but remains in the supine position; and if he be purged by medicine, or spontaneously, the stools are apt

to pass from him into the bed, without his knowing it, or without his taking any care to prevent it. This, however, is more common, and more marked, in the second period or week; the phenomena of which I next go on to sketch.

The changes that occur are usually the following:—The pulse becomes more frequent, weaker, and more compressible. The tongue grows drier and browner. More sordes, and of a darker colour, accumulate on the teeth and lips: and it is in this period that delirium is most apt to ensue; and that certain *eruptions* are most often observed. But the symptoms that relate to the nervous system are often still the most prominent. The patient generally loses his *headache*. His voluntary movements, however, become very much weakened, and are sometimes exercised irregularly. The posture which the patient in this stage almost always assumes is, I say, indicative of this weakness; he lies on his back, and he *sinks down in the bed*, slips towards the foot of the bed. He is unable to make or bear that degree of voluntary exertion which would be necessary to place him upon his side. Hence we hail it as a good omen—because it is an indication that the patient still retains some strength—if we find him on his side, or even on his back with his knees drawn up. Other proofs of muscular debility, approaching to palsy, are apt to present themselves. The voice becomes feeble; the patient can scarcely utter an audible sound. Perhaps he is unable to swallow. This is a very bad symptom, though it is one that has been recovered from. Sometimes it seems that the power of deglutition is not lost, but the sick man is too listless to try to swallow: or the dry and parched state of his tongue and throat render it difficult and painful for him to attempt to do so. The patient is apt to lie with his mouth open: and breathing thus through the mouth tends to dry the tongue. Hence it is well to desire him to swallow a mouthful or two of water, and so to moisten his tongue, before you decide upon the state of that organ, or upon his facility of deglutition. Often, in bad cases especially, there are little convulsive startings of the tendons, (*subsultus tendinum* is the technical name of the symptom,) and other irregular and involuntary actions of the muscles: tremulous movements, especially of the tongue and of the hands; and sometimes the sick person is unable to put out his tongue at all. There are two symptoms which, in the majority of instances, present themselves most obviously in the second week of the fever, and which deserve your particular attention: I mean delirium, and the eruption which belongs to the disease.

The delirium is peculiar. The patient wanders, at first, in the night only; and the delirium commonly appears on his awaking from disturbed sleep. Sometimes he is desirous of getting up, and talks incessantly and earnestly in a loud voice, and can only be kept in bed by the imposition of some restraint. Usually, however, his rambling is of a tranquil kind, and without agitation. His mind seems elsewhere: he is inattentive to all that passes around him; but he lies still, muttering disjointed words or sentences, like a man talking in his dreams. From this state of *typhomania* the patient may sometimes be roused by loud speaking addressed to him, or by the sight of a strange face; so that though incoherent and delirious just before, he may become collected when his medical attendant enters the room. But he presently relapses. During the delirious state there is a great deficiency of sensation, and insensibility to impressions. The patient is deaf. This deafness you may hear spoken of as being a good omen, or favourable sign; but it is only so by comparison: it indicates a condition of brain less perilous than its opposite, in which the sense of hearing is morbidly acute. Imperfection or loss of vision is much rarer, and much more alarming, than deafness; yet the eye is generally dull—unlike the brilliant eye of acute phrenitis; it corresponds with the expression of the countenance, which is perplexed rather than wild. Sometimes, however, as the disease advances, black spots, like flies on the wing, *muscæ volitantes*, appear before the patient's eyes: in consequence, it is presumed, of partial insensibility of the retina. The patient attempts to grasp or catch these in the air, or to pick them from the bed clothes. This is called *flocitatio*. After these symptoms recovery is not common. The mouth and tongue are dry: yet the patient no longer complains of thirst. The taste, the smell, the sense of touch, are all impaired. One trivial yet expressive mark of this dulness of the senses, mentioned by a recent author, is that the flies crawl, unnoticed, over the patient's face. Even external ulceration may occur, especially about the hips and sacrum, and go on to gangrene, without any complaint of pain from him. He seems

altogether careless about the issue of his disorder. If, at this period of the fever, you ask him how he does, he will probably declare that he is quite well. I have already alluded to the involuntary passage of the fæces: this may depend, in part, especially in the advanced stages of the disorder, upon debility or paralysis of the sphincter muscles. The urine also dribbles away frequently; and these are points which must always be looked after; first, for the sake of keeping the patient as clean and dry as possible, the irritation of the urine and faecal matters tending to produce sloughing ulceration; and, secondly, with the view of preventing the bladder from becoming unduly distended. Retention of urine, and all its bad consequences, may otherwise occur. It is a good general rule, therefore, to examine the hypogastric region every day with the hand: and also to ask to see the urine, not for any purpose of prognosis, but to ascertain that it is regularly discharged.

The eruption which is peculiar to and distinctive of typhus fever, is called by Dr. Jenner the *mulberry rash*. I follow his account of it with perfect confidence in his fidelity as a describer. It commences usually from the fifth to the eighth day of the disease; sometimes later. After the third day of the eruption no fresh spots appear. In this particular it resembles, as we shall see hereafter, the eruption of small-pox. It disappears in the course of the third week of the disorder.

The characters of the rash vary with its age. It is never papular, but consists, at first, of very slightly elevated spots of a dusky pink colour. Each spot is flattened on its surface, irregular in outline, fades insensibly into the hue of the surrounding skin, and disappears completely under the pressure of one's finger. The larger spots, more irregular than the smaller, appear to be formed by the coalescence of two or more of them.

In two or three days these spots undergo a marked change. They are no longer elevated, become darker, dingier, and rather more defined; and now they fade only, without disappearing, under pressure. From this condition the spots, in most instances, grow paler, pass into faintly marked reddish brown stains, and finally disappear. In some others a third stage is reached. The centres of the spots take a dark purple colour, and remain unaltered by pressure, although their circumferences fade; or the entire spots change into true petechiæ — *i. e.*, into spots of a dusky crimson or purple colour, quite flat, with a well defined margin, and unaffected by pressure. These petechial spots result from a minute extravasation of blood beneath the cuticle. They occur most frequently on the back, at the bend of the elbow, and in the groin.

The spots composing this mulberry rash are generally very numerous, set closely together, and sometimes they almost cover the skin. They are usually situated on the trunk and extremities, occasionally on the trunk only, now and then they are seen on the face. Each spot remains visible till the whole rash vanishes. To this rule there is one exception. The eruption sometimes shows itself first on the backs of the hands, and disappears from those parts within twenty-four hours. When numerous, the spots have not all the same depth of colour; some being paler than others, and appearing as if seen beneath the cuticle. Hence the surface has a mottled look. Our wards at the Middlesex Hospital in 1838 were full of this form of fever. Not a case, I believe, presented itself without these spots. We spoke of it familiarly as the *spotted fever*; or (from the resemblance the rash bore to that of measles, hereafter to be described) as the *rubeoloid fever*.

The spots which I have been describing, those at least which had reached their second stage, have been found to remain visible upon the surface of the dead body, when death has ensued before their natural time of disappearance. The petechial spots also are persistent. And after death, as well as during life, the spots on the undermost parts of the body are the darkest in colour. This difference seems to be owing to their depending position.

Finally, with respect to this mulberry rash. Dr. Jenner states that in patients less than 15 years old it is mostly either absent, or pale in hue, and scanty in quantity. And the mortality from typhus at this early age is proportionally trifling, not more than 2 or 3 per cent.: while in persons more than 50 years of age it is about 56 per cent., and in them the rash is always present, and ordinarily dark and abundant.

There is another eruption described by the French as *occurring* in this disease without being *peculiar* to it. In this country it is now rare; but it used, when the hot plan of treatment was in vogue, to be very common indeed here, in various febrile

complaints: and it was, and is, apparently connected with copious sweating. *Sudamina*, the vesicles composing the eruption are called. They are small, hemispherical, transparent elevations of the cuticle, containing a clear watery fluid. The vesicles are from a quarter of a line to half a line in diameter; they have no red bases; and they are so perfectly pellucid, that when you look upon them in a direction perpendicular to the skin on which they stand, they may readily elude observation. Viewed sideways, they present bright surfaces, and look like so many drops of water, and you may feel with your hand that they *roughen* the part affected with them. These sudamina are mostly met with on the thorax, along the sides of the neck, and about the axillæ. By degrees, the limpid fluid disappears, and they shrivel up; the cuticle becomes wrinkled, and dries into a whitish powder.

Occasionally these miliary vesicles are so common as to give a character to an epidemic. Dr. Ormerod has described, in his *Clinical Observations on Continued Fever*, an epidemic of this kind which was prevalent here in the spring of 1847, chiefly among the Irish newly arrived in London. He states that the sudamina presented themselves "irrespective of any tendency to profuse perspiration."

Dr. Jenner is of opinion that age has something to do with the occurrence of these miliary vesicles; and that they are rarely seen on persons who are more than 40 years old. They remain after life has departed.

It is in the course of this second week of the disease that, in typhus fever, death is most apt to take place. Among 25 fatal cases noted by Dr. Jenner, 9 deaths only occurred after the 15th day; not one after the 20th.

As the disorder approaches its fatal termination, symptoms which are not unfitly called *putrid* very often show themselves: a peculiar fœtor is exhaled by the patient's body; his tongue becomes dry, black, and fissured; and he is perhaps unable to protrude it when asked to do so; his teeth are covered with dark sordes; sloughs form from the mere pressure of the bed on which he lies; in extreme cases the toes have mortified; and Dr. Roupell relates one terrible instance in which both legs rotted away to the bones, which it became necessary to saw through: yet this patient recovered.

During the third week of typhus fever, the patient's chance of recovery improves; unless indeed some local mischief which pre-existed, or which has sprung up during the course of the fever, shuts out or obscures this more favourable view.

When the disorder is about to end favourably, the more formidable of the symptoms diminish and abate. The patient begins again to attend to questions that are put to him; the air of stupor which had hung over his countenance clears away; he once more shows an interest in what is going on around him; the temperature of his skin becomes more natural; the tongue moist and cleaner at its edges; and the frequency of the pulse is less. The evacuations from the bowels gradually regain their natural qualities; and the patient is aware when the necessity for passing them, or for emptying his bladder, arrives, and he gives notice, or asks for assistance. Generally, at the same time with these tokens of improvement, the emaciation which has taken place becomes remarkably conspicuous; perhaps it is the more observable, on account of the patient's resuming a more natural expression of countenance.

In many instances, the amendment is so gradual that we can scarcely say when it begins. In other cases the favourable crisis is preceded by an aggravation of most of the former symptoms, and a marked increase of the general distress. This is a very curious circumstance: and it did not escape the notice of our great dramatist.

Before the curing of a strong disease,
Even in the instant of repair and health,
The fit is strongest. Evils that take leave,
In their departure most of all show evil.

Certain evacuations are also sometimes observed to accompany or to be connected with the favourable change; and the most common of these is the evacuation of sweating.

On the other hand, when the disease is about to terminate in death, that event may take place in different ways; in either of those modes, in short, which I took some pains to distinguish in the earlier part of this course of lectures. I told you then that I had been taught the importance of studying the tendency to this or that

mode of dying, in reference especially to fever by Dr. Alison. My own experience has since sufficiently approved to me the wisdom of his teaching. Cullen inculcates the necessity of "obviating the tendency to death." To do so, we must ascertain the direction of that tendency. We do not so much *cure* these exanthematous maladies, as keep our patients alive while they are recovering. If we would prevent their dying, we must know in what manner they are in danger of dying.

The most common mode of death in typhus fever is certainly that of *coma*. The organic life survives the animal life. The muttering, half-conscious, dream-like stupor, from which the patient may be roused for a while, becomes, by degrees, more profound, and death begins at the head. This mode of death, occurring in the second or third week of the fever, is associated frequently, with the symptoms of putrescency already described. But as the stupor deepens, the pulse generally grows weak, and the extremities become cold. So that death does not come purely in the way of coma; but we have a compound of coma and asthenia, in which the coma takes the lead.

Now coma may result from at least two different kinds of cause. One cause is pressure, which is mechanical. Another, which is probably chemical, is the circulation of some noxious or narcotic substance (such as opium) in the blood. And there are, doubtless, many physical conditions of the nervous mass itself which are capable of arresting the cerebral functions, and producing coma. To which kind of cause are we to ascribe the stupor that supervenes during the progress of fever? That is an interesting, and in reference to practice, an important, question.

Physicians have diligently attempted its solution, by examining the dead brain. I cannot tell you how often I have looked, and looked in vain, for some palpable disorganization, or some effusion implying pressure. All who are familiar with the dead-house of a hospital are aware that this fruitless search for some physical explanation of the comatose state, after death by fever, is of very common occurrence.

The unnatural conditions that have been sometimes noted are—slightly diminished consistence of the substance of the brain; congestion of its blood-vessels, marked by red points on its cut surface; undue fulness of the vessels of the dura and pia mater, with, occasionally, thin coagula or films of blood in the cavity of the arachnoid; scanty effusions of thin watery liquid in the lateral ventricles, or in the meshes of the pia mater; and diminished cohesion between the membranes and the surface of the convolutions, so as to admit of their separation with unusual facility. Now to what conclusion do these facts lead us? Why, in the first place, to the conclusion that those pathologists are in error who maintain (as Dr. Clutterbuck did, for whose experience and talents I always entertained a sincere respect), that the essence of continued fever is *inflammation of the brain*. Not only do we fail to discover, in many instances, any traces of inflammation, upon inspecting the dead brain, but we find that, during the life of the patient, measures which would be likely to aggravate any inflammatory mischief—strong stimulants, for example, wine or brandy,—do actually and obviously, in cases innumerable, relieve the comatose symptoms, and benefit the patient. The inference seems unavoidable, that the coma, in such cases, has some other cause than that mechanical pressure which arises sometimes from the effusion of fluid upon the surface of the brain, or within its ventricles; and that other cause is supplied by the poisoned blood. Here again we may adopt the pathology of Shakspeare:

The life of all his blood
Is touched corruptibly: and his pure brain
(Which some suppose the soul's frail dwelling-house)
Doth by the idle comments that it makes
Foretell the ending of mortality.

In some malign epidemics the nervous system is overwhelmed at once, in the very outset, by the force of the poison. The patient becomes stupid or bewildered; his surface is cold, clammy, purplish, and his pulse feeble: the coma rapidly augments, and death may ensue within twenty-four hours. We sometimes see this fearful train of symptoms in small-pox; and still more often and more strikingly in the worst forms of scarlet fever. I believe that in these cases there is no deviation, cognisable by our senses, from the healthy texture and appearance of the parts within the skull.

Nevertheless, there may be, and there not seldom is, in these fevers, actual inflam-

mation of the brain or of its membranes: but this is an incidental complication. We conjecture that, in addition to the influence of the poison upon the nervous system, there may be a low degree of inflammation going on within the head, when we find it externally hot, when the patient has flushed cheeks, and a vascular eye, and complains of dull headache. And there are some instances in which we recognise more distinctly the outward signs of encephalitis—severe pain in the head, high and fierce delirium, intolerance of light and of sound, with much heat of skin, and a hard pulse. When coma succeeds such symptoms as these, we naturally ascribe it, in part at least, to the effects of the inflammation: and rightly, for we find traces of inflammation after death; considerable serous effusion into the cerebral ventricles; shreds of coagulable lymph upon the membranes; and more rarely suppuration. I suspect that genuine encephalitis, which is of course attended with pyrexia, is sometimes mistaken for continued fever with intercurrent inflammation of the brain. Great attention, and some skill and judgment, are required for discriminating those cases of fever in which such inflammation occurs, and for directing the appropriate treatment.

The death in fever by *apnœa* is certainly much rarer than that by coma, yet it is not very unfrequent. It often mingles itself with the death by coma. From the earliest period of the fever we may, in most cases, notice some increased quickness of respiration, which is not entirely owing to the mere fever, or to acceleration of the circulation; for the ear, when applied to the parietes of the chest, discovers rhonchus, and sibilus, at least. Frequently there is considerable dyspnœa for some hours, or for a day or two, before death: and this may be apparent only, in consequence of the stupor; or it may be real, and proceeding either from more or less of pulmonary collapse, or from a low form of pneumonia, which, by interfering with the due arterialization of the blood, may aggravate, or even give rise to the coma: and such pneumonia is apt to be masked by the fever; declaring itself by none of the ordinary symptoms of cough, rust-coloured sputa, or pain in the thorax. The inflammation, thus latent, is discoverable, however, by the sense of hearing.

What account does dissection give us of the condition of the lungs after death from continued fever? Why, the most notable, and probably the commonest morbid state is that kind of condensation which indicates collapse of the lungs; and it is met with chiefly at their posterior part and in its diffused form—sometimes however dispersed through them, and in its lobular form. M. Louis found this change, which he calls carnification, in 19 out of 45 instances of death from typhoid fever. It was observed by Dr. Wm. Gairdner in the lungs of many of those who fell victims to the Edinburgh epidemic of 1847. It occurred in each of the three species, but more rarely in the relapsing than in the typhoid and typhus fevers.

As the bronchial tubes are very apt to be clogged with viscid mucus, while the muscular power of the patient is so much reduced by the disease as greatly to impair the force and effectiveness of the acts of inspiration, you will not be surprised that pulmonary collapse should often accompany the course, and aggravate the danger of continued fever. Another unnatural appearance met with in the pulmonary substance is engorgement: a state similar to that which occurs in the first stage of pneumonia. But here the engorgement is probably in a great measure mechanical, and takes place during the last few days of the patient's life. As the vital powers diminish, the laws which govern the physical world resume their empire. The fluids, and the blood especially, accumulate in the most depending parts of the viscera; and the lower and hindmost portions of the lungs in particular become loaded. But besides this, it is not unusual to find large portions of the lungs in a state of hepatization, and even infiltrated with pus. Less frequently, and in those cases principally in which the putrid symptoms have been most marked, the lung passes into a gangrenous state.

The mode of death by *asthenia* or syncope in typhus fever is not very common *as existing by itself*; but it is *often* combined with one or both of the other two modes. Death beginning at the heart is more frequently seen in the *typhoid* disease; to which I may now turn.

The main features, as I have endeavoured to paint them, are the same, or very similar, in both of these two allied diseases. It remains for me therefore to dwell on those only which chiefly serve to discriminate them.

In the first place, then, typhoid fever commences more often insidiously, and with

premonitory symptoms — more gradually — than typhus. Chomel gives the following comparative account, deduced from the exact observation of 112 patients in this particular. In 73 of these cases the invasion of the disease was sudden, without any warning, in the midst of apparent good health. In 39 there were prelusive circumstances.

The aspect of the patient in typhoid fever, though heavy and oppressed, differs remarkably from that of the sufferer under typhus: its hue is less dusky or muddy-looking, its expression less dull and stupid, more anxious, less apathetic; and sometimes, on the occurrence of delirium, it is even vivacious. The delirium is decidedly more active; and the patients are more disposed to try to get out of bed.

A striking characteristic of typhoid fever, and very important in relation to its treatment, and to an ulcerated condition of the intestines which I shall presently describe, is the prevalence of diarrhœa. Often this is an early symptom; sometimes it is postponed to the latter part of the first or the beginning of the second week. It is either spontaneous, or it continues after the operation of a dose of purgative medicine. Pain in the abdomen frequently precedes and accompanies it. Whenever it occurs, the stools are for the most part, loose and frequent: and either of a dark colour, and fœtid, or of a yellow-ochre appearance, like pea-soup somewhat. If you make pressure upon the abdomen, you will find it unnaturally hard and resisting, as though its walls were made of pasteboard, tympanitic, sometimes very much distended, but, whether large or not, Dr. Jenner states that "its shape is invariably the same, and somewhat peculiar. Its convexity is from side to side, and not from above downward. The patient is never pot-bellied, but tub-shaped; the cause probably being that the flatus occupies the colon, ascending, descending, and transverse." Frequently, uneasiness is manifested when pressure is made on the belly, particularly over the cæcal region: and another symptom, not commonly met with in other diseases, is usually noticeable in the first stage of this, viz., a slight gurgling movement, evidently from the intermixture of liquid and gas within the bowel, which movement becomes audible, or palpable to the hand, upon pressing the same region. This symptom is still more common in the more advanced stages of the disorder. It is of rare occurrence in *typhus* fever.

As the disease proceeds, so does the diarrhœa; from three to six stools, or even more, occurring daily. When they take place involuntarily, when they are passed in the bed without notice on the part of the patient, they add materially to his danger by the irritation and the sores which are apt to result from their contact with the skin. There is seldom much pain of the abdomen now complained of by the patient; but if you make pressure, especially about the situation of the cæcum, you may often remark that he winces, or that a transient expression of suffering passes across his features. The character of the evacuations remains the same, and is almost distinctive of the disease; thin, yellowish, ochrey, like pea-soup. When, in fever, such stools persist day after day, and several of them every day, you may safely infer that there is ulceration of the bowels, although there should be no pain complained of even when the abdomen is pressed.

And the same conclusion will become still more certain when *hæmorrhage* from the bowels occurs, as it is apt to do, in this stage of the fever. It often takes place unexpectedly, sometimes in considerable quantity, and rapidly exhausts the patient; or it recurs at intervals to a smaller amount, wasting his strength as surely, though more slowly. The bleeding is probably owing, in general, to the division or opening of some of the mesenteric veins by the ulcerating process which I shall more fully describe by and by. This is not, however, a necessary consequence of the ulceration; for the vessels are usually obliterated previously to their erosion. Sometimes blood may be thus poured into the bowels without being voided. Andral relates a case in which a man died suddenly and unexpectedly at an advanced period of typhoid fever. Large clots of black blood filled the lower two-thirds of the small intestines, which were crowded with patches of ulceration. No part of the blood had passed the valve of the cæcum.

Hæmorrhage from the bowels may occur in continued fever in another way: in connexion with other putrid symptoms, petechiæ, purple spots, bruise-like blotches, and extreme depression of the vital power. In these cases the hæmorrhage is strictly of a passive kind, and it is a symptom of the worst omen. Like those effusions of

blood from the same parts that happen in scurvy and purpura, it depends upon a morbid condition of the blood. This is no matter of speculation, for by this time the sensible qualities of the blood are manifestly changed; its natural tendency to coagulate when withdrawn from the body is diminished, the crassamentum is large and loose, and fills the cup, and sometimes is rather an incoherent sediment than a clot. But hæmorrhage from this cause belongs rather to *typhus* fever, and in that species of fever it rarely happens from any other cause.

Another distinctive mark between typhoid and typhus fevers, is the character of their respective eruptions. That of the latter I have already described. It is strikingly in contrast with the eruption of the former.

The typhoid eruption is papular, or pimply. It consists of little circular spots of a bright rose colour, which fade insensibly into the hue of the neighbouring skin. They are slightly elevated, with round heads which never become vesicular, nor petechial. From first to last these spots disappear completely under pressure, and reappear when the pressure is taken away. Each papula lasts about three days. Others follow. Ordinarily the number present at one time is from six to twenty. Occasionally there is one only. Sometimes there are more than one hundred.

These spots begin to show themselves, generally, during the second week of the disease; and fresh spots come out every day or two till the third week, in the course of which they cease to appear, except in cases of relapse, when they also may recur with the other symptoms. Dr. Jenner holds that this species of fever is over by the thirtieth day, since, under ordinary circumstances, no fresh spots are seen after that day. Of course the *illness* may continue much longer — protracted by the effects of the fever, or by pre-existing local complications.

The spots peculiar to typhoid fever do not remain visible on the dead body.

It may not be superfluous to caution you against mistaking *flea-bites*, which are common to nearly all our hospital patients, for this specific eruption which is peculiar to fever patients. The round red stain, with a dark point for its centre, sufficiently distinguishes the mark of the insect from the rose-coloured spot of the disease.

There are certain differences to be sometimes noted in regard to the appearances of the tongue in the two diseases. It is oftener moist throughout the disease in typhoid than in typhus fever; and when dry, more frequently red, and as it were glazed. Generally, if brown at all, it is of a yellowish instead of a blackish brown. Dr. Jenner says, that “the small dry tongue, with red tip and edges, smooth, furred of a pale brownish yellow, and fissured, the surface seen between the fissures being of a deep red—may be considered differentially as a diagnostic sign of typhoid fever.”

With respect to the comparative *duration* of the two diseases, the same observer found that the average duration of the fatal cases of typhoid fever seen by him was twenty-two days, of typhus fever fourteen days. “Half the cases of typhoid fever survived the twentieth day of the disease. Not a single case of typhus fever survived the twentieth day.”

And it is in the more protracted cases of typhoid fever that death is chiefly apt to occur in the way of *asthenia*. In some instances it seems to take place from mere *debility of the heart*, there having been no pulmonary embarrassment, and the head having remained clear. Death, in such cases, is preceded by those symptoms of debility which have been already described. The pulse becomes small, and weak, and like a thread; the patient lies on his back, and sinks down in the bed; the features sharpen; the eyes are hollow, and dim, as though glazed; the sphincter muscles fail to contract; the extremities grow cold; cold sweats appear on different parts of the body; and at length the heart ceases to beat, and the patient to exist.

Death occurring in this manner does not, I say, occur *early*. It happens at an advanced period of the disease. It is noticed sometimes in persons who have been largely bled, or too actively depleted at the commencement of the fever; and in those who have suffered a good deal from *diarrhœa*. In short, as death in continued fever in the way of coma, and in the way of apnœa, is connected often with morbid conditions of the head and chest respectively, so death, in fever, occurring purely or *chiefly* by *asthenia*, connects itself with morbid conditions existing within the belly.

And in typhoid fevers we discover within the abdomen vestiges of mischief so constant and definite, and so different from what we meet with in typhus, as to have led first to suspicions, then to a closer scrutiny of facts, and finally to what I deem full

proof that these two disorders are as distinct the one from the other, as scarlet fever is distinct from small-pox. To these morbid conditions, then, so striking, and so constantly to be noticed within the abdomens of those who die of typhoid fever, I have now to invite your attention.

I need scarcely remind you that the intestinal canal is largely furnished, on its inner surface, with glands, or follicles, which consist of little more than crypts, and of which the precise office has not yet, I think, been accurately determined. Some of these glands are sprinkled (one may almost say at random) over the whole tract of mucous surface. These are accordingly called *solitary glands*. Till I was better instructed by Professor Todd, I used to speak of them as being the glands of *Brunner*; whereas the glands discovered and described by that anatomist are limited to the duodenum. Other glands or follicles are collected into groups, and are named *glandulæ agminatæ*, or often the glands of *Peyer*, who has given a capital description of them. Now the arrangement of these glands of Peyer is peculiar; and in reference to the morbid anatomy of typhoid fever, very necessary to be known. They are met with in the ileum alone: the groups are, mostly, oblong in form; and they occupy that part of the bowel which is opposite its mesenteric attachment. They are largest, and most numerous, and consequently most thickly set, in the lower end of the ileum, which, in some instances, is almost entirely covered with them: they are found also upon the ileo-cæcal valve; but beyond that they do not go in that direction. Ascending from the cæcum towards the jejunum, these groups or patches become smaller, shorter, more circular, and less numerous; they are separated by longer and longer intervals, till at last they cease to be visible at all. Where there are valvulæ conniventes, there the situation of these patches is very obvious; for the valvulæ conniventes are interrupted, and never run across them. This fact has sometimes led to curious mistakes. I have seen in the museum of the College of Surgeons a preparation put up by Mr. John Hunter (but not labelled I presume by him) professing to be an example of the destruction of portions of the valvulæ conniventes by ulceration. It is nothing more than one of those natural patches, rather more plainly developed than usual.

These glands, in their healthy state, are much more conspicuous in some bodies than in others. You may see, if you look at them attentively, that each patch is made up of a congeries of mucous follicles, of which the *orifices* are obscurely apparent. Now the main alterations met with in the abdomen after death, in typhoid fever, are alterations of these very glands: of the solitary glands, to wit; and still more constantly and remarkably of the agminate glands.

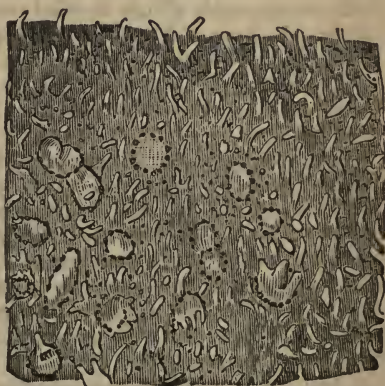
The changes which these glands undergo are of the following kind. In the first place they become enlarged, and more *perceptible* than they are in their natural state. They then present a greyish transparent surface, dotted over with black points; which black points mark, I conceive, the excretory mouths of the several follicles. This appearance, however, which has been likened to that of the recently shaven beard, is not necessarily associated, I believe, with disease. Then, as the inflammation, for such it is, advances, the patch becomes reddish, perhaps; and the follicles burst, or ulcerate, or slough away: not altogether, but partially and by piecemeal: so that an irregular ragged ulcer is generally left, having thickened edges. Sometimes, however, the follicles disappear without there being much redness or thickening: the

FIG. 183.



Solitary gland of small intestine. After Boehm.

FIG. 184.



Part of a patch of the so-called Peyer's glands, magnified, showing the various forms of the sacculi, with their zone of foramina. The rest of the membrane marked with Lieberkühn's follicles and sprinkled with villi. After Boehm.

mucous membrane immediately adjacent, and even the remaining part of the patch of follicles, being pale, and level. Sometimes the patch puffs up into a sort of fungous swelling, in which all trace of the follicular structure is lost. The colour of the ulcerated surface is various, as well as its form and appearance. Sometimes it is pale and grey; sometimes red; oftentimes yellow, as if the exposed cellular and other tissues were stained by the ochrey fluid which had been poured from the bowels during life. What I have hitherto stated relates to Peyer's glands; but the solitary glands participate, usually, in the change. They become, in the first place, large and hard, and present a whitish coloured projection from the surface, which, by a mistaken analogy, has been sometimes called a pustule. At length, a loss of substance takes place in these also; beginning at the summit of each, and producing a small, but sometimes a deep ulcer.

FIG. 185.



Ulceration of the glands of Peyer, with enlargement of the mesenteric ganglions. From a specimen in Dr. Gross' cabinet.

And of these changes it is further to be observed, that they are more common, more numerous, more extensive, more advanced, in proportion as we approach the cæcum. It is natural that we should meet with *more* ulcers near the cæcal valve, because there are more glands there; but undoubtedly the ulcers are (in general) *further advanced* there than higher up in the bowel.

Going along with this ulceration of the mucous glands of the intestines, and above all of the aggregate glands of the ileum, you will frequently find inflammation (*i. e.*, redness, hardness, and swelling) of the corresponding *mesenteric* glands. This would seem to be a consequence of the inflammation and ulceration of the mucous glands. The affection of the mesenteric glands is, I conceive, secondary; and bears the same relation to the ulcers seen in the bowel, as a bubo in the groin bears to a chancre on the glans penis.

These alterations are extremely interesting, because they afford a reasonable explanation of many of the symptoms of typhoid fever. They account for the diarrhoea. They account for the commonest form of hæmorrhage from the bowels: in one instance Dr. Jenner found that water, thrown into the superior mesenteric artery, welled forth freely from the edges of a ragged ulcer not far from the ileo-cæcal valve. They account also for the uneasiness or pain which is experienced when the abdomen is pressed, and teach us why that uneasiness is greatest in the situation of the cæcum. Nay, we can even understand why, although these ulcerations exist, there may be *no* pain occasioned by them. This may be partly owing to the general insensibility to impressions and sensations of all kinds produced by the stupor; but partly, also, it probably depends upon the depth to which the ulceration goes. The mucous tissues are possessed of but little sensibility even under inflammation; but if the muscular and peritoneal coats become involved in the inflammatory process, then pain begins to be felt. I remember, a few years ago, attending a young lady ill of fever, with a very well-informed practitioner, but one of the old school, for he had not turned his attention much to the state of the intestines in that disease. One day we learned that our patient had had hæmorrhage; from the uterus her friends supposed; but when I saw the discharge, I was made certain, by its appearance, and by its odour, though it was not mixed with any fæces, that it had come from the bowels; and I stated my conviction that there was ulceration in the lower portion of the ileum. But she had no pain in the abdomen. You might press any part of it without exciting the smallest uneasiness. So, distrustful of my opinion, they called in an eminent accoucheur, who also pressed and examined the belly; but neither could he detect any tenderness or undue sensibility. He next examined the uterus *per vaginam*; but could discover nothing wrong there. Some few nights afterwards the general practitioner was called out of his bed to this patient. He was told that the hæmorrhage (or flooding as they called it, for they persisted in believing that it was uterine,) had returned; and before he arrived at the house the patient was dead.

We next day opened the body together. The uterus was perfectly natural; there was no vestige of discharge or bleeding in the vagina; but the ileum, for about a foot above its entrance into the cæcum, was in a state of superficial but ragged ulceration, and universally red and besmeared with blood. I mention this as a strong fact in illustration of the possibility of there being much disorganization of the inner surface of the intestines, without any pain to reveal it.

Now the alterations I have last been sketching—the thickening, redness, tumefaction, and ulceration or sloughing of the glands of Peyer, and also of the solitary glands, are met with, almost constantly, in some epidemics of continued fever, and are absent, almost constantly, in others. The kind of fever with which I was conversant in London for ten years before the first arrival of the cholera in this country, I now know to have been *typhoid* fever. The antiphlogistic regimen was indispensable at the outset of the disorder: in many instances blood-letting, either general or topical, seemed to be required, appeared to do good, and certainly was well borne: in well-ventilated wards the disease showed no strong tendency to spread: the mortality was very moderate. About the rose-coloured spots upon the skin I can say nothing, for I did not look for them; but the glands of Peyer, according to my own experience of the fatal cases, were almost invariably affected. Subsequent epidemics, I may specify particularly that of 1838, offered a marked contrast in all these points. A large per centage of those who contracted the fever died: the disorder was propagated from hospital patients to nurses and students: after death, we could not detect any disease of the agminate or other glands of the intestine: the peculiar mottled rash scarcely ever failed to show itself: we were taught by experience to refrain as much as possible from abstracting blood: and almost from the beginning, or quite, we found it necessary to sustain our patients by a liberal allowance of strong animal broths, and even of wine. An epidemic, of which I witnessed a portion in Edinburgh in 1820, was without intestinal ulcers. Chomel, during five years' investigation of this matter in the *Hôtel Dieu*, never met with an exception to the general rule of their occurrence, or of some degree or form of that sort of alteration of the mucous glands, of which the ulcer is the final stage: and the experience of Louis is to the same effect.

Dr. Carpenter has shown it to be probable that the natural office of the glandulæ agminatæ is to eliminate decomposing and noxious matters from the blood, and to discharge them into the intestinal canal: and Dr. C. B. Williams advances the reasonable suggestion, that the ulceration so constantly met with of those glands in typhoid fever may result from the continued operation of the poison of that disease, thus escaping.

When these internal ulcers do occur, is the disease, as is popularly supposed, always or necessarily fatal? By no means. It *may* be fatal, in more ways than one. It may lead to death, by exhausting diarrhœa, in the way of slow asthenia; it may kill by laying open a large mesenteric blood-vessel, and so producing copious hæmorrhage and mortal syncope; it may, and often does, destroy the patient, by *perforation* of the bowel: the ulcer penetrates the mucous and muscular coats, and reaches the peritoneum; and sometimes that membrane gives way, and sometimes it does not. The consequences of its rupture or perforation—the escape, I mean, of the contents of the bowel, and the supervention of intense and uncontrollable peritonitis—I need not again dwell upon. But the ulcers may, and doubtless often do, *heal*; and the *scars* which they leave behind them are frequently to be seen. The ulcerated surface seems to clothe itself afresh, by degrees, with a new mucous membrane, which is thin, however, and adherent to the subjacent tissues, and does not slide over them when pressed between the finger and thumb, as the healthy portions of the coats of the bowel will slide upon each other. And in the place of the cicatrix there is usually to be seen a slight amount of puckering, and a number of little wrinkles or lines, radiating from a common centre. According to Rokitsanski, with whose opinion Dr. Jenner's concurs, the formation of these cicatrices never leads to any diminution of the calibre of the bowel.

But it is plain, and most worthy of consideration, that the existence of these ulcers is likely to prolong the illness of the patient after the fever itself has ended; to protract his convalescence; to hinder his recovery; and even to endanger his life,

though he may seem to be getting well, by causing hæmorrhage, or perforation of the bowel.

Among numerous minuter points of difference between typhus and typhoid fever, observed and recorded by Dr. Jenner, there are yet two which demand mention in the sketch which I am now giving you. In typhoid fever that facile separation of the pia mater and arachnoid from the convolutions of the brain is seldom noticed, which I told you was common in typhus. Again, ulceration of the pharynx was discovered in one-third of Dr. Jenner's fatal cases after typhoid—in no single instance after typhus fever.

The spleen, after death, in both these forms of fever, is very frequently found altered in size and texture: enlarged, of a dark colour, and of soft and sometimes almost rotten consistence.

LECTURE LXXXV.

Relapsing Fever. Causes of Fever. Exciting and Predisposing. Prophylaxis.

WE were occupied, yesterday, in tracing the characteristic and distinguishing features of the two great species of continued fever, *typhus* and *typhoid*, which are of most common occurrence in this country, and which contribute so largely to the general mortality of its inhabitants. There remains yet another species sufficiently interesting and frequent to require a brief description.

There was prevalent in Scotland, in 1843-44, for fourteen or fifteen months, an epidemic fever, characterized by the suddenness of its onset, its wide diffusion, its short duration, and its small mortality;—by its proneness to relapses, by the frequent occurrence of petechiæ, of something like black-vomit, and of yellowness of the skin;—by the absence of intestinal ulcers; and by profuse sweatings, whereby the fever seemed to be solved. The history of this epidemic has been given by Dr. Alison, by Dr. Henderson, by Dr. Cormack, and in great detail by Dr. Wardell. It was thought by these physicians, and by others who witnessed it, to have been a new and distinct pestilence. It was, in fact, what is now well known by the name of *relapsing fever*. Later research has established this to be, indeed, a *distinct*, but by no means a *new* form of fever. In the following paragraph Dr. Jenner traces for more than a century the existence of a disease having the peculiar symptoms and course of relapsing fever.

“Writing on the weather of 1741, Ruttly says ‘there was frequently a fever, altogether without the malignity of the disease already described, of six or seven days’ duration, terminating in a critical sweat (as did the other also frequently); but in this fever the patients were subject to a relapse, even to a third or fourth time, and yet recovered.’ In 1800 and 1801 there was an epidemic in Ireland of a fever generally terminating on the fifth or seventh day by perspiration, and when that happened, very liable to recur. Barker and Cheyne’s Reports, and Dr. Welch’s book on Blood-letting, prove the existence of a similar fever in 1816, 1817, 1818, 1819, and 1820, in Ireland and Scotland: while Dr. Christison’s testimony goes to show the identity of the type of fever in the epidemic of 1826 with that described by Dr. Welch, and also the similarity of the fever in these epidemics to that prevalent in 1843 and 1847.”

Dr. Ormerod has given us a sketch of this kind of fever as it fell under his observation in 1847 in the wards of St. Bartholomew’s Hospital. The subjects of it were chiefly Irish persons newly arrived in London. It was remarkable for the occurrence, in the majority of instances of *sudamina*, or miliary vesicles: so that Dr. Ormerod speaks of it under the name of miliary fever.

Relapsing fever begins, for the most part, with sudden rigors, severe headache, heat and dryness of the skin, loss of appetite, and a very rapid pulse. The tongue is

covered with a thick, moist, whitish fur. Epigastric tenderness, nausea, and vomiting are common among the early symptoms. There is much aching pain of the limbs and joints. The functions of the bowels are not materially disturbed. In many cases yellowness of the skin occurs, amounting to jaundice, with vomiting of matters looking like coffee-grounds, and sometimes as black as ink. Now Dr. Jenner declares, and my own experience is in agreement with his, that he never saw jaundice in typhus or in typhoid fever. Circumscribed petechial spots—"minute hæmorrhagic points," never elevated, are observable upon the skin in a large proportion of patients in relapsing fever. About the fifth, seventh, or ninth day a copious perspiration breaks out, and is followed by a sinking of the pulse to its healthy rate of beating, or even below that, and by what seems rapid recovery. But from the fifth to the eighth day from this apparent convalescence, the primary symptoms return; run, perhaps, a shorter course than before; again terminate in sweating, and in a second convalescence, which is generally permanent. The relapse or repetition of the symptoms may, however, happen three, or even four times.

The rate of mortality in this species of fever is low: and death usually takes place, if at all, before the seventh day of the disease. In the fatal cases jaundice has been observed to be a frequent, but not a constant symptom: the surface becomes cold and livid, the pulse very feeble as well as very frequent, a low form of delirium arises, with drowsiness, which deepens into unconsciousness, and so life departs.

It is stated by Dr. Wardell that in pregnant women affected with this fever, abortion is sure to happen, whatever may be the period of gestation.

In relapsing fever, neither early and extreme frequency of the pulse, nor sudden and great variations in its rate of beating, are, of themselves, prognostic of danger. In this respect, therefore, it differs remarkably from both typhus and typhoid fever, in either of which a pulse of 130 or 140, and in the former of which rapid fluctuations of the pulse, always indicate great peril. In relapsing fever it may amount to 150, or even higher, and upon the breaking out of perspiration it may drop, in the course of two or three hours, to half that number of beats, without warranting any alarm for the patient's safety.

Among the many points of diversity which exist between the three species of fever that we have been considering, one striking and obvious difference is to be found in their respective *duration*. This is apparent even to the observation of the vulgar, who have thus drawn rude distinctions between different epidemics, before they were recognised or acknowledged by the scientific physician. They talk of the one-and-twenty-day fever (typhoid), and of the fourteen-day fever (typhus), according as the disorder "takes the turn" in three weeks, or in a fortnight. In like manner the relapsing fever was called in Ireland the five-day fever; although as it has its crisis most often on the seventh day, it might be termed more properly the seven-day fever. We should thus have the natural duration of the three fevers marked by periods of *weeks*.

Typhus fever appears to be much more common in the great towns of England and of Scotland than it is in Paris, where typhoid fever is the predominant, if it be not the only, form. Relapsing fever has prevailed to a very great extent in Ireland, whence it was probably imported into Great Britain.

The symptoms of all these three forms of fever vary also, *cæteris paribus*, according to the *season of the year*, and the *situation* of the patient: whether, I mean, he be surrounded with pure and cool air, or with a foul and hot atmosphere. In the colder months there is greater risk of inflammatory complications, and especially of pectoral affections: in the autumn we look more for diarrhœa or for dysenteric complaints, engrafting themselves on the disorder. Where the air is close and foul, the symptoms show a much greater tendency to the low or putrid type, and the deaths are more numerous, than where it is pure.

It is of great importance to hold correct notions as to the *exciting cause* of continued fever; respecting which there has been, and there still is, a perplexing contrariety of opinion among medical men. You are aware, from what has already been stated, that I consider the disorder to originate in an animal poison, and to be contagious; communicable, I mean, from one who is labouring under the complaint, to another who is not.

When the same disease attacks many persons in the same house or neighbourhood, at about the same time, the popular suspicion soon arises that the disease is catching. Yet you know that disorders may be widely prevalent without being contagious. Agues, for example, engendered by malaria; ordinary catarrhs and sore throats, produced by vicissitudes of the weather. When an epidemic malady affects large masses of the people suddenly and at once, it is presumably not contagious; at any rate it must have some other source besides contagion. When, on the contrary, it begins in a certain spot, and gradually spreads thence as from a centre, the presumption is in favour of its propagation from person to person. In investigating this subject, if we trace the fever among persons who have had intercourse with the sick, and more frequently in proportion as that intercourse has been close and continued; and if we find that other persons, living in the same place, and under precisely the same circumstances, except that they have had no known communication with the sick, do escape the fever; we have in these facts convincing evidence that the disease has been spread by such intercourse; in one word, that it is contagious.

Have we, then, facts of this kind? We have, in the amplest abundance.

We find, even in hospitals, where cleanliness and ventilation are prized and enforced, that fever attacks many of the persons who come most often and most intimately in contact with those already ill of that disease; chiefly the nurses, next the clinical assistants and the most assiduous of the students, and the medical officers; rarely the other patients, even in the same ward. The separation of a few feet, if due regard be had to ventilation, is sufficient to render the poison inoperative, by diluting and diffusing it in the surrounding purer atmosphere. Three of our nurses in the Middlesex Hospital have fallen ill of fever during the severe epidemic now prevailing (1838); and two of the three have died. It is only when our wards are unusually full of fever-patients that these disasters occur: but they happen very often indeed, constituting the rule rather than the exception, whenever many fever-patients are collected together: as in hospitals which are exclusively appropriated to their reception, or in the fever *wards* of certain general hospitals. In such places the effluvia which proceed from the bodies of the sick are, in spite of all care, the most abundant and the most concentrated.

Dr. Welch, in his account of fever as it occurred in Queensbury House, in Edinburgh, which was opened for the *sole* use of fever-patients during the prevalence of an epidemic in that city, in the years 1817, 1818, and 1819, has the following statement in point. "In this hospital, since it was opened (which was the year before the time when Dr. Welch was writing), my friends, Messrs. Stephenson and Christison, the matron, two apothecaries in succession, the shop-boy, washerwoman, and thirty-eight nurses, have been infected; and four of the nurses have died. With the exception of but two or three nurses, who have been but a short time in the hospital, I am now the only person who has *not* caught the disease, either here, or at the Infirmary, within the last eight or ten months."

I may quote a passage from Dr. Alison, in illustration of the same thing. He is speaking of a more recent epidemic, which occurred in Edinburgh in 1827 and 1828. He says: "During this epidemic, as well as in that of 1817-1819, many of the clerks and nurses employed in the Royal Infirmary have taken fever. Since November last, six of the clerks employed in the clinical wards only, four of those employed in the ordinary wards, and twenty-five nurses or servants have taken fever. All these persons had necessarily frequent and close intercourse with the fever patients in the house, having been employed more or less constantly in the fever wards, excepting only four of the servants. Of these four, two had been employed in the laundry where the linen from the fever wards was washed; one was a porter employed at the gate, who would of course have communication with the fever patients at their entrance or dismissal, as well as with their relations coming to visit them; and one was a nurse employed in the servants' ward, but who was in the habit of visiting the fever wards." Now mark the contrast. He adds, that, "in this very place and season, those of its inhabitants who have *not* had intercourse with fever patients have almost uniformly escaped the disease. Of the inhabitants of the *ground floor* of the house, (including patients in the lock-ward,) none but those already mentioned as having washed the linen from the fever wards, and the barber who shaved the heads of the fever patients, have taken the disease. Yet in the case of malaria" (to which

I must apprise you that many medical men of great authority ascribe the occurrence of continued fever) "it is the ground floor of the house that is generally found the most dangerous. No one of the nurses, whose duty has confined them to the medical or surgical wards where no fever patients were admitted, has taken fever, with the single exception of the woman in the servants' ward above mentioned. And of the numerous *patients* in these ordinary wards, the only one who has taken the fever, within my knowledge, during the present year, was a patient in the men's general clinical ward, who lay in the bed next the door that communicates with the clinical fever ward. If there be *malaria* in this house, therefore, it would seem to restrict itself *in point of space*, as at Queensbury-House *in point of time*, to the immediate vicinity of fever patients." To understand this last remark you should know that, in ordinary years, the inmates of Queensbury-House escaped fever.

We have similar testimony nearer home; in this metropolis. "Every physician connected with the London Fever House, with one exception (writes Dr. Tweedie), has been attacked with fever; and three out of eight have died of it. Also the resident medical officers, matrons, porters, laundresses, domestic servants not connected with the wards, and every female who has performed the duties of nurse, have, one and all, invariably, been the subjects of fever. And to show that the disease is capable of being engendered by fomites, or clothes, the laundresses, whose duty it is to wash the patients' clothes, are so invariably attacked with fever, that few women will undertake the loathsome and disgusting office."

Now it is in vain to attempt to escape from this kind of evidence, by saying that the situation of the Fever Hospital is infected with some local miasm; for the inmates of the Small-pox Hospital, which is immediately adjacent to it, are not affected in this way with continued fever, but remarkably exempt from it, as Dr. Gregory testifies.¹

Evidence of a somewhat different kind, but leading to the same conclusion, is to be found in the fact, that when persons, having the fever upon them, are transferred to some distant spot that was previously free from fever, they frequently form centres from which the disease begins, thenceforward, to spread. It is imparted in this way, even in the country, from family to family, and from village to village.

I was summoned home from Edinburgh on account of my mother's serious illness with continued fever. She was living in a village where there was little or no fever prevailing. While she was recovering my sister took the disease, and had it severely. It had evidently been introduced into the house by a man-servant, whose family lived in a neighbouring village. This family had the disease raging in their house, and he was in the habit of going thither occasionally, and always of sending his linen to be washed there. He first, in our house, had the disorder, then two of the maid-servants, and next my mother.

The cook of Trinity College, Cambridge, living in a street called the Petty Cury, had a daughter in London who fell ill with continued fever, and who insisted upon going home. At that time there probably was no case of fever in Cambridge: certainly none in the Petty Cury, as Dr. Haviland (who gave me this account) satisfied himself by inquiry. The girl was very ill indeed after she reached her father's house; but she ultimately recovered. Every inhabitant of that house, except an old, seasoned nurse, became affected with the fever; and three or four of them died. But no fever existed in the other houses of the same street. When one of the sick persons was convalescent, it was thought that her recovery might be accelerated if she were put into a lodging at Trumpington, a small village two or three miles from Cambridge, in which there certainly was then no fever. Here she and the old nurse were waited on by a servant belonging to the Trumpington House. That servant soon sickened of the fever, and was sent to Addenbrook's Hospital, where she died.

Take one more instance to the same effect, related by Dr. Alison. "Some years ago, at a time when there was no great number of fever cases in Edinburgh, I met with a case in the son of a shoemaker, who was lying in a room in which his father and two apprentices were at work. I could not prevail upon the father to remove his son to the hospital, although I stated the danger of the apprentices being affected. Within two or three weeks after, I found that the two apprentices were lying ill of

¹ Since this Lecture was first delivered, both these hospitals have been removed to new situations.

fever in their own houses: one of them two hundred yards, the other half a mile distant from the workshop, and widely distant from each other. These young men likewise lay at home during the fever; and each of their cases was speedily followed by a succession of others in the inhabitants of the rooms which they occupied, and of those immediately adjoining, who had never been at the workshop. In one of these houses seven, and in the other twelve, were thus affected. Now on the supposition of the fever being contagious, all this was to be expected, and all corresponded to the predictions which were hazarded on that belief. But on the supposition of such succession of fever cases depending on miasmata, there must have been at least two, more probably three, separate and accidentally concurring miasmata to explain the phenomena here observed; one at the workshop, and one at each of the houses of the apprentices; and there must have been this extraordinary coincidence that at each of these last the malaria sprung up just at a time when a patient was lying ill there of fever, which he had apparently contracted elsewhere. Further, the three houses in which these successions of fever cases were observed, are in situations very different from one another; and all of them have been, to my knowledge, perfectly free from fever for years together, both before and since that time, notwithstanding that fever has been much more generally prevalent, and that they have been inhabited by successive families. What probability is there (continues Dr. Alison) that three separate miasmata should have arisen in these three houses, just at the time when their presence was required in each to produce an effect which had been foretold as the consequence of another cause undeniably operating on all?"

If we contrast facts such as I have been advancing, with other cases, in which all the circumstances appear to have been precisely the same, *except* the presence of the alleged cause of the disease—making our observations always upon as large a scale as possible—we approach, as nearly as the subject will admit of, to a demonstrative proof that continued fever spreads by contagion.

The reports which were made by the accredited physicians, to a Committee of the House of Commons, respecting epidemic fever in Ireland, contain abundant and valuable evidence on this point also. Dr. Cheyne states that the farmers and householders in some parts of Wicklow, who would not harbour or admit into their houses strolling persons, nor go to wakes or funerals, remained free from the disease. In Ballytore a committee was formed of persons who took pains to instruct the inhabitants as to the precautions to be observed against infection: such as refusing admission to wandering beggars, absenting themselves from wakes and other assemblies; and even, under certain circumstances, from places of worship. These precautions were so effectual that not a case of fever occurred. Four villages in the neighbourhood of Lismore are stated by Dr. Barker to have been preserved from the fever, chiefly by the exertions of some Roman Catholic clergymen, who persuaded the inhabitants to avoid all communication with Lismore, and with another town in its vicinity, where the fever was rife.

It was observed also that bodies of persons collected together, and fenced about by barriers which precluded intercourse between them and places infected with the fever, remained exempt from it; children in charity schools, soldiers in barracks, and even prisoners in jails. In the same parliamentary report you may see a letter from Dr. McDonald, who had established a fever hospital at Belfast, which hospital alone, at one period, contained 190 cases of the disease. Very near the hospital was a school, containing 700 or 800 young persons; a poorhouse with 300 inmates; and a barrack with 1000 soldiers. These places were never more free from fever than at that time.

Facts to the same purpose abound in the medical reports of the army and navy. Those of the navy are especially valuable and instructive in this matter, because the whole of the circumstances in which the patients are placed come under the certain and immediate cognizance of the medical officers superintending them. I cannot go into particulars here, but must content myself with referring you to the writings of Dr. Lind, Sir Gilbert Blane, and Dr. Trotter. You will there find that ships which had, for a great length of time, been quite free from fever, have had that disease spread rapidly from one individual, recently imported, so as to affect almost all the crew. This was often the case when raw recruits were drafted from the receiving ships. It appears, from reports made to Dr. Trotter by different naval surgeons, and

published in his *Medicina Nautica*, that the fever was conveyed to a great number of vessels forming the Channel Fleet, from the receiving ship called the *Cambridge*.

It is, moreover, found, that when persons ill of fever are taken away from their own close and crowded houses, and when means of purification are employed, the fever ceases to spread in those houses. This well-ascertained fact it is which gives to fever hospitals their greatest, nay almost their only value. They would otherwise, as we have already seen, be detrimental or dangerous to all concerned with them, by concentrating the poison that produces the fever; without equivalent benefit. As it is, they cause, indeed, a certain amount of disease and of death; but by affording opportunities for clearing an infected neighbourhood of the seeds of the fever, and by so preventing its diffusion among a large and healthy community, they save many more lives than they sacrifice.

You may be surprised that I should appear to labour this point; and should take so much pains to prove what may seem to you, as I confess it seems to me, to be an undeniable proposition. But I do so because many have denied, and do still deny it; and plausible reasons are given, by men of name and character, for discarding altogether the notion of continued fever being propagated by contagion. Some even, who have begun by expressing their belief that the disorder was contagious, have ended by becoming strong, aye, violent anti-contagionists. The late Dr. Armstrong was one of these; and there are living men, of high and deserved repute, both in this country and abroad, who hold what I cannot help thinking erroneous — and if erroneous, then manifestly dangerous — opinions on the subject. Chomel informs us that not one in a hundred of the profession in Paris believes that typhoid fever is a communicable disease. Upon a subject so important it is right that you should be furnished with data for forming a correct and settled judgment. But I scarcely expect that the facts I have now brought forward will carry the same conviction to you all; for I agree with Dr. Christison in believing that the discrepancy which exists upon this, as upon some other controverted questions, depends, in part, upon the different structure of men's minds, and their peculiar habits of thought. Most of the anti-contagionists whom I have known have belonged to that party in this country which advocates what are called liberal opinions in politics and in religion. If this should prove to be generally true, it must be regarded as a curious psycholical fact.

I have laid before you some of the positive evidence which goes to prove the contagiousness of continued fever. You may naturally ask, and it is but fair that I should tell you, what are the arguments on the other side of the question.

First, then, it is asserted that continued fever cannot be contagious, because some, nay many persons, who *have* intercourse with the sick, do not contract the disease.

But the force of this reasoning is completely broken by the well-known fact that, in respect of diseases which are on all hands acknowledged to be contagious, and which are even propagable by inoculation, small-pox for example, the same kind of exemption notoriously happens. Some of those who so escape may have had the fever before; and have thereby become less capable of being re-infected by the poison. I believe it to be uncommon for a person to have the same species of continued fever a second time, unless he is exposed to the contagious matter in a very concentrated state, or for a long time together. Some may, by original peculiarity of constitution, be proof against its power; but probably in most cases, the immunity arises from the circumstance that the person has been fortunate enough, or careful enough, to escape imbibing an effective dose of the poison. You may breathe, for a good while, without much hazard, an atmosphere but faintly imbued with the contagious effluvia; and you may, with somewhat more of hazard, breathe, for a short time, air which is strongly tainted by them, and yet go free.

Allance must also be made, in some cases, for the effect of *habit* in fortifying the system against contagion. Persons who are much and often exposed to these effluvia, are thereby *seasoned*, in some degree, to the noxious atmosphere; just as drunkards and opium eaters become at length impassive under such a dose of their customary stimulus as would intoxicate or stupify a novice. Upon this principle has been explained the comparative immunity from contagious diseases, (under like circumstances of exposure,) of medical practitioners, and nurses; of the keepers of filthy lodging-houses, while the new-coming inmates suffer; and even of prisoners,

who, without having had the disease themselves, may nevertheless carry forth and communicate the infection: as is said to have happened at the celebrated "black assizes" in Oxford; and again at the Old Bailey in the year 1750.

This argument, that the fever cannot be contagious because many of those who come near the sick are not affected with it, has been happily ridiculed by comparing an epidemic to a battle. A man might say, "I was in the battle of Waterloo, and saw many men around me fall down, and die, and it was said that they were struck down by musket balls; but I know better than that, for I was there all the while, and so were many of my friends, and we never were hit by any musket balls. Musket balls, therefore, could not have been the cause of the deaths we witnessed." And if, like the matter of contagion, they were not palpable to the senses, such a person might go on to affirm that no proof existed of there being any such things as musket balls.

It must not be forgotten that this same argument, whatever may be its validity, is equally potent against any other general cause: against local miasms; against an undefined something in the air.

Again, the disorder does not spread, say they, in the houses of the rich. But why should it not do so, if it proceed from any cause which strict attention to cleanliness and ventilation does not disarm? The fact, when rightly considered, is a strong circumstance in favour of the theory of contagion.

But then the disorder often occurs when we can trace no contagion: and the full answer to this argument is to be found in the fact that the same difficulty applies to diseases which are not only confessedly contagious, but which are believed to have, now, no other source than contagion; even, as I showed you before, to small-pox. All that the argument can amount to, is a presumption that the distemper may sometimes originate independently of contagion. A person may be exposed to the agency of the contagious matter quite unconsciously, by coming into contact with fomites, or by approaching another person who is just convalescent from the disease. A nobleman died here, in the prime of life, last year, of scarlet fever. The story goes that he had just come to town, and was dining out, and by his side sat a young lady who had just recovered from that complaint; and that from her he unwittingly received it. A lady was attacked with small-pox; and pains were taken to discover how and where she could have caught it: at length it was ascertained that, some days before, she had sat in a carriage, while the post-horses were changed, at an inn-door, near the open window of a room in which a person ill of that disorder was lying. In such a place as this there are a thousand unsuspected ways in which contagion may be disseminated. It may lurk in a hackney coach; you may catch the complaint from your neighbour in an omnibus, at a theatre, or a concert, or at church; your linen may be impregnated with the subtle poison in the house of your laundress; or your coat may convey it from the workshop of your tailor. So that, when continued fever occurs, it is very difficult indeed to be *sure* that it has not arisen from contagion; and it becomes a very interesting question whether the disease ever proceeds from any other cause. It is, however, of primary importance to determine whether it be, or be not, communicable. By our belief in this matter the lives of our neighbours and clients may be saved, or lost. Even if we *doubt* about the contagiousness of the disease, we are bound to *act* as if we had no doubt upon the subject; and I would admonish you (although that is a lower and poorer motive) that your own reputation and success may be endangered by mistakes in this very thing. I have been told of one instance, which may serve as a warning. A lady came from the country to London, to see a sister, who was ill of fever. She asked the medical attendant if there was any danger of infection. He happened to be a staunch non-contagionist; and he said "no danger whatever." Upon the faith of that assurance, the lady resolved to sit up with, and to nurse her sister; but she fell sick herself of the disease, and died. I believe this did not shake the medical man's opinion. He attributed the mischance to some local miasm. But he could not persuade the lady's friends of this. They maintained that her death lay at his door; and whether they were right or wrong, the affair was so much talked of, and was so injurious to him, that he was obliged to leave that part of the town, and to seek for practice elsewhere.

Many sporadic cases occur of febrile disorder, which do not belong to the category of continued fevers, and yet are liable to be confounded with them. All the phleg-

masiæ are attended with pyrexia, and the local inflammation may be slight, or very obscurely marked, and easily overlooked; but the febrile symptoms are much the same as those which constitute the more *inflammatory* forms of typhoid fever. In like manner severe bodily injuries, compound fractures for example, sometimes give rise to constitutional symptoms precisely resembling the most prominent phenomena of the later stages of typhus fever. Excluding all such spurious resemblances of the genuine disease, I confess my own opinion to be, that continued fever, like small-pox and measles, originates, in all cases, from a specific poison. With respect to all these disorders, I conceive that, in every large community, there is constantly kept up a sufficient stock of contagious matter, to act as a sort of leaven, whenever circumstances favourable to the development of the disease as an epidemic, may chance to arise.

Continued fever has been ascribed, by those who deny its origin in contagion, to certain other causes; some of which demand attention, not merely on account of the interest that belongs to the whole inquiry, but also because they really are important agents in the extension of the disease.

First, then, it was, and is, a common belief, that continued fever may be *generated* by the effluvia constantly proceeding from the human body, even when healthy, if these effluvia are accumulated and condensed by the crowding together of many persons in close, dirty, and ill-ventilated places. It is unquestionable that, when once introduced, the disease spreads, under such circumstances, with fearful rapidity; but the point at issue is, whether it be ever so caused *de novo*. I must refer you, for good evidence that it is not so engendered, to Dr. Bancroft's book on the yellow fever. He shows that typhus fever does not spring up in places where it could scarcely fail to appear if that theory were true. He instances the natives of the Arctic regions, who, in order to shelter themselves against the extreme cold of their climate, live during the greater part of the year in close subterraneous dwellings, from which the fresh air is studiously excluded, and of which the atmosphere becomes so offensively foul as to be scarcely endurable by a stranger; yet continued fever is not known among them. A similar exemption from that disease is observed within the tropics, in the African slave ships, where "the poor wretches are crowded together below the deck, as close as they can possibly lie, in a sultry climate, barred down with iron, to prevent insurrection." Although many of them die from suffocation, and from fluxes, yet Dr. Trotter, who was himself, at one time, surgeon to a slave ship, declares that "contagious fevers are not their diseases." Dr. Bancroft quotes also a narrative of the sufferings of 193 Europeans, who, during the time of the first French revolution, were "deported" to Cayenne, in the *Decade* frigate. They were crowded, and even squeezed together, in so small a space, and for so long a time, that the sentinels, who were placed at the hatchways to guard them, and who were thus exposed to the hot and fetid air which came from their hole of confinement, demanded that their period of offensive duty might be shortened. Yet none of these miserable persons perished, nor did fever, properly so called, arise among them. Neither did that distemper present itself in any of the survivors of the black hole at Calcutta; the frightful account of which, by one of those survivors, Mr. Howell, is abstracted in an appendix to Dr. Bancroft's work.

Fever used to infest our English jails; but that it was always imported, and never engendered there by filth and defective ventilation, and by the accumulation of human effluvia, may be concluded from the fact that the benevolent Howard, when he visited the prisons on the continent, found, to his great surprise, that they were free from fever, although they were no less close, crowded, and impure than our own. He brings the result of his observations and inquiries concerning the cause of the jail-fever, to this pointed conclusion:—"If it were asked," says he, "what is the cause of the jail-fever, it would in general be readily replied, the want of fresh air and cleanliness; but as I have found in some prisons abroad, cells and dungeons as offensive and dirty as any I have observed in this country, where, however, this distemper was unknown, I am obliged to look out for some *additional cause* for its production"—which additional cause can be no other than the contagious poison emanating from the bodies of those who have the fever. It is true that fever is most frequently met with, and most rapidly propagated, where men are crowded together, in jails, or in close and ill ventilated places; but this affords no reason for supposing that it is ever

generated there, any more (to use a homely illustration of Dr. Bancroft's) than the general prevalence of lice and other parasitic vermin in such places, proves that these vermin are *generated* by filth, by pent-up human effluvia, and by want of ventilation, instead of being merely *fostered* thereby.

Again, continued fever has been attributed, with great confidence, to a vitiated state of the air, from the putrefaction of *dead* animal and vegetable substances. Dr. Bancroft deals with and demolishes this error also; showing that neither the putrid atmosphere of dissecting-rooms (respecting which *you* must have some personal experience), nor the noisome effluvia from full and ill-conducted burial-grounds, nor those to which tallow-chandlers, soap-boilers, glue and cat-gut makers, and the melters of whale-blubber, are exposed, nor the foul air of sewers and privies, have ever been known to produce anything like continued fever. In some parts of Essex, near the coast, where the farmers are in the habit of manuring their fields with shoals of sprats, I have seen large tracts covered with these fish in a state of putrefaction. The stench they occasion is horrible; but no disease results. Dr. Chisholm, in a paper to which I can only refer, but which I would recommend you to look at, in the sixth volume of the *Edinburgh Medical and Surgical Journal*, brings forward other and very satisfactory instances, to the same purpose: from a bone manufactory, near Bitton, in Gloucestershire; from an establishment (now relinquished) on the banks of the Avon, for converting the flesh of dead animals into adipocire; from manufactories for refining sugar, where the blood of slaughtered animals, obtained from butchers, is kept for that use; from the leather-dressing business;—all tending, I say, to the conclusion, that air, contaminated by the decomposition of animal substances, is not necessarily even noxious to life; still less productive of that specific disease which we are now considering. The old belief, therefore, was unfounded, that the exhalations from the dead and putrefying bodies of men and horses, lying unburied on the field of battle, are sufficient and likely to produce a pestilential fever. Many instances to the contrary are on record: one, of an early date, is thus stated by *Diemerbroeck*:—"Anno 1642, in agro Juliacensi maxima strages facta est, et ad minimum 8000 militum occisi fuerunt, præter majorem adhuc famulorum, rusticorum, aurigarum, puerorum et mulierum numerum, atque equorum copiam innumerabilem: corpora inhumata sub dio computruerunt, nulla tamen pestis insecuta est."

Mind, I neither deny nor doubt that filth, foul air, and the gaseous products of animal and vegetable decomposition, are things hurtful to health; or that they are capable, especially when intense and concentrated, of causing serious disease, and even death. Every now and then some startling proof of this noxious power is forced upon our notice. I well remember the stir occasioned by the sudden outbreak of a violent disorder among the boys in a large school at Clapham about 20 years ago. Nearly a score of them were all at once attacked with most alarming symptoms, and two of them died, in consequence of the opening of an old cess-pool behind the house, and the distribution of its foul contents over a garden adjoining their play-ground. The complaint thus produced was not, however, continued fever, but a sort of cholera. Very recently a formidable endemic malady, arising, there is reason to believe, from a similar cause, broke out in the neighbourhood of the cloisters of Westminster Abbey: and a panic for some days pervaded this end of the town, respecting the "Westminster Fever." The distemper proved fatal in four or five instances. But, although it was attended with febrile symptoms, it was totally unlike (at least the case of it which I saw, and those of which I heard the most, were totally unlike) any form of continued fever that, during no very short or scanty experience, has ever fallen under my observation. The disorder was such as certain poisons are known to excite in the animal economy. Within the space of eleven days it affected upwards of thirty persons; following very exactly, in its course, the line of a foul and neglected sewer, which communicated by direct openings with several, and was in close proximity to nearly all, of the houses in which the disease occurred. It did not spread further or afterwards. What I do doubt—though I would not venture absolutely to deny—is, that genuine continued fever—that typhus, typhoid, or relapsing fever—has ever such an origin. I agree with Dr. Guy—whose interesting report upon the health of night-men, scavengers, and dustmen is well worth your perusal in connexion with this subject—I agree with him in believing that "filth is rather the *nurse* than the *parent* of fever:" but I am not persuaded of the correctness of his final conclusion, that, "in

extreme cases, fever may be bred of filth." The sanitary measures which are now in contemplation by the government are deserving of all praise and help. To cleanse, to drain, to ventilate all parts, and especially the poorer and most populous parts, of our cities and towns, is to promote their general salubrity, and to fortify them, as well as human prudence can fortify them, against the ravages of pestilential disease, which are far greater than even those of war. But, in my judgment, it is wiser and safer to advocate these measures, not upon the disputed opinion that epidemic fever may be engendered by the impurities which they are intended to banish, but upon the unquestionable fact that fever is fostered and spread through those impurities. The specific exciting cause of continued fever cannot perhaps be utterly expelled or precluded; but when present in a community, it may be rendered comparatively harmless by taking away the main conditions of its morbid efficacy, and of its faculty of propagation.

It has been said that fever is produced by some unknown condition of the air, imperceptible by our senses, but distinct from contagion. I believe this is a very popular notion; but it is perfectly gratuitous and untenable, and inconsistent with observed facts. You may have fever raging in certain parts of a town, and yet the parts in the immediate vicinity of these be quite exempt from it. Now this could hardly be, if there were some general property diffused through the common atmosphere capable of engendering continued fever; and, as I stated before, this notion is severely pressed by one of the arguments which the non-contagionists themselves are fond of employing. If the disease depend upon contagion, some persons in the community may come less near the sick, or imbibe a less dose of the contagious poison, than others, and so escape; but if the cause of fever were spread abroad throughout the whole atmosphere, *all* must be exposed to its influence, and few, we may suppose, could avoid suffering from its operation.

Exposure to cold is another cause to which fever has been sometimes attributed. But this is an agency so widely prevalent, that if it alone could excite fever, that complaint would be far more general than it really is, and we should be able to trace it oftener and more distinctly than we can, to the alleged cause. That cold, by its debilitating effect, may predispose the body to be affected by the contagion of fever, I well believe: and the same remark is applicable to another cause that has been assigned; namely, deficiency of nourishment. Dr. Alison, in his admirable essay *On the Management of the Poor of Scotland*, has clearly proved that the prevalence of contagious fever amongst the lower orders is always in direct proportion to their state of physical destitution. The association of pestilence with famine is proverbial. But we do not find that continued fever is ever created by the mere want of nutriment. In persons who have sought to starve themselves to death—among sailors who have of necessity been kept upon very short allowances of food—in cases of isolation by snow storms, or by the accidental closure of a mine—we find, indeed, that *disease* is produced by the privation of nourishment; but it is not continued fever. The condition into which the sufferers are brought is more like scurvy. So that although the want of sufficient aliment may be, and doubtless is, a powerful auxiliary in promoting the effect of the contagious poison, there is no ground for supposing that it ever primarily or solely occasions fever.

In fact, all the circumstances which I have now been considering act as *predisposing* causes. They render the human body an easier prey to the real *exciting* cause, which is a specific animal poison. You will remark that they are all *debilitating* circumstances; and where several of them co-exist, their joint influence in subduing the system, and bringing it into subjection to the contagion, is very great. For the same reason, depression of spirits, however produced, has a strong predisposing effect; as strong, perhaps, as any other single cause. Of this I have seen, if I mistake not, some remarkable examples. Upon this principle we may explain the fact, that continued fever is especially apt to attack those of the lower classes who have recently come to reside in the metropolis; who are often living anxiously, and with persons to whom they were not previously known. We have had numerous opportunities, in the hospital, of noticing this curious circumstance: and it is mentioned by almost all the French writers on fever. To the same principle also we must attribute the tendency to fall down in fever, observed in young soldiers and sailors. You will find statements to that effect in Dr. Trotter's works, and in those

of Dr. Lind and of Sir Gilbert Blane. When fever appears in a ship, the raw sailors are always the soonest attacked by it; and it is the same with the recruits in the land service. In a defeated, dispirited, or retreating army, its ravages are often frightful. Here we have combined, fatigue, a deficiency of wholesome food, and mental depression.

However paradoxical the assertion may seem, a predisposing cause may even be applied, and operate, *after* the exposure to the exciting cause—and so render the latter effective when it might not, otherwise, have been so. Dr. Russell observed the plague sometimes to “hang ambiguously” about persons for several days. In this state, an overheated bath, or a sudden impression of fear, especially *fear of the disease*, has roused the lurking poison into activity. It is a suggestion of Dr. Henry’s that atmospheric variations may call into action contagious poisons already admitted into the system, but not yet manifested by the usual phenomena: and, operating thus over a wide space, and upon numbers at once, may occasion those sudden and violent outbursts of epidemic disease, of which numerous examples are on record.

You must, I think, perceive the importance of distinguishing between the one exciting cause and the many predisposing causes of fever. Both may be combated, with more or less prospect of success. Some of the latter are mostly beyond our absolute control. We cannot hope to prevent or remedy extensive destitution; nor to separate it effectually from its concomitants of filth and despondency of mind. But we may, by timely diligence, root out the specific contagion, or confine it within narrow bounds. When the sick can be at once removed from their crowded homes to a fever hospital, and their impure apartments ventilated, cleansed, and whitewashed, the disease may often be kept in check, if not entirely stopped; and the yet healthy persons of the infected district be preserved from its grasp. For, as I stated before, there is reason to believe that the poison, unless pent up, does not remain active at any great distance from the person from whom it proceeds; not even many yards, or feet. It is very rare, I apprehend, to meet with instances of the disease being communicated in the open air. It is almost always caught, if at all, in the interior of houses. It is extremely uncommon for it to extend from one bed to another in our general hospitals, where great attention is paid to cleanliness and ventilation. The noxious qualities of the poison are diminished, and at length destroyed, by its dilution with common air, just as those of other gaseous poisons are: and hence, in private houses, in the better ranks of society, where the rooms are spacious and airy, and proper precautions are taken, the disease hardly ever spreads.

What are those proper precautions? They are simple, and may be stated in very few words.

Where choice can be made, a large apartment should be selected for the sick person. Unless the weather be very hot, there should be a fire in the room, for it acts as a ventilator. The air of the chamber should be kept fresh by having a window, or a door (according to the weather) always open; or both window and door. Bed and window curtains, carpets, and all superfluous articles of furniture should be removed. Great diligence should be used in keeping the patient clean, by the requisite ablutions, and by frequently changing his sheets and his body linen; and these should be immersed at once in water; and all discharges from the sick person’s body should be instantly carried out of the room.

All unnecessary intercourse with the patient, by his family and friends, should, for his sake, as well as for theirs, be forbidden. As life advances, the susceptibility of the disease appears to diminish; for which reason the nurses and personal attendants of the patient should not be very young: and all who do approach the sick bed should take care to avoid, as much as possible, inhaling the patient’s breath, or the emanations which proceed from his person. Friends who visit the apartment at intervals only, should never enter it *fasting*.

And *in aid* of the simple measures I have just recommended, but by no means *instead* of them, you may, if you please, employ the chloride of lime, or the chloride of zinc, or the nitrate of lead, or freshly burned charcoal, which have been vaunted as “disinfecting” agents; though no conclusive evidence has yet been advanced of their possessing anything more than the very useful and convenient property of destroying noisome smells. People hang a bag of camphor round their necks, and think themselves safe against infection. The mental confidence which that expedient

is calculated to inspire, may perhaps afford some degree of protection : but camphor has, in reality, no prophylactic virtue : and all these artificial scents are objectionable, inasmuch as they tend to conceal offensive odours which might otherwise reveal the actual danger.

Most of what has now been delivered respecting the contagious nature of continued fever, was written and delivered while I yet conceived that there was but one species of that disorder. You may naturally desire to know whether the same contagious quality belongs to *all* the three species of fever which the progress of medical science has since compelled me to recognize — to the typhoid, the typhus, and the relapsing fever : and if so, whether these are all equally contagious : and again, whether they all spring from the same poison, or whether each is caused by its proper virus only.

I believe that they all are contagious : I believe that the contagious property is not equally intense in them all : and I believe that one species of fever cannot generate another, but that each is produced solely and exclusively by its like : that typhus always propagates typhus, and never typhoid or relapsing fever — and so of the rest.

Let us take the last of these propositions ; for the proof of it must include the proof of the first also.

Upon this point we are again indebted to Dr. Jenner for the most satisfactory evidence that has yet been collected. He noticed, as others have done, that whichever of the three species was, in its turn, prevalent in epidemic abundance, cases of the other two were intermixed ; and that these retained without change or modification their characteristic features. There were no transition forms ; nothing like graduation of one species into another. He also took pains, during the years 1847, 1848, and 1849, to track the in-patients of the Fever Hospital to their several homes ; and he found that where more than one fever patient came from the same family or the same house, they all presented the same species of fever. He thus traced 208 patients from 75 different sources of infection, with one exception only to the rule just stated : and this sole exception admitted of an explanation so reasonable, that it could scarcely be said to militate against the general inference.

You may read his evidence in full, in the 33rd volume of the *Medico-Chirurgical Transactions*.

Now this great truth attests conclusively the specific difference of the three diseases. It had suggested itself to other minds, but had never been so successfully worked out. Dr. Alison, in his description of the epidemic relapsing fever of 1833-34, says, "Patients with two kinds of fever, not to be clearly distinguished from one another in the early stage, are daily admitted ; and each of these forms of fever appears to be easily *communicated* in the hospital itself, to *convalescents from the other*." Dr. Henderson, writing in the 61st volume of the *Edinburgh Medical and Surgical Journal* upon the same epidemic, states that in nine instances the same persons had exhibited the epidemic (or relapsing) form, and typhus, within a very short time. He concludes "that the two forms of fever do not acknowledge a common origin ; that they are not propagated indifferently from the same sources of infection ; that the one will not produce the other."

In corroboration of the same conclusion, Dr. Bartlett adduces the remarkable, and otherwise inexplicable fact, "that for the long period of the last thirty or forty years, in the city of Paris, and throughout New England, where this subject has been most carefully studied, only one of these forms of disease has prevailed, to the entire and absolute exclusion of the others."

This being so, the likelihood increases that continued fever never arises except from contagion. Also, since the same person may have each of these separate diseases, it will probably be found, as more exact observations multiply, that the cases in which the same person suffers the same fever twice, are not more numerous than those in which small-pox, measles, or scarlet fever repeat themselves. Dr. Jenner has never known the same individual to be affected twice with typhus fever. A second attack of the typhoid form I believe to be quite as rare. "It is universally acknowledged (says Dr. Henderson) among those who have had much to do with typhus, that an attack bestows immunity from the disease for a long time at least afterwards. I have myself seen no example of a repetition or recurrence of typhus during the

persistence of the same epidemic, and epidemics of typhus usually last between two and three years." He bears similar testimony in regard to the relapsing fever.

Of the three, typhus fever appears to be the most readily communicable from person to person. Dr. Jenner holds typhoid fever to be contagious, "but infinitely less so than typhus." The Scottish records of relapsing fever demonstrate its strong tendency to spread.

I shall speak of the treatment of continued fever when we next meet.

LECTURE LXXXVI.

Continued Fever, concluded. Treatment. Small-pox. Its essential symptoms. Distinction into discrete and confluent. Periods and modes in which it proves fatal.

THE treatment of continued fever has been, at all times, a stumbling-block to young practitioners; and a subject of dispute even among physicians who have built it upon their own experience. Before I attempt to trace out any plan, or to lay down any principles for your guidance, it may be useful to inquire how it happens that the practice in this disease has been so fluctuating and unsettled.

In the first place, then, it is very difficult to estimate the value and efficacy of any particular plan of treatment, and still more of any particular remedial substances, in this disease. Continued fever, of whatever species, like other disorders which run a definite course, and have no direct or necessary operation in spoiling the structure of vital organs, has a strong natural tendency to terminate in health. We see this tendency when the disease is left entirely to itself, and it equally exists when remedies are employed to regulate its course, or to abbreviate its duration. No one can doubt, who has had much experience in fever, that this tendency is sometimes thwarted by the *nimia cura medici*; and that patients get well in spite of the well-meant but mischievous interference of the Doctor. This tendency to recover is a constant source, therefore, of fallacy in our observations upon the behaviour of this disease under different plans of treatment; and upon the effects and utility of remedies. It leads us, too often, into the danger of ascribing to drugs what is really due to the workings of nature; of confounding antecedents and sequences with causes and effects; of counting recoveries as cures. And this danger is increased by the circumstance that continued fever, although it observes a certain definite course, is nevertheless liable, even when left altogether to itself, to sudden and remarkable changes in the symptoms, sometimes for the worse, and sometimes for the better; and often we cannot perceive any obvious reason for these fluctuations. But if this happen when no medicine is given, so also will it happen when the disease is submitted to treatment; and it requires more than a little care and discretion to avoid attributing the changes which so occur to the remedy which was last employed. For example, the abatement or cessation of *headache*, after a few days have elapsed, is a natural phenomenon; whereas an inexperienced or a careless person might easily persuade himself that it had yielded to his method of treatment, and that it was a favourable omen: neither of which conclusions would, however, be warranted by the circumstance upon which it was founded.

There is yet another source of difficulty connected with the subject. I have shown you that not only individual cases of fever, but different epidemics, vary much in their character; nay, that different epidemics may consist of diseases, which though heretofore accounted the same, are really and specifically distinct in kind; so that a plan of treatment which was well suited to one epidemic, may be improper and even hurtful if indiscriminately applied in another.

These considerations may serve in some measure to teach us how it has happened that so many *different*, and sometimes *opposite* remedies and modes of treatment have been recommended by different practitioners for the cure of continued fever. The distinctions established by Dr. Jenner, while they somewhat disturb our previous confidence in the teachings of past experience on this subject, will render all future experience more exact, and probably more uniform also.

The natural tendency to a termination in health was very plainly visible in the epidemic fever in Ireland, to which I have more than once referred; and which was apparently *relapsing* fever. The mortality among the patients who were placed in sheds upon straw, and left with very little medical care, and even without any great personal attention from others, was very small indeed. No one can form even an approximate judgment of this tendency, who has not seen the disease under several varieties of practice. Doubtless one rule which we derive from a clear perception of the same tendency is, that we should *not* interfere *unnecessarily*. Ασκειν περι τα νοσηματα δυο (says Hippocrates), ωφελεειν, η μη βλαπτειν. Two objects are to be kept in view in the treatment of diseases: first, that we do the patient *good*; secondly, that *at least* we do him *no harm*. In all these exanthemata, he must be reckoned the safest and the best practitioner who knows when to abstain from acting, as well as when to act; in other words, who has learned when, and to what extent, the case may be left to the salutary processes of nature.

However, there is an opposite error to that of mischievous activity. The tendency to recovery which manifests itself under different modes of treatment, and even in spite of opposite modes, has induced, in some minds, a degree of scepticism as to the utility of any remedies, that may easily be carried too far. It does not follow, because the majority of patients under continued fever would at length emerge into health, although no remedial measures were employed, that the disease ought therefore to be abandoned to what Cullen calls the *vis medicatrix naturæ*. It is not quite correct to say, with the older pathologists, that the whole disorder is merely an effort of nature to throw off something noxious to the system, and *therefore* is not to be interfered with. The true view of the matter I apprehend to be that which a toxicologist might take. The disease is produced by a poison of which the injurious impression upon the animal economy at length ceases, or passes off, of itself; in the same manner, only more slowly, as the influence of a dose of opium will spontaneously pass away. But during the natural course of the fever, as in many other cases of poisoning, morbid processes are apt to be set up, which, if suffered to proceed unchecked, would inflict irreparable injury upon important organs, and which are fairly within the scope of curative management. Tendencies accompany, or conditions survive the fever, which remedial measures, opportunely and judiciously applied, avail to oppose and to control. Our object must be, when the fever is once established, to conduct it to a favourable close; to "obviate the tendency to death." Upon this point I agree most entirely with Pitcairn, who, being asked what he thought of a certain treatise on fevers, declared, "I do not like fever curers. You may *guide* a fever; you cannot *cure* it. What would you think of a pilot who attempted to quell a storm? either position is equally absurd. In the storm you steer the ship as well as you can; and in a fever you can only employ patience and judicious measures to meet the difficulties of the case."

When some immediate change ensues in the symptoms or in the feelings of the patient upon the administration of remedies that are generally followed by sensible effects, we are warranted in ascribing the change to those remedies. But even here comes in the fallacy already noticed, arising from the sudden and *spontaneous* changes that are apt to occur in fever: and this fallacy is to be got over only by multiplying our observations.

After all, the best guide that you can have in determining upon the general principle of treatment in a given epidemic, or even in an individual case, is that which Dr. Alison has so ably enforced in his lectures and in his writings upon this subject. I mean the observed tendency to this or that mode of dying. The manner and circumstances of the deaths, are of more practical importance than of the recoveries.

After briefly passing in review some of the principal remedies that have been employed and recommended for this disease, I propose to sketch the plan which I am myself in the habit of pursuing in the management of fever patients.

It was once a favourite practice with physicians to attempt to *cut short* the fever at its outset: and the two expedients which were chiefly relied upon for that purpose were *emetics*, and the *cold affusion*. They have both of them, in this country, gone very much out of fashion. In truth, neither reason nor experience encourages us to look for such a result from such measures. If fever depend (as I believe it does) upon a poison in the blood, it is not to be dislodged by the act of vomiting, nor washed out by the forcible descent of cold water upon the skin: and in the few instances in which the one or the other of these remedies may have *seemed* to arrest a fever, or to check its progress, that effect has always occurred at the very commencement of the complaint: so that we cannot be sure (and the probability lies the other way) that these were really cases of fever at all, or that they would not have ceased even if nothing had been done for them. Perhaps emetics may, in the present day, be too much neglected. I have no notion of their *stopping* the fever; but when given early, especially if gastric disturbance be a prominent symptom, they are sometimes followed by a marked abatement of many morbid sensations. "It is astonishing," says the observant Sydenham, "how it happens that a vomit, which does not produce either a large or a morbid discharge from the stomach, should so materially relieve the nausea, restlessness, anxiety, and furred tongue of the patient."

The cold affusion is not more effectual in cutting fever short than the treatment by emetics; and it has this great disadvantage, that it fatigues and alarms the patient: and when the vital powers are naturally feeble, or are much depressed by the disease, the very shock of the affusion may be attended with injurious consequences.

A modification of this expedient is, however, often of great use in abating the morbid heat, and soothing the uneasy feelings of the patient. I mean the practice of cold or tepid *sponging* of the surface. This is one of the remedies which, when the symptoms appear to indicate it, deserves to be tried; and the propriety of continuing or of discontinuing it may be determined by a very simple test, namely, the feelings and wishes of the patient himself respecting it. Speaking generally, it is less eligible in that form of fever which we must henceforth call typhus, and it is chiefly adapted to the typhoid disease.

Great controversies have been maintained in regard to the effects of *blood-letting* in fever. They who hold that the fever consists in a general disturbance of the system, growing out of some local inflammation, and they especially who believe that continued fever is nothing else than inflammation of the brain and its membranes, would naturally seek to cure it by the remedies of inflammation. But although local inflammation, and even inflammation of the brain, is very apt to spring up in the course of continued fever, there is no reason for thinking that inflammation *any where* is *essential* to the fever; but very much reason for the opposite opinion. The active use of blood-letting has been in favour and out of favour, with the medical world, again and again: and this very circumstance would of itself make us doubt the propriety of its indiscriminate adoption.

The late Dr. Armstrong gave a strong and unfortunate impulse to the practice of free blood-letting in continued fever, by the publication of his well-known and ingenious treatise on the disease. I have no doubt that great mischief was done by that work. I may say so without scruple, since Dr. Armstrong is gone, and neither his feelings, nor his success, can be hurt by the expression of such an opinion; and I do so the rather, because it is well known here that Dr. Armstrong saw reason, as his experience increased, to qualify those views respecting the nature and management of fever, which his earlier observation of it in the country had led him to form. This change in his sentiments was probably justified and produced by a specific difference in the character of the fevers that he subsequently witnessed; but it does not appear in his book. You have heard me state already that whereas the fevers which occurred in London for some time previously to the year 1831 or 1832, not only bore, but sometimes even required, the abstraction of blood, in one way or another,—since that period, and especially since the epidemic cholera began to visit us, it has been necessary to abstain, whenever we could with safety, from taking blood at all: and still more necessary, even if we take away blood with one hand, to uphold the patient with the other; while, in the former period, wine and stimulants of all kinds seemed generally superfluous, if not pernicious. Typhoid fever I believe to have been predominant during my earlier, typhus during my later experience.

Dr. Robert Williams, of St. Thomas's Hospital, has shown, very clearly I think, in his excellent work on "Morbid Poisons," that the evidence against the efficacy and the safety of bleeding in continued fever—of bleeding largely, I mean, from the arm, far outweighs that in its favour: and I venture to advise you, as the result of all that I have seen of the disease in London and elsewhere, *not* to draw blood from a vein, even early, *merely* because the disease is or appears to be *fever*; not to order venæ-section unless there be some other manifest reason for it—unless, *i. e.*, the febrile symptoms run unusually high, or unless some local inflammation is unequivocally present: and, when you do bleed, do not take a drop of blood more than seems absolutely necessary to answer the desired end. Bleed your patient, therefore, if at all, in the upright posture.

Purgatives—What are we to say in general with respect to them? This, I believe—that the intestines should be cleared by an active aperient in the outset; and that laxatives should be continued if the bowels do not act every day without them. When however the vital power is much depressed, and when the symptoms indicate or threaten ulceration of the intestinal glands, purgatives certainly ought not to be pressed.

Much contrariety of opinion has prevailed also among practitioners, and does prevail, about the administration of *mercury* in this disorder. Without attempting to strike the balance between these conflicting judgments, it is my business and duty to state my own belief, to tell you what is the result of my own observation, upon this and other disputed points. I must repeat, then, that my practice has altered, in several particulars, within the last few years. In the fevers which I treated, or saw others treat, in London, prior to the appearance of the more recent epidemics, mercury, in one shape or another, was almost constantly prescribed; and a great number of the patients were brought, sooner or later, under the specific operation of that mineral: and in these patients (with one exception only, where the mercury appeared to do neither good nor harm) a *decided improvement* was almost immediately apparent upon the supervention of soreness of the mouth; and all such patients ultimately recovered. I am aware, however, and I wish you to be aware, of an alleged source of fallacy in this matter. The gums in that variety of fever to which I am at present referring, did not readily take on the mercurial action; and it *might be* (though such is not my own impression), it might be that the affection of the mouth by mercury was attributable to the mildness or to the cessation of the disease, rather than the cessation of the disease to the effects of the mercury upon the system: that the improvement was the cause, and not the consequence, of the mercurial action.

In the form of fever that is *now* epidemic (1838), I do not think mercury so proper. Last year the spotted (*i. e.* typhus) fever broke out in a patient of mine while he was in the hospital, and while his gums were sore. I found, upon inquiry at that time, that of twelve fever-patients who had been recently treated in the hospital, by my colleagues and myself, with mercurials in greater or less quantity, four had died, in all of whom the gums were affected: whereas, of sixteen others, who took no mercury, three only died; and of these three, one was pulseless at her admission, from uterine hæmorrhage, and the other two were so feeble and exhausted that they were scarcely able to swallow. I should give mercury therefore very cautiously, if at all, in fever that is marked by the mulberry rash.

Besides a careful enforcement of the antiphlogistic regimen, my own mode of treating continued fever is somewhat of this kind. I am always desirous that the patient's hair should be cut off. The mere removal of it is often attended with benefit; the headache and confusion of thought are relieved, and the patient is calmed. We can then also, with much greater convenience and effect, apply cold washes to the head. Patients sometimes demur to this shaving of their heads; but they generally consent if you explain to them that their hair will at length fall off, in consequence of the fever; and that the head, if on that account only, had better be shaved at once. The head and shoulders should be somewhat raised, and thin strips of linen, kept constantly wet with some cold lotion, should be constantly applied upon the forehead and scalp. It should be the business of one person to attend to this. You would be surprised at the rapidity with which the cloths sometimes dry.

Now with regard to this remedial measure you need not have any difficulty. It

will do good, and should be steadily employed, so long as it is *grateful to the feelings* of the patient; and it *will*, generally, be pleasant and agreeable to him so long as the head remains morbidly hot. If the temperature of the scalp be not above the natural standard, and especially if the cold application make him shivery or uneasy, and give him annoyance instead of comfort, then it must be at once discontinued.

If the patient suffered intense headache, and his face were flushed and the heat of the surface great, and he were wildly delirious, and his pulse were full and hard, I might perhaps deem it right to take blood from his arm, while he sat up. But even under these circumstances I should generally think it better to apply leeches to his temples, or behind his ears, or to remove a few ounces of blood from his neck by means of cupping-glasses, and at the same time to apply assiduously the cold lotion. The combination of headache with delirium warrants the suspicion that inflammatory mischief may be going on in the brain. Dr. Jenner observes, of all these species of fever, that "after the patient becomes delirious he never complains of headache, and rarely admits its existence even when questioned concerning it; while in cases of intracranial inflammation headache is constantly and even loudly complained of, after delirium has commenced."

In the outset of the disease, and while its species may as yet be uncertain, if the bowels have not been already purged by nature or by art, it will be right to give three or four grains of calomel at once, and to follow up this dose by a senna draught. After that, in the earlier period of the fever, especially if the alvine discharges are scanty, dark-coloured, or otherwise of unnatural appearance, a couple of grains of calomel, or four or five grains of the *hydrargyrum cum cretâ*, may very properly be prescribed, in pills, three or four times a day. And the common saline draught will generally be useful and refreshing to the patient; who may be allowed to drink toast and water also, or barley-water, as often as he wishes. As the disorder goes on, if the rose coloured spots declare it to be of the typhoid species, and if diarrhoea arise, the state of the abdomen should be carefully investigated; and when much tenderness is found to exist in the cæcal region, with gurgling perhaps felt under gentle pressure with the hand, a few leeches may be applied over the tender spot, and their bites covered with a light poultice: and should the diarrhoea persist, or become profuse, a certain quantity of Dover's powder, or of the extract of poppy, should be added to the *hydrargyrum cum cretâ*.

But if the fever should prove to be typhus, and to exhibit a strong and early tendency to depression of the vital power, with a signal loss of muscular strength, a confused and dusky countenance, a mottled state of the skin, simulating the eruption of measles, a dry dark-brown tongue, and a feeble pulse — under these circumstances you must begin very early to give the patient a full allowance of good beef tea; and if the symptoms of prostration become more pronounced, you may add ammonia in full doses, Hoffman's æther, or what is much the best of all, wine: and you must omit the mercury.

Under this kind of management the patients will often go on, in a doubtful state, for some days, and at length begin to recover. Many of them, in both species of fever, but especially in typhus, sleep heavily, as the disorder passes slowly off.

There is one point in the treatment of fever, of exceeding importance, and of some nicety: I mean the use of *opiates*. When they are given inopportunately, they are apt to puzzle and perplex the case. You do not know how much of the disposition to coma is owing to the disease, and how much is the consequence of the remedy. Again, you may easily *augment* the natural tendency to coma, and lull your patient into a fatal stupor. But, when judiciously administered, opium will often save a patient who would inevitably sink without it.

It is in that form of fever which the French call the *fièvre ataxique*, — when the patient is affected with delirium, restlessness, wakefulness, and spasm, and the disturbance of the nervous system outruns the disturbance of the sanguiferous system, — that opium is so beneficial. The condition of the patient resembles that of a person in *delirium tremens*. It is said that these symptoms occur most commonly among patients in those ranks of life that are above the lowest rank; and it probably is so: but they are apt to take place in any patients, high or low, rich or poor, who have had the mind overwrought, and the nervous system unstrung, whether by dissipation and intemperance, or by anxiety of any kind. Sydenham was quite aware of

the existence of this particular set of symptoms, and of the remedy for them. Of all these symptoms, *sleeplessness* is the most urgent. Dr. Grattan and Dr. Latham have both written in praise of the same opiate treatment, under such circumstances, as was recommended by Sydenham. Dr. Grattan observes, with great truth, that two or three nights spent in restless delirium are followed by the worst consequences; and that patients who pass *three* nights in succession in that way, almost invariably die. If the symptoms be well marked, the best mode of proceeding is to give a tolerably full dose of opium in the evening; one-third of a grain of acetate of morphia is a common prescription with me. The amendment of the patient, on the following day, is often very striking. Unless the same symptoms recur, it is better, I think, not to *repeat* the anodyne. But, as Dr. Latham cautions us, "there are cases, where the indications for the employment of opium are *doubtful*. Wild delirium, and long wakefulness, and a circulation weak and fluttering, seem to call for a considerable dose of opium. Yet, withal, there is a certain jerk in the pulse, so that we cannot help *suspecting* that the blood-vessels have something to do with the sensorial excitement. Under such circumstances, I have certainly seen (says he) twenty minims of laudanum produce tranquil sleep, from which the patient has awoke quite a new man. But I have also seen the same quantity produce a fatal coma, from which he has never been roused. Now (continues Dr. Latham) since it is a fearful thing to strike a heavy blow in the dark, where the alternative is of such magnitude, it is the safest and best method to administer a small dose, at intervals of an hour or two: so as to stop short of actual mischief at the first glimpse of its approach, or to be led, by a plain earnest of benefit, to push the remedy to its full and consummate effect. Many doses may be required for this purpose; but we shall see, after the first or second, whether to go on or to desist."

When, as is sometimes the case, the stress of the disorder falls upon the thorax, and there is much dyspnoea, with the sounds that denote inflammation of the bronchial membrane, or of the pulmonary substance, leeches or cupping-glasses may be applied to the chest; and, in milder cases, a blister, or a mustard cataplasm.

These remedies—cold to the shaven head; the local abstraction of blood wherever there happens to arise unequivocal evidence of local inflammation; an active purge at first, and mild aperients afterwards, if the bowels are confined or sluggish; moderate astringents, if there be much or urgent diarrhoea, a few grains of Dover's powder, for example, or of the extract of catechu; opium in a more efficient dose, when the nervous symptoms are prominent, particularly sleepless delirium and restlessness; in certain cases small and repeated doses of some preparation of mercury; and in certain cases, *early* support by animal broths, and even by wine—these remedies, adapted to the particular circumstances of individual patients, form the staple of the treatment of continued fever, according to the best of my judgment and experience.

After what has been said, I need scarcely again admonish you to study carefully, not merely the symptoms of any particular case to which you may be called, but the general character of the fevers that are at the same time prevalent, and the manner of dying in the fatal cases. If you find that they who die, die chiefly in the way of asthenia, that will be a strong reason for caution with respect to the removal of blood, and for the early employment of beef-tea, and other means of support. In the form of fever which has of late years been common in the most crowded and unhealthy parts of London, I am sure that the risk, if there be any, of beginning this sustaining treatment a little too early is much less than the risk of commencing it a little too late. If plenty of beef-tea do not suffice, you must give the patient wine, and that sometimes to a very large amount, or even brandy; the egg-flip of the Pharmacopœia for instance, the *mistura vini gallici*. The object is to keep him alive, to keep the heart in motion, until the depressing influence of the exciting cause of the disease shall have passed by. If the wine should flush or excite him, or render the pulse hard, it must be diminished in quantity, given less frequently, or omitted altogether. If there be indications of local inflammation—pain (for example) in the cæcal region, increased by pressure—leeches may be used, and wine given at the same time. There is nothing inconsistent in such mixed practice. We seek to remove the local inflammation by unloading the capillary blood-vessels in or near the part, while we uphold the general powers of the constitution which are ready to sink. The great art of getting a fever patient through a bad attack, is to have him judi-

ciously and perpetually watched, by night and by day. The remedy that is proper one hour, may do harm if pushed during the next. And there is another reason, which I may mention *en passant*, why the sick person should never be left alone, even for a moment. I have heard of more than one or two instances, of patients, in the delirium which attends the disease, getting out of bed, and out at the window, during the temporary absence of their nurse, and perishing from the fall.

When the patient relishes and wishes for the beef-tea, or the wine, that is no small warrant of the propriety and usefulness of its administration.

A word or two more may be proper, even in this cursory sketch, respecting certain incidental points of practice that are apt to arise.

Great comfort and soothing may often be obtained for the patient, when the abdomen is uneasy, by the application of a large warm linseed-meal poultice; or of the epithem called *spongio-pyline*. In the latter periods of typhoid fever it is not uncommon, even when pain is no longer excited by pressure made upon the belly, for the diarrhœa to persist, and for a troublesome degree of *meteorismus* to come on; a tympanitic distension of the intestines with gas. Under these circumstances, a large *blister*, laid over the abdomen, has often very happy effects, upon both the diarrhœa and the meteorismus. The diarrhœa, when very urgent and obstinate, may often be checked by opiate *enemata*. In some cases I have recently found catèchu of great use, in these long-drawing forms of diarrhœa during, or after, fever.

Again, if the patient sink into profound coma, a blister should be applied to the shaven scalp. The sick man will sometimes awake from deep stupor while the blister is still rising.

It is always a matter of importance, as I stated before, to inquire carefully, every day, into the condition of the bladder of these fever-patients. In the state of stupor and indifference in which they often lie, they appear not to be sensible to the want of passing the urine, which collects in the bladder, and distends it enormously; not only increasing the present hazard of the patient, but laying the foundation, sometimes, of future disease of the kidneys, in case he recovers from the fever. You must not take the assurances of the nurses upon this point. They will often tell you that the patient has made plenty of water, when in fact the urine has been dribbling away from him, overflowing, while the bladder is stretched to the utmost. Feel, therefore, with your hand, and percuss the hypogastric region, as well as that of the cæcum, at every visit.

It is requisite, too, that the under surface of the patient's body should not only be kept scrupulously dry and clean, but be *looked at* every day, or twice a day. If the projecting points, the hip bones, the sacrum, the shoulders, the elbows, should become *red*, that is a sign that they are likely to slough or ulcerate. This evil consequence of continued pressure upon parts of which the vital power and healthy tone are lowered, may often be prevented by washing the erythematous spots with brandy. Should the skin be already broken, the place may be covered with soap plaster; or with the *amadou* plaster, lately introduced by Mr. Wetherfield, which does not wrinkle or ruck up. An adjustment of pillows and of posture—or the water-bed—or the water-cushion—may sometimes supersede the necessity of these local expedients.

When ulceration of the mucous follicles perforates the bowel, that catastrophe does not *always* so distinctly declare itself in fever, as in other cases. I have seen such perforation, and its resulting peritonitis, when no complaint of pain had been made by the patient, so great was his insensibility. In general, however, the rupture of the gut is denoted by symptoms which cannot be mistaken. When it does occur, there is but little chance of the patient's recovery; and that little will be best husbanded by the adoption of the plan of treatment which I formerly laid down; opiates, to check the peristaltic movements of the intestine; and a rigid adherence, for some days, to the horizontal posture. Dr. Jenner, indeed, concurs with Rokitanski in believing that perforation of the intestine in typhoid fever, even though the passage of its contents into the cavity of the peritoneum may be for a while prevented by adhesions, is always fatal at last; that the adhesions are never permanent. It is well to bear in mind the risk of hastening, or even of causing, this fearful accident by careless or heavy pressure of the hand upon the abdomen, in a late stage of the disease.

I have seen a few instances, in which an œdematous swelling of one leg and thigh has occurred in the advanced stage of typhoid fever, like that which is incidental to parturient women, and depending upon the same cause—inflammation and obstruction of the great vein that returns the blood from the limb towards the heart. I believe that the inflammation extends itself, in such cases, from some of the smaller veins of the mesentery, which have been injured by the intestinal ulceration. Fomentation of the affected limb comprises all the remedial treatment which this accident requires or admits of.

The management of the patient during convalescence is scarcely of less importance than during the progress of the fever. The chief danger is, that his desire to be allowed to get up, and his wish to eat animal food, should be too soon indulged. The latter of these errors is more frequently the cause of a relapse than any other circumstance; and relapses are often more perilous and difficult to remedy than the original malady. You must be prepared, therefore, to withstand the solicitations of the patient and of his friends, who think that if strength be wanting, strong drinks and plenty of meat, are the things to impart it. Until the tongue is quite clean and moist, and of its natural colour, and the pulse has lost all its undue frequency, and the skin its excess of heat, the patient must be kept to broths, jellies, puddings, and preparations of the well-known farinaceous articles of food. Then he may begin with some boiled white fish, and so gradually eat his way, through chicken, and a mutton chop, to his ordinary diet again.

Such, I say, is the general plan of treatment which some observation of this disorder has persuaded me is the best. Summarily expressed, it consists in the exercise of incessant vigilance, and the adoption of the proper remedy at the proper moment. It lies between a timid or sceptical abandonment of all known resources, and a meddling rashness in applying them. The flame of life may be suffered to expire for want of timely succour and tending, by the practitioner who folds his arms, and looks on; as it may be rudely extinguished by a restless or routine interference which has no definite or intelligible purpose. Boerhaave, in the preface to his *Aphorisms*, professes that he knows of nothing which can be fitly termed a *remedy*, "*quin solo tempestivo usu tale fiat.*" In fevers the wisdom of this maxim is eminently conspicuous. The rational objects of treatment are, to mitigate the urgency of symptoms that cannot be wholly subdued, to redress (so far as art may redress) those dangerous complications which are incidental but not essential to the disease; and to aid the conservative efforts of nature, when these manifestly languish and fail.

Dr. Robert Williams held that enemata of warm water and syrup of poppies would do all that could be done beneficially. For some time he lost only one patient out of sixty-three thus treated. This was a most encouraging result. But then, when the fever changed in species, and typhus began to prevail, as it did about the period of the arrival of the epidemic cholera, he lost one in every four or five: a very large mortality. These facts illustrate, in a strong manner, the necessity, which I have so often endeavoured to inculcate, of taking into account, when we would estimate the value of a particular remedy or plan, the difference which obtains in different epidemics, whether bad, good, or no treatment whatever be adopted. A far surer method is to compare (if you would experiment at all) two or more different modes of practice in different cases of the *same* epidemic. Thus Dr. Latham, finding during one season that his wards were full of fever, while yet its type was so mild that scarcely any died, thought this a favourable opportunity for trying whether mercury had any beneficial operation upon the disease. Accordingly he treated half his cases with small doses of the *hydrargyrum cum cretâ*; and the other half with the *liquor ammoniæ acetatis*, and so forth, and no mercury; and he found that the patients in the first of these classes were, on the average, convalescent sooner than those in the last. Chomel fancies, from some trials, that the chlorate of soda is a useful remedy, in addition to the general plan of management. For some time past I have myself been in the habit of giving to all my fever patients a drachm of the chlorate of potass dissolved in a pint of water, as a daily drink. Without being able to tell you precisely in what respect, in what degree, or in what way, this salt appears to do good, my own impression is strong that it does exercise some favourable influence upon the general character and course of the disorder.

It may be right that I should notice briefly a method of treating continued fevers

which has of late been strenuously advocated by Dr. Dundas. Led, apparently, by his own experience of its admirable effects in the malarious fevers of Brazil, which often run into the continued form, Dr. Dundas maintains that quinine, in large and frequently-repeated doses, is a specific cure for continued fevers as we see them here: to use his own words, that "*cinchonism* will be found to control generally the continued fever of this country in all its forms, in all its stages, and in all its complications." The condition here called "*cinchonism*" is marked by the occurrence of giddiness, deafness, and a sense of buzzing or some kind of tinnitus in the ears. Large and frequent doses of quinine exercise also a remarkable influence over the pulse, rendering its beats weaker, and slower.

This plan, thus strongly commended, has been tried with varying results by sundry medical practitioners. In some hands it has signally failed; in others it has seemed to prosper. Favourable reports are made of it from Dublin, from Drogheda, from Cork; from Liverpool, which is full of Irish poor; and from Manchester, which probably is not less so. In this town it has not succeeded, nor in Edinburgh. Dr. Barclay of St. George's Hospital, Dr. Peacock of St. Thomas's, and Professor Hughes Bennet of Edinburgh, report severally, as the upshot of numerous trials, that large and repeated doses of quinine neither cut short the fever, nor in any way favourably influenced its progress. If in any of our three species, I should have looked for success in the relapsing fever; which in some of its habits resembles the malarious fevers, and which is much more common in Ireland than elsewhere, and among Irish emigrants to our large towns. But of the cases in which this quinine treatment is reported to have effected cures, some are expressly described as instances of maculated typhus, and others as instances of typhoid fever.

[THE YELLOW FEVER. — The yellow fever is a disease which bears a close affinity, in some of the features of its etiology and symptomatology, to the febrile diseases of acknowledged malarial origin, and yet differs from these, as well as from all other fevers, in many striking and important particulars. It would appear to be, in fact, as it has been described, by several distinguished physicians, an affection *sui generis* — a specific fever.

The occurrence of yellow fever is confined, strictly, within a certain geographical range, and limited by the atmospherical temperature of the climate and season — too great, as well as too low a degree of heat appearing to be alike inimical to its production. It is invariably arrested by the appearance of frost.

The natives and permanent residents of the localities to which the disease is endemic are in a great measure exempt from its attacks, as is also the negro race.

It is a fever of but one single paroxysm, terminating, usually without the crisis of perspiration, in the great majority of cases, within seventy-two hours, and followed by an entire remission, succeeded either by complete convalescence, or, after a longer or shorter interval, by a state of exhaustion, and a series of morbid phenomena unattended with febrile reaction, eventuating, most commonly, in death.

Persons who have once suffered an attack of the fever are generally, though not invariably, exempt from a recurrence of the disease in future.

Yellow fever has received a variety of denominations, founded on the place from whence the disease is supposed to have been originally derived, on one or other of its characteristic symptoms or peculiarities, or on its supposed pathological character. The term yellow fever, by which it is known to the large majority of English and American physicians, as it involves no hypothetical views in relation to its etiology or pathology, is perhaps, in the present state of medical opinion, as good a one as can be adopted. It is based upon the icteroid hue of the surface by which the disease is usually attended in some one or other of its stages.

In its pathological character, and pathognomonic phenomena, yellow fever is invariably, one and the same disease in whatever locality it may appear, as well as during all its subsequent recurrences in the same locality. Nevertheless, it is liable to assume, in different places, at different periods, and often at the same place and during the same season, various important modifications in its general characteristics — proceeding, not from any specific difference in the nature of the disease, but from varying degrees of reaction — from a tendency to depression in the vital forces — from the preponderance of certain symptoms, appertaining or not to the disease, and vary-

ing according to the greater or fewer number of the vital organs involved, or from other causes; the whole depending on peculiarity of constitution, temperament, habits, or state of health in those attacked; on a difference of intensity in the morbid agent, and on various contingencies resulting from the nature of the localities at which the disease prevails, the degree of temperature, humidity, and other atmospherical phenomena.

Hence, it follows, that, in the examination of the disease, no invariable concatenation of phenomena, succeeding each other in regular order, as the disease progresses towards a favourable or fatal termination, are to be expected in every case.

"But a variety of groups of symptoms which, though linked together by certain phenomena—which, being pathognomonic of the disease, approximating to each other in several other respects, afford very strong evidence of their being all members of the same family—are yet sufficiently distinct in their general outline and their mode of progression, to justify their being made the subject of separate consideration. Certain of those groups of symptoms, or varieties of the same disease, prevail more generally in some regions than in others; they are also more frequently encountered in some seasons than in others, in the same place; while in some epidemics, several or all the varieties are intermingled among the different individuals attacked; in others, the fever assumes much the same character in the majority of those affected. Such being the case, it is evident that the description of the disease as it affects one individual, or one set of individuals, will not necessarily apply to another case or group of cases. In some, the fever presents itself with marks of inflammatory action of greater or less intensity; in others, that action is almost or totally absent. In some, the pathognomonic symptoms are combined with an element of malignancy and putridity which imparts a totally different aspect to the disease. In a different set this element is absent, and replaced by one of a nervous character. In some instances, phenomena not characteristic or pathognomonic of the disease, and depending on accidental complications, assume the predominance, and thereby impart still greater differences in the features of the case."—(*La Roche on Yellow Fever*, vol. i. p. 122).

In accordance with these varying characteristics of the diseases at different places, and in different seasons, or in different individuals, it has been divided into various forms. The division of all cases into the *inflammatory*, or those characterized by well marked reaction, and the *congestive*, in which this reaction is but faint or totally absent, would seem to be founded in nature. These general forms have been again subdivided, the first, into the mild, the violent, and the intense; the second, into the slight, the aggravated and the apoplectic. Under one or other of these divisions all the cases of yellow fever that present themselves may be, very conveniently, arranged.

The attack of yellow fever usually occurs in the after or fore part of the day—often during the night. It may commence abruptly, without premonitory symptoms of any kind. The patient may be struck down at once, as by a blow or lightning, and sink immediately into a state of coma. Occasionally, the attack is preceded for several days by anorexia, general uneasiness, costiveness, flatulence, heat in the stomach, lowness of spirits, vertigo, pain of the head, with dull and watery, or brilliant, yellow, or red eyes, &c. Very generally the attack is ushered in by a sense of chilliness, often alternating with glowing flushes of heat, or by a regular chill, amounting sometimes to a perfect rigor. On the other hand, in even severe and dangerous attacks, these symptoms may be absent.

Dr. Blair, (*Yellow Fever Epidemic of British Guiana*), considers that the most valuable premonitory symptoms of yellow fever, are supra-orbital pain and a punctated tongue; the first especially giving notice of an impending attack several days before it occurs.

The alternation of chilliness and heat is seldom of any duration. It soon gives way to confirmed fever, which, though continued, is more intense in the latter part of the day and during the night, than during the other portion of the twenty-four hours. In the more malignant forms of the disease there may occur but a slight reaction, or it may be entirely wanting; the pulse being feeble, soft, occasionally full, or scarcely to be felt—the patient sinking at once into a state of collapse, or of stupor, coma, and convulsions.

When febrile reaction becomes fully developed, the pulse is generally quick and

tense, and during the exacerbation full and strong, though, occasionally soft, and from ninety to one hundred and twenty in a minute. In very malignant cases it is gaseous. There is a violent throbbing and beating of the temporal arteries and carotids. The skin is hot, dry, harsh, and pungent, or it may be dry, unctuous, or perspiring, flabby and cold, except over the centre of the body. The face is either highly flushed, pale, or purplish. The eye decidedly red, sometimes as though blood-shot, hot, and more or less painful, the patient experiencing a sensation as though grit or sand had been introduced beneath the eyelids. Often the ball of the eye resembles a mass of vessels distended with blood; it is, at the same time brilliant, shining, and watery, in some cases presenting somewhat the expression peculiar to intoxication.

Dr. Blair (*op. citat.*) describes a *specific capillary irritation* that shows itself in the flush of the face, and which he pronounces to be as characteristic of yellow fever, as the hectic is of phthisis, or the fuliginous complexion of typhus. "This suffusion," he remarks, "generally occupies a zone over the eyes, and about an inch above and below them. The eyes are injected, like those of a person *just awake*, but generally without any lachrymation or photophobia, although the injection may be as intense as in ophthalmia."

Dr. Blair also remarks that, upon a careful examination of the chest, a subcutaneous rash may sometimes be observed, which occasionally extends to the arms and abdomen. The face, chest, arms and legs have likewise, in some cases, a slight purplish appearance after the second or third day, and sometimes the colour of a boiled lobster. This appearance varies much in degree, but may be detected by pressing the hand flat on the chest, when the fingers will for a short time be delineated in white with purple outline. The presence of this languid capillary circulation is, according to Dr. Blair, to be looked upon with favour. It would seem to indicate, he remarks, that the congestions were directing themselves towards the periphery of the body, thus relieving the internal organs from a part of their load. In these cases the skin is generally cool and moist, and sudamina occasionally appear.

Occasionally the condition of things described above continues from the onset of the attack to the close of the stage of reaction, or it may only partially exist during the first or second days. Sometimes profuse perspiration occurs and continues to the second or third day of the attack; at others, the temperature of the surface undergoes but little change; at others, again, the skin speedily becomes dry and cool, with complete torpor of its vessels, and an entire loss of irritability.

The foregoing symptoms are very generally attended with more or less intense pain, sometimes described as in the orbits, more rarely in the upper part of the forehead, shooting from temple to temple, but occasionally confined to one side, and in some cases extending to the occiput. This constitutes, in the majority of cases, one of the most distressing symptoms throughout the entire stage of reaction. It in most cases subsides spontaneously in the later stage of the disease. There is also pain, often of the most intense character, in the back, loins, and large joints—extending to the hips and down the thighs, or even lower. Cases occasionally occur, however, in which no such pains are experienced, or the pain in the back and limbs is dull and obscure, or that of the head is replaced by a sense of weight and stupefaction.

During this stage, the tongue is moist, covered with a thin, white, cottony fur, and most commonly red at the edges and apex; occasionally there is a soreness of the throat, sometimes even rendering deglutition difficult.

From the very onset of the disease nausea or other uneasiness of the stomach, with or without vomiting, is not unfrequently present. Generally there is a distension of the epigastrium, which is often, but not always, painful upon pressure. The stomach is irritable, especially after taking any kind of drink or aliment—it is frequently affected with a sense of nausea and more or less propensity to reject its contents. These gastric symptoms are not, however, in general fully developed until from twelve to twenty-four hours from the commencement of the attack, or at an early period of the second stage, when they become prominent.

On the first or second day of the attack, sooner or later, the patient experiences a burning pain, or a sense of stricture, weight, distension, or oppression—sometimes overwhelming—at the præcordia, which feels as if tightly bound with a cord. Tenderness or pain is experienced on pressure in most cases—it is often excessive. The irri-

tability of the stomach augments and proves distressing—every thing swallowed is rejected, and even when the stomach is undisturbed by drink or medicine its morbid contents are thrown off spontaneously—consisting either of substances that have been swallowed, mixed with clear, glairy mucus, or with matter of a sea-green colour and bitter taste. In mild cases, bilious vomiting sometimes occurs. The act of vomiting is often violent, and attended with retching and much distress and suffering. There is, at the same time, considerable, though not often insatiable thirst. The desire for cold drinks is nevertheless generally extreme.

In the early period of the attack, the matters vomited are, according to Dr. Blair, alkaline. When the second stage of the disease sets in, on the second, third, fourth, or as late as the fifth day of the disease, they become acid, and continue so to the close of the disease. The acid discharge may, at first, consist of a quantity of clear, pale, almost limpid, or slightly opalescent fluid. With this ejection the disease may terminate, or go on to a protracted period, without change in the matter vomited; usually, however, it is the precursor of the black vomit.

The urine is commonly deficient in quantity, and of a dark red colour; often depositing a copious sediment.

The urine is described, by Dr. Blair, as always acid in the first stage of yellow fever, and as continuing so generally until convalescence, when it becomes alkaline, or until it becomes heavily charged with bile. He confirms the accuracy of Dr. Collings' observations in regard to albuminosity of urine as a characteristic of the disease. The albumen generally appears on the second or third day: it has been found as early as the first day of illness, while, in a few cases, it did not appear till the day of death, and after black vomit had set in. In three cases the albuminosity was *intermittent* for one or two days. It was present in every fatal case of normal duration. It sometimes ceased in convalescence suddenly, always before the yellow suffusion of the skin and eye, or bile in the urine.

The turbidity of the urine in yellow fever is not necessarily connected with its albuminosity. The urine may be deeply tinted with bile, and highly albuminous, and yet clear. The turbidity is caused by the presence of mucous epithelial matter, coagulated albumen, and casts of urinary tubuli. It is probable that the free acid of the urine has a coagulating power, and thus sometimes causes turbidity. The presence of mucus will have a similar effect, but the turbidity then only occupies a lower stratum of the fluid, while the supernatant liquid is clear. Perfect epithelial scales are rarely found in the sediment, but broken epithelial matter is abundant. The albuminosity of the urine furnishes, according to Dr. Blair, one of the most obvious manifestations of yellow fever entering its second stage, and its extension to the great solid viscera.

A copious discharge of transparent urine, though ever so coagulable, and intensely tinged with bile, is always a favourable symptom. Scanty, oily-looking urine may almost invariably be received as a fatal indication.

The bowels are ordinarily costive—sometimes obstinately so. When stools are obtained the discharges are, at first, usually soft and feculent, seldom tinged with bile—occasionally they are of a drab colour. When cathartics have not been given, the stools, in the course of the disease, become lighter coloured, and assume often a starchy, cream-like or puruloid appearance. In a few cases they are watery, or even bloody, from the beginning of the attack.

The patient is affected with extreme restlessness and jactitation; he moans, sighs, and shifts his position continually in search of ease. In the very few cases in which jactitation is absent, the patient sometimes feels a disposition to rise from his bed, and walk about the room, his muscular strength remaining unimpaired to a degree unusual in febrile diseases. There is nevertheless, in numerous instances, from the onset of the disease, and during its entire course, universal debility.

In very many cases the patient complains of feeling as though he were unable to expand his chest or inflate his lungs. Spasmodic pains about the chest are not unfrequent. Respiration in some cases is laborious and hurried; in others, slow, and accompanied with deep and heavy sighing; in others, again, it is unaffected.

From the observations of Dr. Blair it is shown that the breath of yellow fever patients, especially in cases in which the urine is greatly diminished or suppressed,

has an ammoniacal odour. The more intense in proportion to the deficiency of the action of the kidneys.

The blood drawn in the early stage of the disease, and when there are symptoms of well marked reaction, and especially when any local inflammation is present, is sometimes of a bright arterial hue, sily, and even cupped, as in ordinary phlegmasiæ. In a very large number of cases, however, it presents neither cupping nor buffy coat, and the coagulum is flabby and easily torn. When the separation into crassamentum and serum takes place, in some cases the latter is of a natural colour; in others, it is of a yellow hue, or slightly tinged with red, and transparent; in other cases, again—more frequent in some epidemics than in others—the separation does not take place, the blood remaining for hours, or altogether the same as when first drawn. In a few cases, when drawn later in the disease, or throughout its course in cases unattended with reaction, the blood is of a dark colour, void of all inflammatory indications, and not unfrequently as fluid as molasses; while in other cases, again, it is smeared over with a pellicle of sily lymph, at the same time that the part lying at the bottom of the vessel is dissolved. In the early stage of the inflammatory form of yellow fever, the blood is very hot, and has a peculiar odour, which, according to some accurate observers, is supposed to furnish a sure indication of the true nature of the disease.

At first, the patient attacked with yellow fever is apprehensive and anxious to a distressing degree, as strongly expressed by his countenance. In very malignant cases there is an expression of apathy, or one indicating a sense of horror or intense agony. In most cases, there is some confusion of intellect attended with constant pervigilium, though without so much derangement of the reasoning faculties as to amount to decided delirium. In some cases, however, the latter symptom assumes a more marked character, the disturbance of the mind reaching to the degree constituting mania, attended with wild or fiery looks, and uncontrollable agitation of body. In other instances, there is a greater or less degree of stupor, through which, as Dr. Wood remarks, when short of coma, the signs of distress show themselves as through a veil. In not a few cases, but particularly in young persons of both sexes, and in females at two different periods of life, hemorrhages take place from one or both nostrils, during the afternoon exacerbation.

The stage of febrile reaction continues with little or no mitigation during a period varying from a few hours to two, three, or more days—the duration being commonly in inverse ratio to the violence of the attack. Having run this course, the fever, with all the attending symptoms, subsides, never more, or at least very seldom, to return—the disease being one of a single paroxysm—a period of remission or metapostosis, ensuing, during which the several organs of the system resume their normal functions. The patient feels himself able to sit up, or even get out of bed. His eyes and face become tinged with yellow, or copious evacuations of bilious matter occur by stool, or a gentle or profuse perspiration sets in, or often, without any such critical signs, convalescence is established, and the patient speedily recovers.

This, however, is the course only of the most favourable cases. In the larger number, the period of remission which follows the first stage is only temporary, and is soon succeeded by other phenomena of a more formidable character. During the very period of deceitful calm, symptoms may be detected, denoting the existence of undiminished danger. The tenderness of the epigastrium is unrelieved or even increased; the eyes and face usually acquire a yellow or orange colour, which gradually extends from the forehead to the face, neck, chest, and, finally, diffuses itself more or less generally over the whole surface. The urine also, diminished in quantity, is found tinged with the same yellow hue, or so intensely coloured as to assume a dark mahogany, or almost black appearance. The pulse is sometimes slower than in health, and in bad cases the patient betrays a little heaviness of intellect or stupor.

After a period, varying from a few hours to twenty-four or more, the symptoms just enumerated become aggravated, and others are added. In the majority of cases, the pulse remains natural, or slower than in health, and becomes still slower as the disease advances, until at length the pulsations are reduced to forty or even thirty in a minute, being, at the same time, feeble and irregular. The heart, nevertheless, even in the most malignant cases, often beats with considerable energy, even after the pulse has ceased to be felt at the wrist, &c. The tongue becomes loaded—particularly in the centre—moist or dry, and with or without redness of the edges. Thirst

increases, and is often insatiable, — nausea and vomiting, with heat in the stomach, return, and become constant, — the matter ejected being mixed with streaks or flakes of a red or brownish colour. Respiration quickens, or becomes embarrassed — the skin becomes cool, dry, and parched — the anxiety at the præcordia is now distressing, and attended with a sobbing kind of sighing, constant hiccough, and, occasionally, an expression of deep anguish and despair.

The mind often remains clear and undisturbed. There is generally an extraordinary degree of apathy evinced — the countenance presenting an expression of resignation or indifference as to the issue. Sometimes the patient evinces a kind of cheerful delirium, imagining himself well; in other cases, without delirium, he remains for a long time as if in a deep reverie, and when aroused, starts with surprise, and answers in a hurried manner. Many, while apparently in great distress, declare that they are well.

As the disease progresses, coma supervenes, from which the patient is aroused by vomiting or by dreams, and fancies himself perfectly restored to health — attempts to rise, but soon relapses into a state of insensibility. In many cases, debility is more or less considerable. In not a few, however, the patient retains, to a late period, his muscular strength — to such an extent, indeed, that he will get out of bed and walk about his room, or even beyond that, if permitted. The physiognomy is peculiar and striking, conveying, as has been well remarked, an impression at once of the malignant and dangerous nature of the disease.

Usually the foulness of the tongue increases, though it is not uncommon for it to become, after a short time, cleaner and moister than before. It is sometimes tremulous and protruded with difficulty: when the patient succeeds in showing it, he not unfrequently forgets to put it in again. It is brown and dry in the centre, or smooth, red and chapped, or white at the edges, with a black streak in the middle. The gums, lips, teeth and nostrils are covered with sordes.

As the case advances, the vomiting becomes less frequent, while irritability of the stomach, which rejects everything introduced into it, continues undiminished. When vomiting does occur, however, there is an increase in the quantity of the matter ejected, and to the momentary relief of the patient. The matters ejected from the stomach are often thrown out forcibly and to a considerable distance.

From the condition just described, recovery not unfrequently takes place — generally by a gradual receding of the symptoms, but sometimes by an evident critical revolution — the pulse acquiring force and activity, the skin becoming warm and moist, the urinary discharge becoming free and copious, the irritability of the stomach lessening and finally disappearing.

In other cases the symptoms are of a still more formidable character. The matter thrown up by vomiting consists of brown, blackish, or chocolate flakes or particles, diffused in a colourless liquid, which, though at first slightly tinged by them, ultimately becomes black and opaque, resembling coffee-grounds floating in a serous fluid. In some cases, grumous dissolved blood is thrown up. The matter vomited is acrid, often excoriating the throat, tongue and lips.

Normal black vomit is described by Dr. Blair (*op. citat.*) as having a laminar or granular sediment, of a deeper or paler shade of brown, sometimes verging on jet black, with a clearly defined supernatant serum of low specific gravity, and without mucosity, partaking of the colour of the sediment, but sometimes nearly limpid when the sediment is black, as if all the colouring matter had subsided. Many deviations from this standard occur from the presence of ingesta, hæmorrhage, and excessive secretion of mucus.

The sediment of black vomit would appear to consist of coagulated albumen and the débris of blood cells. In no case in which the black vomit was normal to the eye, was a single perfect corpuscle observed.

The colour of the black vomit, when this is pressed through a paper filter, is rendered considerably paler. It is decidedly acid — the sediment more so than the supernatant liquid. The sediment acts as a ferment on fluids containing sugar.

The presence of ammonia in black vomit, according to Dr. Blair, is universal: that is, it has always been found when looked for, and may be considered one of the tests of black vomit.

Although fatal cases of yellow fever may occur unattended with vomiting of black

matter, it is a common attendant upon the disease at the period indicated, and always portends the most imminent danger, for, though some recover after its occurrence, the number is very limited.

During the epidemic of yellow fever in British Guiana, preceding that of 1852, it was noticed, Dr. Blair informs us, that when the black vomit *preceded* the yellow suffusion of the skin, the prospects of life were improved. "The relations of this fact were not then understood. Black vomit is significant of imminent danger, from the circumstance that it is the *dernier ressort* of nature to relieve that contamination of the circulation which has been produced chiefly by impairment of the function of the kidneys, and the retention thereby within the system, of the worn-out nitrogenous elements of the body and their poisonous metamorphoses. Now, if black vomit appear early in the disease, before its march has extended to the great internal viscera, before the bile function has been disturbed, or the urine rendered albuminous, it ceases to be the significant symptom which has obtained so much ill-omened celebrity. It is then the sign of a local, instead of a constitutional affection."

The abdomen is soft, seldom meteorized. The stools, when they occur, present the same character as the matter ejected from the stomach, or resemble tar or molasses, or they may consist of blood, more or less pure. The urine becomes natural in appearance, or of a dark colour, and limpid. It is often suppressed, from deficiency of secretion, or simply retained. Sleep is interrupted, and attended generally with painful dreams. The face and breast become spotted as with ink. The jaundice—which although so common an attendant of the disease as to have given it its name, is nevertheless often, especially in rapid cases, totally absent—becomes more diffused and of a deeper hue. The skin assumes often a deep, dusky yellow, or brown, mahogany, bronze, or purple hue, imparting the idea of blood settled in a bruised part. The blood in the capillaries becomes stagnant, forming petechiæ, vibices, or large blotches, and accumulates in depending parts, and the extremities. In many cases it oozes from the nostrils, tongue, gums, anus, eyes—from leech bites, blistered surfaces, and the punctures of the veins made in venesection, and is, like all the blood in the vessels, dark coloured and dissolved.

As the disease advances, these symptoms increase in intensity; hiccough sets in, and is soon constant and accompanied with the hippocratic countenance, with difficulty of swallowing, and slow and stertorous convulsive respiration. The pulse becomes small, feeble, intermittent, and finally fades away. The alvine evacuations are highly offensive, of a cadaverous smell, and, like the urine—which, if at all secreted, assumes a blackish and bloody appearance—are voided involuntarily. Subsultus tendinum not unfrequently follows; so also gangrenous spots, and, in a few cases, buboes, carbuncles, and eschars in various parts of the body. Loss of speech, dimness of vision, insensibility, low muttering delirium, and coma, at times supervene; but it is not unusual to find patients retaining their intellectual faculties unimpaired to the last. Rattling in the throat, cold clammy sweats, cadaverous and peculiarly offensive odour of the body, cold respiration, are the immediate precursors of death, which often occurs quietly, but, in other instances, in the midst of violent convulsions.

The duration of the disease varies, according to the nature of the case, from three to nine days; sometimes it is shorter, at others longer; while, in cases of recovery, the convalescence is usually secure and rapid.

Dr. Blair (*op. citat.*) states that, during the epidemic, as it occurred in British Guiana, in the years 1852 and '53, relapses were of frequent occurrence, occasioned, most likely, in a great measure, by a return of the patient to the focus of infection. These relapses were met with, almost exclusively, among the aborted cases. They frequently recurred, and were aborted several times. The primary attack was generally, but not always without albuminosity of urine, and frequently the relapse also—generally, however, in the relapse, albuminous urine was expected—even if the disease was again aborted during convalescence. Only two relapses occurred after the disease had run on to black vomit, both of which were readily aborted, although in one the relapse proceeded to albuminous urine. The tendency to relapse or second attack was generally within the first month after the primary attack.

In the foregoing account of the symptoms of yellow fever, considered in the aggregate, without reference to the particular groups in which they may present themselves,

in different cases, from the slight ephemeral to the most intense inflammatory grade, and in the several grades of the congestive form, we have followed very closely the admirable delineation of the disease given by Dr. La Roche in his late invaluable treatise on the yellow fever, using frequently his very words.

Following the author just referred to—and we know of no better guide—we shall now rapidly sketch the features of the disease as exhibited in the several grades of its inflammatory and congestive forms.

In the more intense grade of the inflammatory form, we have the initiatory chill—a of a more or less decided character, followed by intense febrile action—a quick, frequent, strong and full pulse; hot, and usually dry, parched skin; violent throbbing of the temporal and carotid arteries; flushed face; red, blood-shot, brilliant, shining, watery eyes, with a sense of pain or soreness in the balls; sometimes tumid eyelids; intense pain in the supraorbital organ, in the back, thighs, and legs; tongue usually crimson red at its edges and apex, and covered with white or yellow fur; a sense of anxiety, constriction and intense pain at the præcordia; nausea, succeeded by retching and vomiting, at first of the matters swallowed, mixed with a clear, glairy mucus—occasionally with a sea-green coloured substance of a bitter taste—now and then of pure bile—often, distension of stomach, which is sometimes painful on pressure, and generally irritable to an extreme degree, especially after the first twelve or twenty-four hours of the attack. The urine is deficient in quantity, high coloured, and often albuminous and sedimentitious. There is obstinate costiveness—the stools, when obtained, being at first soft and feculent, sometimes tinged with bile; when no cathartics are used, they become, subsequently, lighter coloured and of a starchy, cream-like appearance. There are considerable jactitation and restlessness, with moaning and sighing—and a disposition to rise from bed and walk about. The respiration is laborious and hurried. There is an anxious, gloomy, sad, or impatient expression of countenance. The patient experiences great apprehension—there is confusion of intellect, constant sleeplessness. Delirium, properly speaking, is generally absent; occasionally, however, it is present, even from an early period of the attack, reaching sometimes to a degree amounting to actual mania.

The stage of reaction lasts from a few hours, to two, three, or more days—generally from sixty to seventy-two hours. It is succeeded by a remission of all the symptoms. The patient becomes at once cheerful, sits up or gets out of bed, and expresses a desire for food. The adnata of the eyes now usually assume a yellowish tinge.

The remission, which is too generally a deceitful calm, may continue from a few hours to twenty-four or thirty, and then gradually glide into the second stage. Prostration follows; the pulse becomes rapid, irregular, and depressed, or more generally, it is natural in frequency or even slower than in health. The tongue becomes loaded with a brown fur, having a darker streak along its middle, swollen, and moist, or, frequently, it is clean, with a slight pasty coating, or it may be of a deep fiery red, and, occasionally, in an advanced period, it is bloody, or dry, black, and chapped; with dark coloured sordes on the mouth, lips, gums, and nostrils. The respiration is quick and laborious; there is augmented and insatiable thirst; a distressing sense of anxiety at the præcordia, accompanied, often, with hiccough and sighing. There is an augmentation, in many cases, to an intolerable degree, of the pain at the epigastrium, which is aggravated during the vomiting that now occurs almost spontaneously—the contents of the stomach being forcibly ejected and to some distance—the matters discharged consist of brown, blackish, or chocolate flakes or particles, diffused in a colourless fluid, and gradually acquire, in fatal cases, the characteristics of black vomit. Occasionally, involuntary discharges occur from the bowels, of a black, acrid, offensive matter, sometimes resembling tar or molasses; at others the stools consist of blood. A yellow tinge, which appeared at first about the forehead and eyes, extends, subsequently, to the face, chest, and, finally, over the entire surface, gradually acquiring a deeper hue, the skin assuming a dusky, brown, mahogany, or bronze colour. The jaundiced hue of the skin is sometimes, however, absent, or restricted to the adnata of the eyes, or appears only after death. The mind is often clear and undisturbed to the last—frequently the patient exhibits a degree of apathy, with an expression of resignation and indifference. In other cases various modifications of delirium are present. Frequently, there is more or less debility; in perhaps the majority of cases, however, the patient regains his muscular strength, if

he had previously lost it, and retains it to the last. The body, in the progress of the disease, becomes cold and clammy; the urine blackish or bloody, and is often passed involuntarily; more generally it is suppressed or retained. Hæmorrhages of dark, dissolved blood, occur from all the natural outlets, and death, preceded by intolerance of light, petechiæ, meteorism, singultus, eructation of offensive gas, subsultus tendinum, convulsions or coma, closes the scene.

The rapidity of the fatal termination in this grade, is, in general proportioned to the violence of the inflammatory action of the second state, by which, in its higher degree, the vital organs may become rapidly overwhelmed and disorganized, to a degree incompatible with the continuance of their functions. Recoveries seldom occur in this form and grade of the disease; when they do, it is by a gradual amendment of the symptoms, or some critical movement taking place before the accession of black vomit.

In the milder grade of the inflammatory form, the symptoms of the first stage are pretty much the same as in the more intense grade, but less violent, rapid, and tumultuous. It is attended by more decided exacerbations and abatement of the fever—the abatement often approaches to an actual remission. It often extends to four or five days. The second period or that of remission is more perfect, and in many cases the harbinger of recovery—convalescence dating from the final subsidence of the fever—the disease terminating with bilious critical discharges by the bowels, a moisture over the skin, or diaphoresis, a copious emission of urine, or a hæmorrhage from the nose—with or without jaundice—often without any evident crisis.

In other cases, however, the remission is followed by many of the symptoms which mark the second stage of the preceding grade. These sometimes assume a character of great malignancy, and if not arrested by art, or the recuperative efforts of nature, terminate in death. In other cases, they stop short of black vomit, and the patient is gradually restored to health. In others again, though in a very small number, recovery ensues, even after the supervention of the black vomit, and other usually fatal symptoms. The disease in this grade is evidently of a less malignant character, and within the range of remedial agencies. The bowels are acted upon without much difficulty by cathartics or enemata, the operation of which is productive of relief, the pain and affection of the head and other parts are under the control of proper depleting remedies, general and local, revulsives, &c.; the gastric irritability, though obstinate, is not always as uncontrollable as in the more intense grade, and diaphoresis is generally easily obtained by external and internal means.

In the ephemeral grade of the inflammatory form, although there are the same general features, the same outline of phenomena, as characterize the preceding grades, they are of a still milder and more manageable nature—and terminate, under proper and even mild treatment, sometimes in a single day. Occasionally, however, the fever continues from three to five days, when, in some cases, it is attended with slight and imperfect remissions.

The symptoms indicative of an open and well developed febrile paroxysm subside, sometimes, suddenly; at others, the crisis being marked by increased alvine evacuations, by diaphoresis, or epistaxis.

In the aggravated grade of the congestive form, the attack occurs suddenly. From the outset there is considerable prostration. In most cases, from an early period, there is giddiness, stupor, almost unconquerable disposition to sleep, loss of memory, and a desire to be left alone. There is a sense of weight and oppression, rather than of acute pain in the head. In a few cases, there is delirium, either transient, or ending in confirmed coma. The face is pale, purplish, or livid in colour, with a stolid or apathetic expression of countenance—the patient being taciturn, and uttering no complaint. Sometimes there is entire insensibility, the eyes being wide open, at others there is an expression indicative of distress, horror, or even intense agony.

There are obscure pains of the loins and extremities, and a feeling of helpless debility about the spine—most distressing at the sacrum—sometimes attended with a paralytic failure of the lower extremities. There is a dull, red, glassy, or drunken, idiotic look of the eyes, with, in some cases, a dilatation of the pupils, and sleepy movement. The skin is always deficient in tone, dry, dense, or unctuous, or sometimes covered with, and as if melting in, sweat. It is generally cool—sometimes cold, except at the central portions of the body, which are hot; in some instances it is

smooth and white, and occasionally loses, more or less completely, its sensibility and irritability. The pulse is sometimes accelerated, at others not more frequent than in health; sometimes full, at others small. It is always weak, offering no resistance to the pressure of the finger; occasionally it is scarcely perceptible at the wrist, though at the same time the heart and carotids may be throbbing forcibly. As the disease advances, it diminishes in frequency, the beats not amounting, at times, to more than forty, or even thirty in a minute. In some cases it becomes intermittent. When blood is drawn, it is generally found black or discoloured; it seldom retains its natural character.

There is tenderness of the epigastrium, tension of the hypochondria, weight and oppression at the præcordia. There is early irritability of the stomach—and vomiting—the matter ejected rapidly assuming the character of black vomit. The discharges from the bowels are scanty—cream or clay-coloured, puruloid, or gelatinous—sometimes of a pea-green colour, or black and bloody. The respiration is laborious. The tongue, sometimes natural, is at others, first pasty, with patches of white fur; its edges and apex being red. Occasionally, it appears as though seared with a hot iron. It is often tremulous, and when the patient puts it out he often forgets to draw it in again. It sometimes becomes dry, while the papillæ are separated by deep fissures. There are, also, orthopnoea, sore throat, deep and interrupted sighs, hæmorrhages of dissolved blood from one or more of the natural outlets, a yellow or bronze colour of the skin, suppression of urine, extreme restlessness, low, monotonous wailing, and other symptoms indicative of the utmost danger, or the approach of death.

In some cases, the leading symptom is an overwhelming oppression at the præcordia, attended with slow, laboured respiration, deep sighs, and groans. In others, constant vomiting, and intense epigastric distress, quickly followed by black vomit and death; in other cases, again, the pulse is nearly natural, the tongue clean, and the stomach calm, but excessive restlessness, anxiety and distress ensue, soon followed by black vomit and fatal collapse.

In some instances, the disease, though marked by the same train of phenomena, assumes, nevertheless, in its course, a less formidable character, stops short of the black vomit and other fatal symptoms, and proves comparatively mild and manageable. As a general rule, however, but very few of those attacked with this form of the disease recover.

The adynamic or typhoid grade occurs in persons deficient in vital power, or under circumstances tending to foster or develop the typhoid diathesis. It is generally ushered in by a sense of chilliness, succeeded by one of burning heat, partially distributed over the body—affecting principally the under parts of the arms, and inner surface of the thighs. The circulation is depressed, the pulse being small and weak. The eyes have a dingy appearance. There is severe pain of the head, with confusion of thought and dimness of vision. The skin assumes an olive hue, and is covered with petechiæ or vibices. Hæmorrhage from the natural outlets, leech bites, &c., follow, as also from excoriations about the nose, mouth, or other parts; gangrene of blistered surfaces, sometimes anthrax, buboes, and, more frequently, venous infiltration under the skin, or in the interstices of the muscles.

The walking grade of yellow fever is so named from the fact that in it the organs of animal life remain almost unaffected. The patient most frequently sauntering about his room, or, at times, even walking in the streets for recreation or on business. In some instances he confesses to a feeling of weakness, but, in others, he exhibits at intervals, or throughout, indications of considerable muscular strength. He complains of nothing, denies his being ill, amuses himself in reading or otherwise, and to a casual observer, appears to be slightly, if at all, indisposed. The physician will be able, however, generally to observe that the patient exhibits an unusual expression of countenance—dulness and listlessness. The eye is watery—the complexion almost of a mahogany hue—the pulse exceedingly weak, or even totally absent. Black vomit overtakes him, even while occupied as described, or very soon after, and death speedily ensues.

In some of the cases of the apoplectic grade, the patient is struck down suddenly as if by lightning, with stupor or coma, and death, preceded by convulsions, soon follows; or, without the slightest premonition, he is instantly seized with vertigo and confusion of mind; accompanied with dull pain and fulness in the head—spasmodic

pain and considerable debility in the legs—coldness, debility, and a sense of uneasiness in the spinal region—a pulse varying, in different cases, in fulness and frequency, but always weak and finally faltering,—a cold skin, sometimes dry and flabby, but generally unctuous or bedewed with cold perspiration—and irritability of stomach. The patient lies as if stunned; with dilated pupils and an expression of gloom on his countenance. An effort at reaction occasionally takes place—but scarcely ever leads to a favourable result. More generally, the patient becomes perfectly comatose, the eyes assume a glassy appearance, the pulse fades away, involuntary discharges and profuse hæmorrhage supervene, and death soon ensues.

“The yellow fever is far, however, from pursuing, always, everywhere, and under all circumstances, the even tenor of its course, without experiencing more or less important modifications from other causes which may operate on the system in conjunction with, or antecedently to, that by which it is produced; while the special morbid agent which gives rise to it, seldom fails to modify, to some extent—when it prevails extensively and with great virulence—other diseases arising from the impress of other causes. Hence arise, on the one hand, numerous complications of the fever with complaints of various kinds; in other words, cases in which, to the symptoms of the fever, are added others indicating the coexistence of some other disease, which owes its origin to the operation of distinct causes; and, on the other hand, those modifications of prevailing complaints occasioned by an impress of the yellow fever cause, which, though not sufficiently powerful to produce the fever to its full extent, is enough so to stamp those complaints with some of its features, and, in the language of Dr. Rush, to make them wear its livery.”—*La Roche, op. citat.*

Dr. Blair describes four distinct causes of death in yellow fever; they may, however, be blended occasionally in the same case. They are syncope, uræmia, apoplexy, and asphyxia. Death from syncope may be owing to excessive discharges of black vomit, or from hæmorrhage, as profuse epistaxis, or bleeding from the mouth and gums, or from the black vomit and hæmorrhage combined. If before death there is entire suppression of urine, and the black vomit is not copious or has ceased, the circulation becomes contaminated, and in its mildest form the action of the impure blood upon the brain, produces an effect not unlike alcoholic inebriation. Patients, a few hours preceding death, have been known to sit up in bed, and joke with their comrades, or in a chair regaling themselves with a pipe of tobacco. When all the secretions and excretions are locked up, as occasionally happens, the symptoms of uræmic poisoning are of a more violent character; the sensorium is painfully affected—irritability of temper, screams and wild ravings ensue, followed by convulsions, coma, and death.

Death by apoplexy may occur as the result of congestion and extravasation of blood on the brain. Death from asphyxia may occur from excessive pulmonary engorgement, or from laryngeal suffocation.

In proceeding to consider the pathological anatomy of yellow fever, we may remark in the outset, that cases occasionally present themselves, especially when the disease has proved suddenly fatal or has run a very rapid course, in which no appreciable lesion in any of the organs or tissues can be discovered on dissection, or lesions of too slight a character to permit us to refer to them any agency in the production of the phenomena of the disease, or its fatal termination. In the great majority of cases, however, morbid changes, often considerable, are to be detected, by which the disease can usually be identified, while they throw more or less light on its pathology.

The surface of the body in general presents a yellow colour, varying from a pale to a dark orange or brown. In many cases it has a greenish, or mahogany, or leaden hue, or even a purple or black aspect. The lighter shades of colouration, are usually observed in subjects carried off rapidly and by an inflammatory attack, the others in such as have fallen victims to attacks of a malignant or protracted character. Sometimes a pale yellow line, mingling with the other colours, can be traced from the nose to the pubes. The discolouration may be confined to the face or eyes, neck or chest, or extend over the entire surface. It is generally deeper on the face and trunk than on the extremities. It is more intense and general after than before death; it may even not have appeared previously.

The scrotum, penis, fingers, toes, and ears, which, especially a short period before dissolution, are often very much discoloured from stagnation of blood, become, as do

also the back and neck, of a dark purplish hue. These are cadaveric effects. Ecchymoses, in spots of different sizes or shape, round or in stripes, sometimes occupy the forehead, upper portion of the face, as also the trunk and extremities. These latter are the results of disease, and appear before death. The surface is, sometimes, also covered with minute ecchymoidal spots, bearing some analogy to petechiæ, and increasing in number after death.

In many cases the cellular membrane and fat are found to partake of the yellow colour of the skin. This occurs less frequently in malignant and congestive, than in ordinary cases of the disease. Extravasations of blood in the subcutaneous cellular membrane, and between the interstices of the muscles, are not uncommon. In some cases, livid and gangrenous spots occur on portions of the body.

The joints and muscles are generally rigid and stiff; and the latter, in those who have died of the malignant form of the disease, are often of a dusky or dark hue. They are generally softened in texture and easily torn or broken down by pressure. Sometimes, especially after ordinary and inflammatory cases, they are but little, if at all changed in colour and firmness. They are occasionally pale, as if they had been submitted to prolonged maceration.

The face, in some cases, is tumefied, in others shrunken.

The brain is often found entirely free from diseased changes. Traces of inflammation are occasionally, however, detected in it or its membranes, but have no direct relationship to the yellow fever. In many cases, the pericranium, the sinuses, and the vessels of the brain are more or less gorged or congested with blood. In some the membranes alone, or together with the brain, are injected throughout or in patches; and in many, a fluid of a limpid or yellow colour, or mixed with blood, is effused in the ventricles, at the basis, or on the surface of the brain, or in its membranes.

Nearly the same remarks may be made in reference to the lesions detected in the spinal marrow. When unequivocal traces of inflammation of this part or its membranes are present, these must be referred to some accidental complication, and not as forming an essential part of the disease. Many of the changes met with in the spinal marrow after yellow fever, are probably to be referred to mere congestion, and the hæmorrhagic tendency which constitutes a main characteristic of the disease.

Various morbid appearances of the ganglia and ganglionic nerves have been described as occasionally met with after yellow fever. No one of these are invariably present—all of them are frequently absent—they often exist in subjects who have died of diseases having not the least resemblance to yellow fever; hence when present they are not to be viewed as among the true anatomical characters of the latter.

In general, the respiratory organs present no appearances indicating that they partake largely and necessarily in the diseased action of the system in yellow fever. The lungs are often found, at their posterior or lower portions, more or less gorged with dark coloured and altered blood. They are frequently in parts or throughout, black, resembling a sponge; in substance sometimes firm and dense, not unlike the substance of the spleen, or gorged with blood, black and dissolved, and do not collapse upon the removal of the sternum. Not unfrequently their surface is covered with melanic patches or ecchymoses of from two to five lines diameter, or masses of a black colour, impermeable to the air.

The bronchial mucous membrane is in general free from disease; sometimes it is injected, or spotted with blood, or even inflamed. The pleura is usually unaltered, the ecchymoid spots noticed upon it being situated in the cellular membrane beneath, or in the substance of the lungs. In some cases the pleura has been found inflamed, or containing more or less effused serum of a yellowish, orange, or reddish colour, with or without marks of inflammation. In a few, the fluid is of a sanguinolent character, and resembles, more or less closely, the black vomit.

The substance of the heart, like that of other muscles, is sometimes of a dusky colour, and soft and more flabby than natural, and easily broken down by pressure between the fingers. In perhaps the greater number of instances, however, the organ retains its natural appearance. The pericardium, which generally appears healthy, sometimes contains a notable, though not unusual quantity of serous fluid of a yellow or reddish colour. The endocardium, in some cases, is slightly red, apparently the effect of staining. In many others it is, as all the fibrous parts, the valves, &c., of a

yellowish colour, which often extends into the aorta and the larger vessels. On the surface of the former, spots closely resembling petechiæ, are sometimes observed. The cavities of the heart contain a greater or less quantity of blood, usually dark coloured, and for the most part grumous or fluid, with or without coagula of the same colour. In a large number of cases these cavities—especially the ventricles—contain albuminous concretions, varying in size and consistency, and of a transparent yellow colour, having the appearance of meat-jelly or fine amber. They penetrate sometimes into the aorta.

The stomach is the organ most generally and seriously implicated in yellow fever—in it indications of disease are most frequently discovered after death. Externally it is sometimes of a yellow colour, but generally retains its normal appearance. It is usually found to contain more or less of matter similar in appearance to the black matter thrown up by vomiting in the latter stages of the disease. This matter has been shown by recent investigations to be blood—most probably diseased in character—poured out by the capillary vessels of the digestive mucous membrane, and still further changed by the action of the acid it meets with in the cavity of the stomach. In some cases, the contents of the stomach consist of blood more or less pure, with or without the coagula, and generally combined with a portion of glairy matter, and substances swallowed a short time before death. In some instances, the mucous coat is smeared over with a dark, adhesive jelly-like substance, containing portions of blood. Under this substance, and sometimes when it does not exist, we find a layer of greyish matter, not unlike a mixture of linseed meal. In a certain number of cases, the mucous coat, when cleansed from these various coatings, is found to present a normal appearance. In many instances it is even whiter than in its normal state, from the effusion from its vessels, in all probability, of the blood with which they had been loaded, in the form of black vomit.

In the greater number of instances, however, the mucous membrane of the stomach is found more or less diseased, indicating that it had been the seat of inflammation more or less extended and of different grades of intensity. Thus the stomach is sometimes contracted, at others distended. The longitudinal rugæ are enlarged. The surface has often a vermicular appearance, being corrugated and thrown into numerous folds. Its capillary vessels are injected to a greater or less extent with blood. It is reddened, presenting various shades, from a rose to an intense dark hue, or it may be leaden, livid, or even nearly black. The discolouration being either uniform in appearance or in the form of arborizations—it may extend over the greater portion of the mucous coat, or be confined to the cardiac or pyloric orifices, the large curvature, presenting itself in patches different in number and size in different cases. Streaks or spots of a purple colour, spread in various directions over both the altered and healthy parts. The spots, differing in size, have the appearance of ecchymoses. At other times, with or without these, there are numerous small, dark red, or violet round spots, resembling petechiæ, contrasting very decidedly with the rose hue of the mucous membrane upon which they are scattered. The lining membrane may, besides, present abrasions, or small depressions or pits, like holes or furrows, as though a portion of the tissue had been removed. It is frequently mammilated even to a remarkable degree, also, more or less considerably thickened and opaque. It is sometimes softened, and easily detached, especially about the great *cul-de-sac*. In a few cases it is ulcerated, or presents a gangrenous change. Sometimes, more especially after malignant or congestive attacks, an effusion is discovered under the mucous tissue.

“These changes are not all found in the same cases, nor are they present at whatever period of the disease the patient may have succumbed, or whatever may have been the character and duration of the disease. In congestive or malignant cases, we find more or less injection of the capillaries—the redness generally of a dark hue. There are usually ecchymoses, and petechiæ, but no thickening, softening, or similar changes. These, together with capillary injection, are, as a general rule, met with after attacks of a different character. In these, if death takes place on the second, third, or fourth day, the increased vascularity is noticed in bright red, or dark, dusky patches, more generally confined to the vicinity of the orifices, but sometimes extending to the greater part of the membrane. If the case has been protracted to a later period—to the sixth, seventh, eighth, or ninth day—a larger portion of the surface is

found involved, and we may expect to find it of a leaden, livid, greenish, or mottled appearance, and presenting the marks of disorganization already noticed. In instances unattended with these changes, in which there is mere redness, with ecchymoid and petechial spots, these are probably not the effect of cadaveric changes, for they are found immediately after death, and too soon to be attributed to such cause, they must rather be referred to simple congestion. But whenever this redness is attended with thickening, or softening, or the mammilated appearance of the membrane, we cannot but join in opinion with Louis in attributing these changes to an inflammatory condition of the parts," (*La Roche*). The appearances discovered in the cesophagus and intestines do not differ materially from those exhibited by the stomach. The intestines, when the case has been rapid, contain often bilious, yellow or ordinary excrementitious matter—at other times their contents are brown, black, thick or jelly-like, often of a tar-like appearance—or they may be fluid, of a reddish or soot colour, or even consist of blood, more or less pure. Sometimes they are whitish, and often present the characteristics of the black vomit. The intestines, in a few instances, are contracted to a greater or less degree, and with more or less force. Extensive invaginations are occasionally observed. The duodenum and upper portions of the jejunum are the parts most generally affected, though, in other cases, the lower portions of the ileum are more implicated than the latter.

The glands of the intestines, especially those of Brunner, are occasionally found in a diseased or abnormal condition.

The gall-bladder is either empty, diminished in size—withered, as it were, or distended, with its usual amount of bile more or less natural in quality—or its contents may be small in quantity, viscid, inspissated, or mixed with more mucus than common. It is either dark green, blackish-brown, or of an obscure red colour, and of the consistence of tar. Not unfrequently the gall-bladder contains a quantity of thick viscous blood, grumous, tar-like, or ink-coloured, or of serum, and more rarely of pus. Its internal membrane is often spotted, or punctated, and sometimes largely injected with blood of a bright or obscure red or brown, or even dark colour. It is said often to present traces of unequivocal inflammation.

The liver is usually of a light yellow, nankeen, fresh butter, straw, coffee and milk, gum yellow, buff, gamboge, light orange, or pistachio colour. In some cases, this discolouration occupies the whole surface, and pervades the entire parenchyma of the organ; while, again, in others, it extends only partially over both, giving a marbled appearance—presenting throughout patches or regular striæ, and alternating with others of a dark green colour. It is limited occasionally to a single lobe, usually the left. Recent observations would seem to show that this discoloration is due to a fatty degeneration of the organ.

"Frequently, however, as this peculiar coloration of the liver has been observed, it is far from being universally so; cases occurring in which the organ is found of a different hue—dark yellow, brown, red, purple, bluish, slate, chocolate, or livid. It has been described as of a brick colour, and compared to rhubarb, or to Peruvian bark. In other cases, again, it retains its natural appearance externally and internally, and is otherwise healthy. The parenchyma, when divided, is often found hard, dry, tough, and sometimes dry and brittle, and more or less devoid of blood; while, in some cases, the viscus is more or less gorged with blood, and softer in texture than natural. In some cases, the biliary pores contain bile, but more frequently there is no indication of biliary secretion."—(*La Roche*).

Though often more or less enlarged, and at other times shrunken, the liver seldom exhibits traces of inflammation, and, if these are discovered, they must be viewed simply as the effect of complications; while the alteration in, or suppression of, the secretory function of that organ, may justly be referred to some cause different from that morbid state.

The kidneys are occasionally found in a normal state, or with only trifling marks of having partaken of the disease. In other cases they are congested—filled, more or less, like other organs, with dark fluid blood, the mucous membrane of the pelvis and infundibulum being sometimes minutely spotted with blood, or ecchymoses; or they exhibit a morbid condition similar to that observed in Bright's disease. In other cases they bear the marks of acute inflammation.

The bladder is often contracted; sometimes its coats are thickened. The mucous

coat is generally healthy, or only injected and dotted with small points, or ecchymosed. In some cases it is covered with a yellow mucus. The bladder is often empty, or nearly so—it may, however, contain more or less urine, natural in appearance, bloody, or more or less bloody. Occasionally it contains black matter resembling that ejected from the stomach, or pure blood.

The spleen is usually of a darker colour than natural—sometimes somewhat enlarged, and friable. It is often moderately softened, and generally engorged with dark currant-jelly-like blood. In some cases it is found unchanged. No prominent change has been detected in the pancreas.

In some cases the penis is found covered with eschars; and the scrotum swollen and thickened—brown or black, as in senile gangrene—sometimes with excoriations.

The yellow fever is a disease of hot climates and hot seasons. In every locality where the disease prevails as an endemic, or in an epidemic form, the thermometer gives us an average heat of 80° , or thereabouts. In all these places, the disease shows itself only at the period of the year when the heat is greatest—and usually with the most severity during seasons of the highest temperature, seldom attaining its greatest degree of intensity before the heat has continued at its maximum average for some time. Heat alone, however, is insufficient to produce the disease, for in climates and seasons of a higher and longer continued average temperature than that given above, it has not made its appearance.

An excess, or at least a certain degree, of atmospherical humidity would also appear to be necessary to the development of yellow fever. The disease frequently occurs during or immediately succeeding rainy seasons, and is, ordinarily, encountered in damp localities, where rain is common and falls abundantly; where the soil, previously dry, has been rendered wet by copious rain, freshets, overflows, &c., or where the dew point is high, and vesicular humidity generally or often noticed, or is considerable at the time. But facts innumerable go to show that humidity combined with heat, is not sufficient alone, for the production of fever.

Some local cause capable, when acted upon by heat, moisture, and perhaps other conditions of the atmosphere, the exact character and influence of which have not as yet been fully observed, of generating a special poison—by which an infection of the surrounding atmosphere is produced; which infected atmosphere, when taken into the system—particularly in one especially predisposed to its morbid influence—the poison it contains enters the blood and is by it distributed throughout the body—impairing the vital properties of the blood itself, and producing a morbid impression upon the nervous centres—and in this manner deranging the functions of, and producing the morbid conditions which we detect in the several organs and tissues of the body. The variation in the character and extent of these lesions in different subjects depending on the difference of age, habits of body, and numerous other circumstances connected with the individual; on the degree and concentration and violence of the efficient cause; on the peculiar character of the epidemic constitution of the atmosphere; on the nature of the localities and numerous other modifying agencies.

It would be impossible to enter here into the evidence of the invariable origin of yellow fever from a specific poison existing in the air of the localities where it prevails, and produced from causes there existing. That evidence is in our opinion conclusive, and irrefutable.

In referring the production of the yellow fever, in every instance to a local malarial cause, we mean, also, to deny its propagation by contagion;—we are acquainted with no other disease of a strictly local origin ever evincing a contagious character or assuming such a character under any possible contingency. The facts, in evidence of the non-contagious character of yellow fever, are well authenticated and numerous—and sufficiently establish the correctness of the position. As a general rule, we use the words of Dr. La Roche, the disease may be regarded as one of low, flat, and level localities, and as appertaining more especially to hot latitudes. It never shows itself beyond a certain elevation, the limits of its altitudinal zone being even more restricted than those of ordinary paludial fevers.

The inability of the yellow fever to be generated at a high elevation above the level of the sea, depends in part on the greater elasticity and purity of the air, on a diminution of atmospheric pressure, and on a more thorough ventilation. But the

main cause is the absence there of the degree of atmospheric heat, which, as we have seen, is indispensably necessary for the elaboration of the morbid agent to which the disease is due. For the same reason, in part, though not exclusively, its geographical limits are restricted within certain bounds in a northern direction, while, in a southern, the same effects are produced, as it would seem, by an excess of heat, and a variety of influences of a meteorological and telluric nature. Be the causes, however, what they may, on one point there can be no doubt—that the yellow fever has geographical limits, beyond which it does not appear—and that within those very limits there are many places where its usual apparent cause would seem to exist, but where, nevertheless, it has never shown itself, or has done so very seldom. The West Indian Islands, and part of the coast of South and North America, constitute its proper soil. From Brazil to Charleston, in one direction, and from Barbadoes to Tampico in another, the causes of the fever are in constant though unequal force, in regard to different seasons and localities. It prevails often, though not very generally, in some places more north than Charleston; visits, occasionally, the Atlantic cities of our Middle States, and has ascended as far as Boston, while in the Mississippi Valley it has prevailed as high as Memphis, perhaps Gallipolis, or even higher. In an eastern direction, but within the same latitudes, it has extended to Cadiz, Xeres, Carthagenia, Malaga, Alicant, Seville, Barcelona, and other cities of the coast and the interior of Spain. It has prevailed several times at Gibraltar, once at Rocheford, once at Lisbon, and once at Leghorn. Hence, we find it embracing a considerable portion of the earth's surface. In its fullest latitudinal extension, it reaches to between the twenty-second and twenty-third degrees south of the equator, and, on the other side, to the forty-second degree on the Atlantic coast, to the thirty-fifth degree on our western waters, and to the 8.56° on the Pacific. Considered only in reference to its legitimate longitudinal boundaries, it stretches from about the sixtieth to the ninety-seventh degree of longitude east of Greenwich. Its true area includes the Caribbean and other islands called the West Indies, and Bahamas, the contiguous coast of Colombia and Guatimala and the extensive shores of the Mexican Gulf, sweeping from Cape Catoche on the west, to Cape Sable on the east, and running thence along the coast of America to Wilmington, (N. C.) Norfolk, Baltimore, Philadelphia, New York, Boston, and intermediate towns: in some of which places it is an occasional, not annual, or even frequent visitor.

“Until recently, the river Amazon, which divides Brazil from Guiana, formed the boundary of the disease south of the equatorial line; for, although it is said to have prevailed at Olinda from 1687 to 1694, and to have shown itself as far as Montevideo in the beginning of the present century, the latter circumstance is open to some doubt, while in Brazil, from the close of the seventeenth century to the middle of the present, the disease was not observed. Since 1850, it has invaded Rio Janeiro, Bahia, Pernambuco, and other places of that country. It is, in a great measure, a stranger to the Pacific, having prevailed but once at Panama, twice at Guayaquil, and once at Callao. It does not appear in the East Indies. It has never prevailed in China, Cochin China, Singapore, Siam, Ceylon; it has prevailed occasionally on the African coast, Senegal, and the Gold coast, and has but three times, in the space of eight-six years, showed itself in Cayenne.

“Within those limits, it has, in some one or more places, originated and prevailed to a greater or less extent—occasionally or frequently—either as an endemic, or as a mild or wide-spreading epidemic. Beyond these it never shows itself; and though—whether north or south, east or west—it does not reach the point at which common malarial fevers stop, it approximates to these diseases; so far, especially, as its northern or western extension is concerned, it being circumscribed within certain bounds; for they, too, have their limits. The effect in both instances is due to modifications in the same morbid agencies.”—*La Roche, opera citat.*

Acclimatization is preventive of an attack of yellow fever. Thus, in places where the yellow fever is endemic—where the climate is continuously warm, and the causes of the disease are more or less permanent, or frequently evolved—individuals accustomed by long and continued residence to the influence of the climate, and the agency of those causes, lose their susceptibility to an attack. But after a prolonged residence in cold and more salubrious localities, the individuals thus protected lose, to a certain extent, their acclimatization, and on their return to their former places of resi-

dence become once more liable to suffer from the disease. The children, too, of natives of, or of those acclimatized to tropical regions, do not enjoy the same advantages in regard to protection, as their parents, but acquire them rapidly as they advance in age. The residents of some portions of tropical regions suffer to a certain extent from the disease, on removing to another portion less salubrious. The protection of acclimatization is also, to some extent lost, by a long exemption of the locality from local sources of infection, or by the prevalence, during several successive summers, of a cooler and purer atmosphere than before.

The most susceptible subjects of yellow fever are those who have recently arrived in infected localities, particularly the inhabitants of northern climates—the predisposition to an attack increasing with the degree of the northern latitude from which the stranger has arrived, and the shortness of the interval that has passed since he left the northern for the equatorial region. Even the inhabitants of situations in the neighbourhood of infected localities, but more elevated and salubrious, or of rural districts generally, though less prone to the disease than strangers from cold climates, are, nevertheless, liable to suffer when they venture into an infected place.

As a general rule, an attack of the yellow fever exhausts the susceptibility of the system to further attacks, or renders it less liable to be severely affected by the poison of the disease. Second attacks of yellow fever, in individuals who have passed through the disease, are somewhat rare; the immunity thus obtained is greater than that derived from simple acclimatization. Second attacks constitute, therefore, exceptions to a rule, and are perhaps but little more frequently met with than second attacks of other diseases through which the system usually passes but once.

Individuals of the sanguine temperament—the robust, strong, and plethoric, are those most prone to the disease—especially when their mode of living is calculated to keep up that temperament, or to bring out its elements in bolder relief. In times of unusually violent epidemics, when the fever spares none, individuals of all temperaments become alike its victims. As a general rule, females are less obnoxious to the impression of the poison productive of yellow fever than individuals of the other sex, and when attacked have the disease in a milder form. The disease, usually, effects in preference individuals of adult age—sparing, to a greater or less extent, young children as well as persons advanced in life.

“In all places, whether within the tropics or in temperate climates, in which the yellow fever has manifested itself—sporadically or epidemically—the negro race has manifested a greater or less susceptibility to the influence of those causes that give rise to the disease; every where, however, that susceptibility is far inferior to that exhibited by the white race—the disease in the former spreading less extensively, and assuming usually a milder and more tractable character. In warm regions, the almost general exemption of the blacks is due, in some measure, to their being acclimatized to the country—a circumstance they share with creoles, and those who are inured to the climate; their more frequent liability to the disease in colder than in warmer regions, is due to the same cause which renders the white inhabitants, whether natives or long residents, more prone to the disease than creoles—the want of acclimatization. By losing, through means of expatriation, the power of resistance imparted by acclimatization, they are placed much on the same footing as negroes of temperate climates, being no longer as surely exempt as they were before from the disease when again they are exposed to its influence.”—*La Roche*.

Fear, in common with all the depressing passions, is a powerful predisposing cause of yellow fever.

“Excessive joy, fits of anger, by stimulating the action of the heart and arteries, as well as the nervous system generally; strong emotions of any kind; despondency from pecuniary or other losses; disappointment from even trivial causes; intense mental application, are no less to be deprecated, though producing their effects in a different way; while the feeling of hope, courage, cheerfulness, as well as equanimity of temper, have the contrary tendency, of shielding the system from the morbid influence of the efficient cause of the disease, both by promoting the healthful play of the functions, and placing the happy possessor of them beyond the reach of the depressing passions. They prove powerful adjuvants of treatment.”—*La Roche*.

Whilst sleep, especially in infected and exposed localities, must be viewed as a pre-

disposing or exciting agent of the disease, the deprivation of it — watchfulness from any cause—gives rise to the same effect.

Intemperance in respect both to food and drink; the use, especially when carried to excess, of aliments of an exciting and nutritious, as well as those of a crude and indigestible character; unripe or acid fruit, perhaps still more particularly the use of ardent spirits, and, indeed, of stimulating liquors of any kind, have almost invariably exhibited a tendency to excite the development of the yellow fever. The danger arising from such indulgences has been fully recognized from the earliest period, and is recorded by almost every writer, ancient and modern, who has treated of the cause of the disease. It is proper, however, to remark, that a sudden change from a generous to an abstemious diet, will be apt to bring on an attack of the disease in individuals who might otherwise, in all probability, have escaped. A too rigid and abstemious diet is equally injurious.

Immoderate evacuations; venereal excesses; fatigue of body from whatever cause induced, as well as all other things calculated to debilitate the system, may be ranked among the predisposing and exciting causes of yellow fever.

Butchers, curriers, tanners, soap-boilers, tallow-chandlers, scavengers, and in general, all those who habitually breathe an unwholesome atmosphere are far less liable to the disease than others differently circumstanced. While, on the other hand, cooks, bakers, black and whitesmiths, hatters, tailors, and sugar-refiners, are reputed to be particularly exposed to attacks of the disease.

Among the more frequent exciting causes of yellow fever may be ranked exposure to cold in any way, especially when the body is heated or perspiring, through the effect of exercise or otherwise, as also exposure to the coolness and chilliness of night air, to a shower of rain, &c., sleeping in the open air, or exposure in any manner to the night air—lying upon the ground, drinking large draughts of cold liquors, especially water.

The suppression of any of the natural, or of any artificial excretions, as also, the sudden diminution or removal of irritations to which the system has become in some measure accustomed, issues, blisters, alvine evacuations, ulcers, chronic cutaneous eruptions, etc., have been found to produce an injurious result, and prove the harbingers of an attack of the fever.

The diagnosis of yellow fever, under ordinary circumstances, and to one familiar with its phenomena and course, as contrasted with those of other fevers endemic to the same localities and prevalent at a similar season of the year, is attended with little difficulty. There is not, however, a single symptom appertaining to the disease, when viewed by itself, or without regard to the degree of its frequency, that can, strictly speaking, be considered as really pathognomonic, each of its more prominent symptoms, is, at times, absent in cases of undoubted character, and each will be found to occur in other diseases, but remotely connected with it. But, as Dr. La Roche very properly remarks, when viewed in connection with each other—when found associated together, or when the greater number of them are present in the same case—especially when this assemblage is found to hold in a large number of individuals affected, the result is different. Under those circumstances, the peculiar jaundice described, varying from the bright yellow to a dark mahogany or livid hue—the emission from the stomach of the dark coffee-ground matter, so well known under the name of black vomit—the injected, brilliant, transparent, fiery, and glassy eye—the thin, slimy, white or moist, thick and dirty yellow fur, and clean, red edge and tip of the tongue—the super-orbital pain—the rachialgia—the single febrile paroxysm, and its sudden cessation at the end of some forty to seventy-two hours, more or less—the absence, from that period, of all fever—the progressive increase in the slowness and depression of the pulse—the gradual loss of cutaneous heat—the albuminosity of the urine—all these may be viewed, in their *ensemble*, as typical of the disease, and as its characteristic and pathognomonic phenomena. When they all occur together, or when only one or two fail to do so, the physician may be assured he has to deal with the yellow fever; and the certainty is enhanced when the case in which they are observed presents itself at a period of the year, and under circumstances favourable to the development of the disease. On the other hand, when they are all, or for the most part, absent—the black vomit and jaundice particularly—we may, in the majority of cases,

safely conclude that the disease is of a different kind, even when circumstances are favourable to the development of the yellow fever cause.

The prognosis in yellow fever is upon the whole unfavourable — under ordinary circumstances, and with the exception only of some epidemics of unusual mildness, the disease is of the most dangerous character, and the chances of recovery are slender. But little aid is afforded in enabling us to arrive at a correct prognosis by the presence or absence of signs which in other diseases are of the highest value. Cases occur in which, when everything would seem to indicate a favourable result, the patient has been carried off with great rapidity, while on the other hand, cases of recovery happen in which, from the nature of the symptoms, a fatal termination might naturally have been anticipated.

According to Dr. Blair, (*op. citat.*) a slow pulse and moderate temperature of the body, and quiet stomach, are always favourable indications. But the more fiery crimson the tip and edge of the tongue, the more irritable the stomach, the severer the headache, the worse the prognosis of the first stage, and *vice versâ*. Slight or moderate epistaxis is a sign of little prognostic value in any stage; but streaks of blood in the early vomit indicates much danger, while, during the stage of black vomit, or after an elimination has set in, it is a favourable appearance, provided the blood corpuscles are found entire. In the second stage, the earlier or more complete the suppression of urine, and the more copious the ejection of black vomit, the more imminent the danger. But if the urinary secretion continue, and the black vomit be scanty from the first, or is afterwards suppressed, the patient may yet survive. Urine simply albuminous is a less serious sign than when it also contains tube casts; but if the latter are thin and few in number, they do not add much to the gravity of the indication. Free, copious urine, no matter how dark or bilious, is the most favourable of any single sign. If the urine be scanty, and, at the same time, loaded with tube casts, entangled in epithelial and fibrinous matter, forming a light, buff-colored sediment, it indicates a complex lesion of the secreting structure of the kidney, and is as fatal a symptom as entire suppression. Blood corpuscles in the urine are not a bad indication. A faltering of the articulation is a bad prognostic, still more so when attended with a difficulty of protruding the tongue. The danger of the attack is enhanced by its inflammatory complications, and by hypertrophy of the heart. The recency of residence in a cold or temperate climate — the race or complexion of the individual — his age, habits of life, and preceding condition of health — and the fact of his having already suffered from an attack of the disease, are all to be taken into consideration in judging of the chances of a favourable recovery.

Dr. La Roche has presented in detail, and at great length, the various phenomena, the absence or occurrence of which in a case of yellow fever may be considered as favourable or unfavourable indications, and to the work of that gentleman we must refer for an account of all that the most accurate and repeated observations have taught us in reference to the prognosis of the disease. In the meantime we borrow from him the following general remarks:

1. As the yellow fever is more or less modified as to the degree of its prevalence, the severity of its attack, and the mortality it occasions, by certain peculiarities connected with the condition and habits of the patient, his age, sex, race, constitution, idiosyncrasy, &c.; it follows as a necessary consequence, that when it occurs under circumstances the least favourable in the above respects, the prognosis will be equally unfavourable.

Hence, we need not fear so much the issue in children, females, negroes, or those who have already passed through the ordeal of the disease, in natives of the warmer latitudes, or in those whose habits are temperate; while youth, a plethoric state, a sanguine constitution, high living, and intemperance, predispose to, while a debauch, excessive fatigue, or terror, a fit of anger, the intemperate use of venery, &c., often excite a severe attack, from which the chances of recovery are less to be anticipated.

Generally speaking, the more recently a stranger has arrived the more severe the attack. The same may be said of the remote cause itself, which, in some seasons, is of such a degree of malignancy as to produce a disease, which, though apparently differing little from that of other periods, has a greater tendency to end fatally, and must, therefore, call for a very different prognosis. Again, the latter must be more guarded during the continuance of the same epidemic in different parts of an infected

city—the symptoms appearing the same—inasmuch as the malignancy and fatal tendency of the fever differ in them. The same remark is applicable to the several periods of the same epidemic. In general the prognosis should be more guarded at the outset, as the disease is more apt then to terminate fatally.

2. As a general rule, it may be said, that in yellow fever it is not so much the presence of good signs which we are to look for in order to form a favourable prognosis, as the absence of bad signs. What would be regarded as good signs in most other diseases, are of little or no avail in this, and many patients recover after exhibiting some one or more of those which experience, in many fatal cases, teaches us to look upon with suspicion. Nevertheless, the appearance of any one of the decidedly bad signs, and still more a combination of them, must be viewed with fear, leading, as they very generally do, to a fatal termination.

3. It is not less to be noted, that much more is to be expected from a gradual amendment of the febrile and other symptoms than from a sudden disappearance of even the most unfavourable of these. From the latter change, indeed, the most disastrous results may in general be expected—followed, as it generally is, by delirium, coma, and other bad symptoms. The danger of the disease, great as it is, when uncomplicated with any other complaint, becomes much more so, and calls for a more unfavourable prognosis, when to the phenomena which reveal its existence, are superadded those indicating the coexistence of other disorders.

4. With the exception of the mildest or ephemeral forms of the disease, the danger to be apprehended is, generally speaking, proportionate to the shortness and rapidity of the case. When the disease extends to the seventh, ninth, or eleventh day, recovery may reasonably be expected. The same favourable view may be taken from the prolonged duration of the stage of reaction—the danger in the subsequent stage being proportioned to the shortness of the first, and the early supervention of the state of metaptoxis.

5. In cases marked by decided remissions, the disease, as we have seen, is of a milder character, and admits, therefore, of a much more favourable prognosis, however severe the febrile reaction may be during the exacerbation.

6. Rigors, at the commencement of the attack, denote considerable danger, and are usually viewed as signs of fearful omen, the danger being proportionate to their duration. The same may be said of chills, when violent, long continued, and repeated.

7. Considered in a general way, the yellow discolouration of the skin, which has given a name to the disease, is doubtless a sign of importance in a prognostic point of view; for, though not invariably observed in all fatal cases, it is much more frequently seen in these than in cases of recovery. Hence, generally speaking, the appearance of this symptom must be regarded with suspicion.

The appearance of jaundice at an early period of the disease may be viewed as a symptom of serious import, and as indicating an attack of a dangerous, and even fatal character—the danger increasing in proportion to the deepness of the discolouration. When, on the contrary, it appears at a late period—after the sixth or seventh day—it is of dangerous import, and, may even be said to assume the character of a critical sign. As regards the peculiar hue it presents: in many cases, and during certain epidemics, the light yellow or lemon colour has proved more dangerous than the dark yellow; while in other seasons and localities, the reverse is said to have been the case. Jaundice is indicative of more danger when it assumes a greenish, violet, mahogany, or bronze hue, and particularly when the skin presents a mottled or party-coloured appearance, characterized by livid, olive, and ash-coloured patches of all sizes, and blending into each other. Greater apprehension is to be felt when the discolouration in question is rapidly and very extensively diffused over the body, than when it is limited in extent, and spreads slowly. (See *La Roche, opera citat.*)

In proceeding to a consideration of the treatment of yellow fever, we may remark, that this must be based upon the character assumed by the disease, in its different visitations, and even in each separate case that presents itself. The same therapeutical measures are not adapted alike to the inflammatory and to the congestive forms of the fever. Nor will the same course of treatment tend to conduct to a favourable termination of those cases, in which, from the very onset, the disease assumes an unquestionably malignant or deadly aspect, and those the symptoms of which are so mild, that recovery will be spontaneously, or with but slight aid from medicine—while it will be

as little adapted to the cases intermediate between these two extremes — where the object of the physician should be to apply in time those means best calculated to produce a favourable impression on the train of morbid actions, and thereby arrest their dangerous tendencies.

The treatment of yellow fever must be modified according to a variety of circumstances. As remarked by Dr. La Roche, *op. cit.*, “in encountering this formidable disease, we must content ourselves with endeavouring, not to neutralize the poison circulating in the system, but to correct the morbid effects it occasions on both solids and fluids. We must, while watching carefully the course of the disease, prevent undue mischief from being done, especially to organs essential to life. We must keep these organs in as healthy a condition as possible — restore, if possible, equilibrium in the play of the functions — reduce undue and dangerous excitement, general and local, and sustain the powers of life when these threaten to become impaired, or are already reduced beyond the point of safety. But we are forced to confess that, beyond this, art is of little avail. The idea of *curing* the disease, or greatly abridging its course, is entitled to little confidence. To nature must be left the chief management of the case; time must be allowed for the elimination of the poison; and the physician must be impressed with the conviction that, in cases where no marked organic mischief has been done, or is likely to occur, he must keep his hands off as much as possible, and restrict his agency to the employment only of such means as are strictly necessary to fulfil particular indications. He must not attempt to do what is more safely done by the recuperative powers of the system, and rest assured that in these and indeed in all instances, more danger is to be apprehended from too great than too little interference on the part of the medical attendant.”

When called to a case of yellow fever, a primary object of solicitude on the part of the physician, should be, to guard as much as possible against all disturbing influences. He must see that the patient is confined effectually to his bed, and prevented from rising. He must give proper directions for the free ventilation of the apartment, and the preservation of cleanliness; while, whatever be the form the disease assumes, he should proceed to the medical treatment of the case with the least delay possible. The great rapidity with which the dangerous symptoms make their appearance, the little time afforded for preventing their onset, and the great importance of effecting that object — besides the difficulty of their removal when they do appear — render such promptness imperative. At the same time, a knowledge of the insidious and treacherous nature of the disease, the great difficulty of predicting whether symptoms indicative generally of a mild attack are not soon to be succeeded by those of an opposite kind, and whether changes calculated to make us hope for a favourable issue are not to be followed — perhaps in a few hours — by others portending approaching death, should make him constantly attentive to the nature and succession of every phenomenon.

Attention being paid to these details, the medical treatment will necessarily vary according to the particular form which the disease assumes. In the several varieties of the inflammatory form, recourse must be had to antiphlogistics, sedatives, and evacuates, graduating the energy of these to the degree of the violence of the reaction, the force of the circulation, the heat of the skin, and the extent of the local inflammations or congestions. Of the indispensable necessity of antiphlogistic and evacuant treatment, which in this, as in other fevers of kindred nature, consists in sanguine evacuations, sedatives — internal and external — and purgatives, there can be no doubt. It is based on the evident character of the complaint, and the success which has attended its application, and comes to us under the sanction of innumerable and high authorities.

The foregoing remarks apply exclusively to the first stage of the inflammatory form of the disease. To be productive of benefit the remedies indicated must be resorted to without loss of time — their success being proportionate to the earliness of their application. They are not, it is also to be recollected, as a general rule, to be used with the same freedom as in ordinary inflammations. Except when there is inordinately high reaction — or when important organs are seriously inflamed or congested, and the patient is vigorous, plethoric, and young, and the recuperative powers of the system energetic, it would be safer to avoid the use of agents calculated to debilitate suddenly and considerably, and to trust to milder means. While endeavouring to

relieve inflammation or congestion, we should never lose sight of the imperative necessity of husbanding the strength of the patient, and of avoiding everything calculated to depress the powers of life and foster the tendency to collapse.

While general excitement, local inflammations and congestions, if they exist, are reduced by suitable depletion and sedatives, and the bowels, when costive, opened by mercurial and other purgatives, attention must be paid to tranquillize the stomach — and the physician must watch the efforts of nature, and promote any critical movement she may indicate.

Various means have been proposed and recommended to calm the stomach. Leeches or cups to the epigastrium in cases where they are admissible, have, occasionally, been found beneficial. Lime water by itself, or combined with an equal quantity, or two-thirds of new milk, in doses of a wine-glassful, has been highly recommended; other means have been also used with more or less success.

Among these may be enumerated the carbonates of soda and potass, the saline mixture, the effervescent draught, yeast, calcined magnesia, porter, chalk mixture, liquor potassa in barley water, carbonate of ammonia and hydriodate of potassium, and spruce beer and essences. Of all remedies of the alkaline class, the chlorate of potass is entitled to most notice in this place, from the praise it has recently received from the high authority of Professor Frost, of Charleston.

The nitrate of silver — small doses of creasote diffused in water — chloroform, in doses to the extent of even half a drachm, have likewise been recommended — they would all appear to us to be of doubtful propriety in the first stage at least of inflammatory cases.

After the more decided inflammatory symptoms have been subdued, iurpentine, combined with a portion of mucilage, in doses of ten drops every two hours, will often be found very speedily to check vomiting — under the same circumstances also, a blister or sinapism over the epigastrium will be found of service. The acetate of lead, in doses of from grs. $2\frac{1}{2}$ or 3, every three or four hours, either in pills or simple solution, has been highly recommended, and will sometimes succeed in restraining the frequent retching and violent vomiting so characteristic of the disease. When we have succeeded in checking the violence of the fever, and in relieving whatever local irritation or congestion may be present, and an intermission has been obtained, “or,” to use the language of Dr. La Roche, “when the latter or metaptoxis has occurred at the regular time by the spontaneous subsidence of the febrile excitement, little remains to be done beyond keeping up the strength of the patient by mild tonics and light nourishment, and preventing, if possible, the onset of further and malignant symptoms, by the use of antiperiodic, tonic, and astringent remedies. But when the remission is incomplete, or is succeeded by a train of symptoms indicating a continuance and exasperation of the disease, another and different course must be pursued. The irritability of the stomach and the vomiting, as also the inflammation of that organ, if it occur, must be combated by sedatives and antacids internally, emollients and revulsives externally; the cerebral organs, if implicated, must be attended to, and their irritation or congestion treated by ordinary means; the failing powers of the system must be sustained by nourishment, tonics, and stimuli, either by the stomach, if it will bear them, or, in the contrary event, by the rectum; while the hæmorrhagic tendency must be arrested by astringents or other suitable remedies.

In the congestive form of the disease, when the reaction is deficient or altogether wanting, recourse must be had to means calculated to arouse and sustain the dormant energies of the system, and, at the same time, to relieve the sufferings of the overloaded organs. External stimulation, by means of rubefacients, hot baths, sinapisms, vesicatories, &c.; the internal use of stimuli, tonics, &c., by the stomach or bowels, and, in the milder cases, stimulating and mercurial cathartics, must be resorted to; and, whenever it can be done with safety, the congested vessels of important organs, and the inflammation of particular parts, which sometimes is combined with the congestion of others, must be relieved by general and topical bleeding.

Local depletion, by cups or leeches, may be resorted to in many cases of yellow fever, where general depletion is not considered advisable; or for the relief of particular organs. The cups or leeches should be applied as near as possible to the part affected — on the epigastrium, when the stomach is to be relieved; on the temples,

along the course of the jugular veins, inside the nostrils, or at the back of the neck, when the condition of the encephalon calls for assistance. As a general rule, local depletion should be resorted to at as early a period of the disease as possible. When practised late, it is of no utility, and may even act disadvantageously, by increasing the prostration of the powers of life.

Sudorifics are strongly recommended by many in the treatment of yellow fever in its earliest stage. The warm and vapour baths, or pediluvia of warm water, or warm water with mustard, aided by warm lemonade, or a warm infusion of some agreeable vegetable substance, have unquestionably been found, in many cases, when properly timed, to produce a general diaphoresis, from which very decided relief has been obtained. It is only at the very onset of the milder cases, however, that much benefit is to be expected from these sudorifics. In the more decidedly inflammatory forms, they cannot fail to prove prejudicial. From the irritable state of the stomach, there is little to be expected from the usual diaphoretics internally administered.

The warm bath is particularly appropriate when, in the early stage, the reaction is imperfect, partial, or deficient—when, in a word, the disease assumes one of the grades of the congestive form. In such cases, a general bath of high temperature—not less than 100°—will tend to promote a return of heat to the surface, and diffuse it if unequally distributed, and at the same time revive the activity of the circulation. In general, the water may be used alone; but in the case of extreme collapse, its beneficial effects will be greatly enhanced by the addition of salt, spirits, or mustard.

There are a number of means which may be resorted to, in order to meet particular indications—to relieve pain and local inflammation. Pediluvia, warm or tepid, fomentations and cataplasms are generally used, and found useful. Warm frictions, either dry or with soap, with hot oil, whiskey, or stimulating lotions, or, again, with lemon juice, will also find a useful application. The latter means—frictions with lemon juice—constitute a main instrument of what is called the creole treatment, or that of the colored women of the West Indies, and is favourably spoken of by the medical writers of experience in tropical climates and this country.

In the early period of the first stage of the fever, when the skin is hot and dry, cold water, applied over a considerable extent of the surface, either by sponging or affusion, has been found, at times, to exercise a beneficial influence, and by many physicians, is considered an important agent in the treatment of the disease. Such has been the case in tropical climates, and the results there obtained will be found fully corroborated by those recorded in temperate latitudes.

Dr. Blair remarks, that when the heat of surface was ardent, the *wet sheet* or *blanket* was used for the reduction of temperature by evaporation, with frequently very good effect.

The same rule must be observed in the application of cold water in cases of yellow fever, as in other febrile diseases—namely, it should be resorted to, only when the skin is decidedly hot and dry, and the reaction well established. Under such a condition of things the application of the cold water is usually followed by a reduction of heat and of vascular action, as well as, also, in numerous cases, by a tendency to perspiration, a sensation of comfort, and an abatement of many unpleasant symptoms. The same effects, but in a less degree, will result from partial applications of cold water or cold pediluvia. When applied, on the contrary, under other circumstances, very opposite results will very generally follow.

“If,” as Dr. La Roche well remarks, “the skin be cool and the pulse depressed, the sedative effect of cold water will have a tendency to aggravate the symptoms. Reaction, in such cases, seldom occurs; the disease becomes more concentrated within the internal and important organs, and the prostration of the vital powers increases rapidly. When the skin, instead of being dry, is warm, moist, and relaxed—which, as seen, occurs sometimes in all epidemics, and very generally in others—the application of cold to the surface has a tendency to check the salutary condition, and will almost always sorely aggravate instead of benefiting the disease. Nor are cold ablutions to be less avoided when the patient complains of chilliness or dyspnoea; or when he labours under diarrhoea, or deep congestion, or well-marked inflammation of internal organs; or, again, when he is of weak constitution or of advanced age.”

“In cases attended with a moderate degree of temperature of the surface; or when, after depletion, or without, the skin exhibits a tendency to perspiration; when, again,

the patient suffers from restlessness or other nervous symptoms, or when the shock from cold water is unpleasant, the tepid bath, or ablu-tion with water of slightly elevated temperature, or a sheet dipped in tepid water, is found highly advantageous."

"In cases in which it is inconvenient or improper, for particular reasons, to have recourse to the hot or tepid bath—general or partial—the effect will often be obtained by means of the artificial vapor-bath, obtained by pouring water, either alone or mixed with vinegar, on hot bricks and introduced under the bed clothes."

It is scarcely necessary to point out the importance of placing the patient in apartments, large, clean, dry, and well ventilated, and so situated as to secure for him the advantages of pure, cool, fresh air. Under all the circumstances of the disease, let whatever be its form, this is an all-important measure.

The drinks of the patient should be cool and refreshing—in the first stage of the inflammatory form of yellow fever, cold water is perhaps the best, taking care, however, that it be taken in moderate draughts, at short intervals, to avoid over distension or prostration of the stomach. Where the stomach is irritable, and bears with difficulty the ingestion of water or other cool liquids, small portions of ice, held in the mouth and slowly swallowed, will often prove highly refreshing.

A variety of drinks have been suggested, and in many cases may be resorted to with advantage, as weak lemonade or orangeade, tamarind, currant jelly, and raw-apple water; thin flaxseed, or gum water, plain or sweetened; iced carbonic acid water. "Under the use of cold and iced drinks, the irritation of the gastric mucous membrane," remarks Dr. La Roche, "often subsides; the sense of heat and ardour at the epigastrium; the nausea and vomiting; gastric hæmorrhage, as well as the general excitement of the heart and arterial system, diminish; while the temperature of the skin is apt to lessen, and a disposition to perspiration not unfrequently manifests itself.

When the mucous surfaces, as indicated by the tongue, were denuded of epithelium, we are informed by Dr. Blair, that the use of gum water was found decidedly beneficial, by its lubricating, defending, and soothing the raw surfaces. In general, three drachms of the purest powdered gum were dissolved in six ounces of cold water, of which a tablespoonful was given every hour or two. For thirty-six or forty-eight hours of the most critical period of the disease, the patient will take it without dissatisfaction; after that, it can be substituted by, or alternated with, arrow-root pap.

In all cases—even in those characterized by considerable thirst—care must be taken not to overload the stomach, as nausea and vomiting will almost inevitably be the result. The preferable plan is to direct the patient to drink often, and to take but a very small quantity at a time. Even in cases unattended with much thirst or irritability of the stomach, it is proper to enjoin the same rule of frequent and moderate drinking, for the double purpose of soothing the irritation of that organ, and guarding against awakening a disposition to nausea and vomiting, which, as we have seen, are almost constant attendants on the disease, and require but a trifling cause to bring them on.

Dr. Blair found a dislike of sweets in yellow fever patients, and when lemonade was asked for, the usual quantity of sugar was objected to. Tea was found uniformly to disagree with the patients, and to cause vomiting, particularly in the advanced stages.

In cases characterized by symptoms different from those enumerated, the drinks should be of a higher temperature; and where there exists a tendency to perspiration, they should, the condition of the stomach allowing, be taken warm and slightly aromatized.

In an advanced stage of the disease—when the powers of life are fading, the drinks should be of a stimulating and nourishing kind. Porter and water, weak punch, weak wine-whey, claret, champagne, or hock wine, brandy and water, &c.

When the gastric irritability is so great as to forbid the admission of any thing into it, much benefit is often derived from the use of small and strongly purgative enemata. They act on the principle of revulsion—excite the peristaltic action of the tube downwards, and thus tend to quiet the stomach. In the advanced stage of the disease, especially when the stomach rejects every thing, stimulating and tonic substances introduced by means of injections, sometimes prove serviceable.

Blistering is not admissible in the early stage of the inflammatory form of yellow fever. At the commencement of the second stage—when symptoms of an unfavourable character are present, blisters to the ankles and other parts will often produce a favourable revulsive action, or prevent the depression of the vital powers. At a later period of the disease, when symptoms of prostration present themselves, blisters, in conjunction with other stimulants and excitants, will not unfrequently contribute in arousing the sinking energies of the system.

In the early period of the congestive form of yellow fever, blisters are adapted to aid in the restoration of action to the surface of the body, and in the excitation of the general circulation and nervous system. They will, also, often prove serviceable for the relief of gastric distress, pain, nausea, and vomiting, in cases of the disease where the febrile excitement does not run high, or in its second stage, when applied to the epigastrium or spine; they will, likewise, not unfrequently be decidedly beneficial for the relief of the headache, delirium, and stupor, attendant on the various stages of the disease, when applied to the nape of the neck, to the occiput, or to the upper extremities. So, in like manner, pain in the region of the thorax, whether resulting from disease of the lungs or external muscles, as well as the tormenting rachialgia, will sometimes be relieved by blisters to the affected parts.

In order to obtain the effect desired from the application of a blister; it is not always necessary to produce vesication. By removing them so soon as they have produced a rubefacient effect, a revulsive action is obtained without risk of gangrene, soreness, or hæmorrhage, while the effect may be renewed when it subsides too soon.

Sinapisms may, in many cases, be usefully substituted for blisters, and should always be preferred when the urgency of the symptoms calls for a prompt and energetic treatment. They are particularly useful to tranquillize the stomach, and remove local pain; and, in congestive, as well as in the advanced stages of ordinary cases, to invite excitement to the surface, and arouse the failing power of the nervous and arterial systems. By some they are used early to counteract congestion.

While all internal stimulants are strongly contra-indicated in the first stage of the inflammatory form of yellow fever, they are sometimes required in cases in which from the onset the reaction is feeble and deficient—in some of the modifications of the congestive variety—to arouse the energies of the vascular and nervous systems—even when bleeding and other means of depletion are employed to empty the congested organs. In not a few cases, however, of even the congestive form of yellow fever, the early use of stimulants is unnecessary and improper. It is chiefly in the advanced or sinking stage of the disease that they are admissible; and they are here often required in very large doses. They are sometimes instrumental in sustaining the powers of life or mitigating particular symptoms, and thereby enabling the patient to outlive the disease.

Nearly all the articles included in the class of stimulants have been recommended, capsicum, serpentaria, wine, brandy, ammonia, camphor, chloride of sodium, sulphuric ether, spirits of turpentine, etc. No one of these can be considered as specifically adapted to the disease; all have no doubt been found useful, and it is equally certain that all have equally failed.

According to Dr. Blair the employment of ether was frequently found to be attended with marked advantage in removing or abating the distressing symptom, hiccup. It was also used as a diffusible stimulant, and when acceptable to the patient, he pronounces it fully equal for that purpose to brandy.

Opium, which is admissible, at an early period of the disease, only in the decidedly congestive form, when stimulating means are required to rouse the prostrated powers of life, has been recommended, in the latter stage of ordinary cases to support the system—and, in combination with the other stimulants may, sometimes, be found useful.

At this particular stage the cinchona bark, quinia, and the tincture of the hydrochlorate of iron, have been recommended as tonics.

The Peruvian bark, which has been suggested by some as an appropriate remedy even at the very onset of the fever, and by others at the close of the first stage or during the remission, is now, we believe, very generally considered as useless, or even mischievous, except during the last or sinking stage, when, probably, it may sometimes do good as well by its tonic as its astringent properties. Of the sulphate

of quinia, which has supplanted the bark as a therapeutic agent, the same remarks may be made.

Of late years the quinia has been recommended at the very onset of the attack, in large doses, with the view of at once cutting short the disease. In reference to the success of this abortive treatment of yellow fever, as it has been termed, we have been favoured with much discordant testimony, but none which, in our estimation, are of sufficient weight and clearness to recommend it strongly to our notice.

It would, nevertheless, be unfair not to refer to the strong testimony recently presented in its favour by Dr. Blair. In the course of the epidemic which occurred in British Guiana in 1852 and '53, the primary object of treatment was, he states, to abort, if possible, the attack by the administration of a mixture of calomel and quinia. Twenty grains of the first, and twenty-four of the second, for an adult, given in some simple syrup. The same being repeated at intervals of four or six hours, to the extent of four doses, unless the attack was earlier arrested. If, he remarks, after three or four doses are given the disease is not cut short, little room is left for active interference on the part of the physician, although still much may be done in putting the patient in the best condition for sustaining the struggle, and keeping off intruding complications.

The aborting dose of calomel and quinia, we are told, should be used as early as possible in the attack. When a state of apyrexia is induced, it may be relinquished—the end is attained; but, if the urine has become coagulable, or the epithelium of the tongue has begun to peel, it is of no use pushing it further, the time for its use has passed, and subsequent to this it will be a noxious irritant.

Sometimes, Dr. Bailey remarks, the disease is incompletely aborted, that is, although it does not proceed to the second stage, a certain amount of febrile action still continues after the abortant has been pushed to a reasonable extent. It was the practice then to give half an ounce of camphor water and spirit of mindererus every three or four hours, till the skin became cool and soft. Should, however, the stage of acid elimination supervene, this medicine is stopped, and small doses of bicarbonate of soda and nitre (five to ten grains of each) substituted.

In preparing the irritable stomach for the reception of the abortive dose of calomel and quinia, creasote, is stated by Dr. Blair, to have had often an admirable effect.

The muriated tincture of iron comes to us with such strong testimony in its favour, as a remedy adapted to even the earlier stages of yellow fever, that it demands a further trial.

We have said nothing as yet of the mercurial treatment of yellow fever, so highly lauded for its superior efficacy by several physicians of high repute. It does not appear to us that any evidence has been adduced to prove that this plan of treatment has been more successful than others; while from the known effects of the mercury upon the blood, we should apprehend an increase of the very morbid condition of that fluid, which, from an early period of the attack, constitutes so prominent a feature of yellow fever. From these considerations, and the uncertainty of our being able to place the system under its specific effects—the probable failure, even when obtained, of their producing the desired result, and the time lost in waiting for those effects to develop themselves, we should be inclined to reject mercury from the list of the therapeutic agents adapted to the disease.

In the management of a case of yellow fever, more perhaps than in one of any other disease, is it important to remove from the patient all impressions of a moral kind—to keep up his spirits and sustain his courage. His alarm and anxiety for the result of his case must, by every means, if possible, be quelled, and the stimulus of hope constantly kept alive; unless this be done—remedies are of little avail.

In the early stage of the inflammatory form of the disease, total abstinence from food should be enjoined, or only thin gum, rice, barley, or apple water allowed; or in the milder cases, arrow-root, sago, Indian, or oat-meal gruel, or the like, may be allowed in small quantities, and at reasonable intervals.

When the activity of the pulse has been reduced, and the skin softens and loses its excess of temperature; when the gastric irritation lessens also, and especially when the stage of metaptoxis sets in with fair prospects of increasing amendment, the diet should be made more nourishing.

Animal food, whether in the form of soup, carefully freed from fat, or in substance,

may be given, but with great circumspection; it may even be better to restrict the patient to farinaceous and vegetable articles. Thin rice and bread cream, arrow-root, sago, Indian mush, oat-meal gruel, slightly sweetened and aromatized, thin panada, weak coffee, tea, or chocolate, milk and barley-water, answer well, when given in small quantities at a time, and at short intervals. They are better suited than animal food to the existing condition of the digestive powers, which are often considerably impaired.

Wine, and other spirituous liquors, except when the debility is unduly prominent, and all signs of inflammatory irritation have subsided, must be avoided as unnecessary and often hurtful. Considerable attention is also required as to the quantity in which food is allowed — much mischief is often done by the patient indulging largely in even the most bland and proper articles. As a general rule, it is safer to give but a small portion of nourishment at a time, and gradually to increase the quantity.

As febrile irritation and gastric irritability further subside, and the favourable crisis approaches, the food must be given at shorter intervals, and rendered more nourishing. It may now consist of the same articles prepared in a more generous manner, or of milk, stewed fruit, chicken or veal broth. When the debility is considerable, beef-tea, animal jelly, &c., may be allowed. This choice of aliments, graduated in the way mentioned, to the condition of the digestive powers and of the system at large, must be persevered in to the period of convalescence. It is equally well suited when the disease, instead of progressively subsiding without the occurrence of bad symptoms, passes to the third stage, or that of depression. When, however, the disease assumes a malignant character, and symptoms of positive prostration call for tonics and stimulants, nourishing food, in the form of strong broths, essence of beef, animal jellies, administered often and in small bulk, may be tried as long as the stomach remains quiet, and it has sometimes proved advantageous.

In congestive cases little or nothing can be done in the way of diet so long as reaction has not taken place. When reaction has been brought about, the diet must be regulated in the manner already adverted to.

During convalescence, while the use of the remedies employed in the preceding stage, is to be gradually diminished, the patient must be allowed more and better food, but the transition to the diet of health must be gradual, and care must be taken to select none but light and nutritious articles, and to avoid indulging in these frequently, and especially overloading the stomach. For some days, unless the debility be great, and all signs of gastric irritation completely subdued, the patient had better abstain from animal food, and limit himself to the use of eggs, bread, rice, bread and milk, panada, and oysters. As strength returns, and the functions of the stomach acquire energy, he may be allowed chicken, veal, mutton, or beef broths, prepared with a large proportion of rice, barley, and other vegetables. Poultry, game, and fish may follow; and finally, the more substantial meats may be allowed.

But necessary and indispensable as this gradual return to the diet of health undoubtedly is, the physician does not always find it possible to enforce compliance; for there is often such a sudden revival of the appetite for animal food at the commencement of convalescence that it is difficult to restrain the patient within proper bounds.

Cases not unfrequently occur, in which, from feebleness and languor of the stomach, or a state of general debility, mild, and even strong tonics and stimulants are called for. Under these circumstances, the infusion of bitter plants, of Peruvian bark, of serpentaria, or the sulphate of quinia, will prove serviceable; while malt liquors, wines — claret, sherry, madeira, hock — and even brandy and water, find a successful application.

Hæmorrhages occasionally occur during convalescence, which, as they indicate great debility of the system, and an atonic condition of the vessels, and tend to increase, by the loss of blood they occasion, the very cause on which they depend, must be checked with the least possible delay. In these cases, acids internally, and nitrate of silver, creasote, or the tincture of iron externally, cool air, as well as remedies calculated to impart tone to the system at large, are employed with advantage.

Quiet and cheerfulness of mind must be secured, and sleep must be encouraged. They are essential to a rapid and complete recovery. So long as debility is prominent, bodily exertions must be avoided; but, as soon as the strength admits of it, exercise, alternating with rest, and graduated to the condition of the patient — short,

and frequently repeated—must be enjoined. The venereal act—to which, convalescents are prone—should be carefully avoided, as always highly detrimental, and often, when indulged in to any excess, of fatal tendency.

Great stress must, at the same time, be laid on cleanliness and free ventilation, but more particularly on change of locality, and removal from the infected to a pure and cool atmosphere.

When circumstances will permit, and convalescence is long and tedious, a sea voyage, and a residence in a cold climate, must be recommended. — C.]

I pass, without pausing, from the consideration of continued fever, to that of *small-pox* or *variola*; a disease, fortunately, less common in this country than it used to be, yet still sufficiently frequent and formidable to require that we should acquaint ourselves with the phenomena it is accustomed to present; and very prevalent here, as it happens, at present (1838). I have already mentioned, by anticipation, several points in its history.

This frightful disease sets in with smart febrile symptoms: rigors, followed by heat and dryness of skin, a hard and frequent pulse, pain in the epigastrium, with nausea and vomiting, and headache. Sometimes wild delirium, sometimes convulsions, attend its outset. Then, to use the words of Cullen's definition, "*tertio die incipit, et quinto finitur, eruptio papularum phlegmonodearum, quæ spatio octo dierum, in suppurationem et in crustas demum abeunt, sæpe cicatrices depressas, sive foveolas, in cute relinquentes.*"

When small-pox is fully formed, it cannot be mistaken for any other complaint: but it is of some importance to recognise it at its very commencement, for the force of the impending disorder may sometimes be lessened by judicious measures adopted at that early stage. The symptoms, however, that mark the outset of all febrile diseases are necessarily very much the same. If the pyrexia set in when small-pox is prevalent in the neighbourhood, if the person in whom it occurs be an "unprotected" person (*i. e.* one who has neither had that disease, nor been vaccinated previously), and especially if he be known to have been exposed, within from nine or ten days to a fortnight, to the contagion of variola, we may well suspect that the disease will turn out to be small-pox, and act upon that suspicion.

Nevertheless there are some symptoms which, being common in the commencement of variola, and not common at the beginning of continued fever, or of the other exanthemata, may assist the early diagnosis. Vomiting is one of these; pain of the back another. When these symptoms are violent, they usually usher in a severe form of the disease. The same may be inferred from a continuance of the nausea and vomiting, after the coming out of the eruption; which is very unusual. Heberden noticed that acute pain in the loins was almost always followed by a severe disorder; that pain higher up, between the shoulders, was of better augury; and that it was to be reckoned in all cases a good sign, if there were no pain of the back at all. Early delirium, stupor, or convulsions, announce severity in the subsequent course of the malady. Yet not always, especially in children. Within the last month I was asked to see a child which had been suddenly attacked with convulsions, followed by coma. In due time the eruption of variola appeared, and the disease ran a mild course, with little aid from medicine, although the child was previously unprotected.

The peculiar eruption almost always begins to show itself on the third day of the fever. The earlier it comes, the severer generally does the disorder prove. In judging of the date of the eruption, you must bear in mind that parents and servants are apt to state its accession to have been later than it was in reality: for the spots are at first so minute that they often escape observation. They also frequently begin to come out in the night; and the morning of the second day of the eruption is then called erroneously the first day.

The eruption comes out first on the face, then on the neck and wrists and on the trunk of the body, and lastly on the lower extremities. Such is the rule; so that (as is specified in the definition) it does not cease to come out till the fifth day: and it keeps a-head, in that order, throughout the disease. There are, indeed, some exceptions to this rule. Occasionally the spots appear first upon the extremities, but this is very rare. In some instances straggling papulæ continue to spring up after the

main crop is fairly completed; but these stragglers seldom arrive at the same size with the others.

The pimples, or papulæ, ripen gradually into *pustules*, the suppuration being complete by their eighth day; and on that day the pustules generally begin to break, and crusts or scabs to form. In four or five days more the scabs are falling off. There are some variations in all this also. In children, the crusts are sometimes visible on the seventh day: and in adults, when the disease is severe, they sometimes do not begin to form till the ninth day. In all cases some of the pustules are liable to be prematurely broken, by accident, or by the patient's scratching; and these will crust over earlier than they otherwise would have done. So that in fixing the period of incrustation, you are to regard those pustules only, of which the natural progress has not been interfered with.

All that I have hitherto been saying, applies with more or less exactness, to the disease in all its varieties. But its severity differs exceedingly, as I have already hinted, in different cases. Its severity, in truth, is almost always in direct relation to the *quantity of the eruption*. The number of pustules indicates, in the first place, the quantity of the variolous poison which has been reproduced in the blood. In the second place, it is also a direct measure of the extent to which the skin suffers inflammation. Sometimes there are not more than half a dozen pustules; sometimes there are many thousands. If all these were collected into one, it would be an enormous phlegmon. For both these reasons the system suffers commotion, distress, and peril, in proportion to the quantity of the eruption.

When the pustules are very many, they run together: when they are few, they are separate from each other. And this affords a broad line of distinction, which can neither be overlooked nor mistaken, into the *variola discreta*, and the *variola confuens*. In the one, the pustules are distinct, and of a regularly circumscribed circular form. In the other they coalesce, and their common outline becomes irregular. Now the discrete form of the disease is scarcely ever dangerous; the confluent form is never free from danger. The distinction therefore is of the highest importance and interest. For its full estimation, each form must be considered separately.

In the discrete variety, in which the disorder may be presumed to run its most natural course, the eruption is at first, according to the phraseology of Willan, *papular*. The pimples gradually increase in magnitude, but it is not till the third day of their appearance that they begin to contain a little fluid on their summits. For two days after this they increase in breadth only, and a depression is observable in the centre of many of them. The cuticle is bound down there somehow, for a time, to the cutis vera. It is the eighth day of the disease, or the fifth day of the eruption, before the pustules become perfectly turgid and hemispheroidal. During the time in which they are thus filling up, the face swells; often to so great a degree that the eyelids are closed; and the natural aspect suffers a complete and hideous change. The skin between the pustules on the face assumes a damask red colour. About the eighth day of the eruption, a dark spot makes its appearance on the top of each turgid pustule, and at that spot the cuticle breaks, a portion of the matter oozes out, and the pustule dries into a scab. When this crust at length falls off, it leaves behind it either a purplish red stain, which is still very characteristic of the disease, and which very slowly fades; or a depressed scar, which is indelible. In the latter case the patient, or more properly his skin, is said to be *pitted* with the small-pox, or pock-marked. The swelling of the face begins gradually to diminish after the eruption has become thoroughly pustular.

This is the course which the eruption pursues on the face, where the pustules, even in the discrete form of the disease, are usually thicker set than on any other part of the surface. And it pursues the same course, only two or three days later, upon the extremities, where it also begins later. The feet and hands swell just as the face swelled, but they begin to tumefy as the features begin to subside. Some of the pustules, especially on the extremities, do not burst at all, but shrivel up.

In this, the distinct variety of the disorder, the fever generally ceases entirely upon the coming out of the eruption: the headache, the pain of the back, the vomiting, the restlessness, abate and disappear, the pulse resumes its natural force and frequency, and the skin its natural temperature. About the seventh or eighth day of

the eruption there is commonly for a day or two a recurrence of fever. This is called the fever of maturation.

You are to observe that we judge of the eruption as it appears on the *face*. The disease is of the confluent kind, when the pustules are confluent there, whether they are so or not upon the trunk and extremities. Sometimes they are neither strictly confluent nor strictly separate, but stand just thick enough to touch each other, without absolutely coalescing; every pustule preserving its circular outline. In that case the disease is said to be of the *cohering* form. When the pustules are confluent over the whole body, their number is often prodigiously great, and their progress is less regular than in the discrete and milder variety of the complaint.

In the first place the eruptive fever itself is usually more violent and tumultuous in the confluent disease: the disturbance of the sensorial functions is more common and more decided, the sickness more distressing, the pain of the back and loins more severe. The eruption comes out earlier, and more confusedly; the pimples being at first very minute, and crowded together in patches, and not seldom accompanied by a rash like that of scarlet fever, or erysipelas: whereby the diagnosis, in so far as it depends upon the appearance of the skin, is rendered for a while uncertain. I have at present in the Middlesex Hospital a patient in whom the papulæ of small-pox were, at the outset, so intermingled with the appearances and sensations of urticaria, that I doubted, for twenty-four hours, what the true character of the eruption might be. It is sometimes like that of the measles; but the similarity and the uncertainty are soon at an end, for the pimples soon begin to exhibit a fluid on their summits. They do not, however, as they advance, and pass into pustules, fill up so completely as in the distinct form; they are flatter, less plump, more irregularly depressed, and even of a different colour; being at first whitish, and then of a brown tint, and seldom of the yellow purulent hue which is seen in the variola discreta. Sometimes they are even bluish, or purple. In the confluent form there is commonly some abatement of the febrile distress upon the coming out of the eruption, but the remission is much less decided than in the discrete. About the fifth or sixth day fresh rigors are apt to occur, marking the fever of maturation. Most of these points of distinction between the two varieties of the disease are well set forth in Cullen's definitions. The distinct form he defines thus: "*Variola (discreta) pustulis paucis, discretis circumscriptione circularibus, turgidis; febre, eruptione factâ, protinus cessante.*" And of the confluent kind his definition is, "*Variola (confluens) pustulis numerosis, confluentibus, circumscriptione irregularibus, flaccidis, parum elevatis; febre post eruptionem perstante.*"

But the most important difference between the two forms is in what is called the *secondary fever*, which sets in about the eleventh day of the disease, or the eighth of the eruption, just when the maturation of the pustules is complete, and they begin to desiccate. This secondary fever is slightly marked in the distinct small-pox, and very intense and perilous in most instances of the confluent. It is at this period of the disorder, that death, in the fatal cases, oftenest occurs. Of 168 such cases, recorded by Dr. Gregory, the deaths happened in twenty-seven (nearly one-sixth of the whole) upon the eighth day of the eruption. That, therefore, is the most perilous *day*, as the second is the most perilous *week*. Thirty-two died in the first week, ninety-nine in the second, twenty-one in the third. The early occurrence of death denotes a peculiar malignancy in the disease. The nervous system appears to be overwhelmed by the force of the poison. During the second week the disorder proves fatal chiefly in the way of apnoea; from some affection of the respiratory passages. After that period the characters of asthenia commonly predominate. The patient sinks under some casual complication, or the powers of life are gradually worn out by so much irritation of the surface, and so large an amount of suppuration.

So much for the ordinary course of small-pox, and of the symptoms that are essential to that disease. There are, however, other concomitant circumstances, with which you ought to be acquainted: and these I will endeavour to specify at our next assembling.

LECTURE LXXXVII.

Small-pox continued. Inoculation. Vaccination. Their comparative advantages. Treatment of Small-pox.

IN the last lecture I brought before you, in a rapid sketch, the ordinary course, and the essential symptoms of small-pox; both in its distinct and in its confluent form. I have yet to mention some other circumstances that are very frequently to be noticed in connexion with that disease.

Both kinds are accompanied by *sore throat*; the tonsils and fauces are tumid and red: and with this sore throat there is associated, about the period when the face swells, sometimes in the discrete variety, and almost always in the confluent, more or less *salivation*, which lasts for several days. At first the discharge is thin and plentiful: but, towards the period of maturation, it often becomes viscid and ropy, and is with difficulty got rid of by the patient. This salivation is of some importance as a *prognostic* symptom. If it cease abruptly, and especially if at the same time the swelling of the face suddenly and prematurely subside, the peril is great. Besides this, Sydenham regarded the *ptyalism* as a *diagnostic* circumstance; as a mark which identified with true small-pox a fever called by him the variolous fever, the *variola sine variolis* of De Haen and others. The resemblance (says he) which this fever bore, in its symptoms, to small-pox, induced me to give it the title of variolous fever, which seemed indeed so much the more appropriate, as the fever raged at the same time with small-pox, and got well under the same treatment. The two diseases belonged evidently to one family, and there was no difference between them, saving that in small-pox the morbid matter was directed towards the skin, in the shape of an eruption; while in the variolous fever this matter was expelled from the system by the salivary glands." Notwithstanding this statement, it is difficult to believe that any such disorder as *variola sine variolis* ever proceeds from the contagion of small-pox.

This affection of the salivary glands does not so often occur in children; but diarrhoea appears sometimes to take its place.

The soreness of the fauces often depends, in great measure, upon pustules there situate. You may see that the tongue, the roof of the mouth, the inside of the cheeks, the uvula, and the velum palati, are thickly studded with them. It is affirmed by some writers that the pustules of small-pox occur in various internal parts of the body, and especially upon the mucous membrane of the intestinal canal. I believe this to be a mistake. The enlarged solitary follicles often put on very much the appearance of pustules. Cotunnus, who has written a good book *De sedibus varicellarum*, asserts that pustules appear only upon the skin, and upon those parts of the mucous membranes which are freely exposed to the air. In one solitary instance he met with them in the trachea so low as its third ring. He fancied that previous desiccation of the part was necessary to their formation. He says that none appear on the cornea, while it is kept moist. He denies that they are seen in the interior of the body, or upon the foetus in utero: but in this last point he is certainly wrong; and this is a strong objection to his theory. The most striking facts which he alleges in support of his opinion of the necessary presence of air—besides the fact of the eruption being more copious on the parts usually exposed to the atmosphere, as the face and hands—are, that pustules do not form on the inner surface of the eyelids, except in cases of *ectropium*; that they appear on hæmorrhoidal tumours only when these project beyond the margin of the anus; and that that portion alone of the glans penis is ever affected by them which is uncovered by the prepuce.

Dr. Gregory, however, states that true variolous pustules do not form upon the conjunctival membrane: and that the blindness of one or both eyes, which is so common a result of small-pox, especially in children, is produced by an intense kind of ophthalmia, which sets in at the period of the secondary fever, and rapidly involves and spoils the transparent tissues of the organ.

During the period of maturation, a peculiar greasy, disagreeable odour, quite *sui*

generis, proceeds from the body of the patient. If taken into the sick chamber blindfolded, one might name the disease at once by the smell. About the same time also many patients are tormented by itching of the surface; so that they are provoked to scratch off the heads of the pustules; and by so doing they often ensure the formation of pits. In many cases of confluent small-pox this itching seems to constitute the chief part of the patient's suffering.

The various troublesome complications to which, in severe cases of the confluent form, the patients are liable during the secondary fever; erysipelatous inflammation involving the subcutaneous areolar tissue in various parts of the body, and leading to the formation of abscesses; glandular swellings in the groins and axillæ, going on sometimes to suppuration; sloughing sores on the hips and sacrum; phlebitis; and in two or three instances I have seen the large joints, after death, full of matter. One of the most serious symptoms, at this period of the disease, is dyspnoea. The air-passages, and especially the larynx, become clogged by viscid mucus, the arterialization of the blood is interfered with, and the patient is in danger of suffocation. Occasionally life is suddenly extinguished by œdema of the glottis, supervening upon that affection of the fauces which I mentioned just now.

In one most fearful phase of this always formidable disorder, symptoms indicative of what is called the putrid diathesis manifest themselves—petechiæ, vibices, hæmorrhages from various parts of the body. The pustules, instead of being plump and yellow, are flat, red, purple, or blue; that is, they contain blood, or a sanious ichor, in the place of pus, constituting the *variola nigræ* of Sydenham, the *bloody* small-pox of Mead. I believe that these appearances augur in all cases a fatal result. Hæmorrhage from the uterus is not uncommon; and in pregnant women abortion, and then, most commonly, death. Heberden says that he examined, in many instances, the fœtuses so parted with, but never could perceive upon them any traces of small-pox. His experience, therefore, agreed with that of Contunnius; and we may conclude that infection of the fœtus in utero is very rare. Yet, unquestionably, it sometimes happens; and the circumstances under which it has been noticed are various and interesting. In one instance, related by Mr. Flinders, the disorder was eight or ten days later in the fœtus than in the mother. A woman, near her full time, took small-pox. The pustules were mature about the 10th or 11th of June. On the 18th she gave birth to a full-grown boy, upon whose face and body there were many pustules, discrete, and nearly ripe. The child died the same night. It is a very curious fact that the fœtus had caught the disorder, doubtless through the medium of the mother, although she, having had it previously, was unaffected by the contagion. Dr. Mead relates that "a certain woman, who had formerly had the small-pox, and was now near her reckoning, attended her husband in this distemper. She went her full time, and was delivered of a dead child. It may be needless to add, that she did not catch it on this occasion; but the dead body of the infant was a horrid sight, being covered all over with pustules." In the first volume of the *Medico-Chirurgical Transactions*, Dr. Edward Jenner gives an account of an infant which, upon the fifth day of its age, became indisposed, and on the seventh exhibited the eruption of small-pox; so that the contagion must have been communicated to it while yet in the womb. A few days before her confinement the mother of this child had seen in the street a person covered with small-pox pustules, the smell and sight of whose body had sensibly affected her. I see no reason, therefore, for doubting that the unborn being may pass safely through the disease while in the womb, and derive from that attack the customary immunity for the future. My namesake, Sir William Watson, describes, in the *Philosophical Transactions*, an instance in which the scars left by the pustules were visible upon an infant at its birth. This child was afterwards inoculated without taking the disease. Its mother, who had formerly had it, nursed, when far advanced in pregnancy, a servant ill of small-pox. Dr. Pearson met with a similar example. Mary Spooner was inoculated by him in her sixth month of utero-gestation, and had the disease severely. Her child was twice inoculated with small-pox matter, but without effect.

Like all these contagious exanthemata, small-pox has its periods of dormancy, and its periods of activity. Every now and then, at irregular intervals—and, as it would seem to our ignorance of the cause, capriciously—it overspreads a district or country as an epidemic. At this moment (1838) it is more prevalent in London, and in

many parts of England, than it has been known to be for many years past. When epidemic, it is also, in general, more than ordinarily severe; although different epidemics vary much in that respect.¹

There is no contagion so strong and sure as that of small-pox: none that operates at so great a distance. Dr. Haygarth states, "that during his long attention to this subject, not a single instance had occurred to prove that persons liable to the small-pox could associate in the same chamber with a patient in the distemper, without receiving the infection." It is readily communicable in every way; by inoculation, by breathing a contaminated atmosphere, by the contact or vicinity of fomites. Nay, it may be caught from the dead body. Mr. Cæsar Hawkins has recorded an interesting example of this. The body of a man, who died of small-pox, was brought into his dissecting-room in Windmill Street; and four students took the disease from that source. Of these, one only had *touched* the body.

There is one appearance which I think curious, although perhaps it has not any great practical interest; and which I omitted to notice in the last lecture, when describing the course of the eruption. Without going minutely into the anatomy of the pustules, you may distinctly see, if you closely examine them when they are about five or six days old—you may see, at least in many of them—two colours, viz., a central whitish disk of lymph, set in, or surrounded by, a circle of yellower puriform matter. In truth, there is, in the centre, a *vesicle*, which is distinct from the pus. You may puncture the vesicle, and empty it of its contents, without letting out any of the pus; or you may puncture the part containing the pus, and let *that* out, without evacuating the contents of the vesicle. The vesicles have even, by careful dissection, been taken out entire; and they are said to consist of several little cells. It is most probable that the lymph contained in this separate vesicle is the purest part of the variolous poison.

Before I say anything of the measures to be adopted during the progress of small-pox, I have to bring under your notice two expedients of still greater interest and importance; the one of them contemplating a mitigation of the disorder, the other its total prevention. You anticipate that I am about to speak of *inoculated* small-pox in the first instance, and of the *vaccine disease* in the second.

I have many times stated, and all the world knows, that small-pox may be imparted to a healthy person by inserting beneath his cuticle a minute quantity of the matter taken from a variolous pustule. This, perhaps, is not very surprising; but it is surprising that the disease, so received, should be much milder than if it had been contracted in what is called the "natural way," by breathing an atmosphere charged with the contagious poison. Why it should be so it is difficult to conjecture. The fact is sometimes expressed by saying that the disease is milder when the virus is admitted through the cutaneous, than when through the mucous tissues. But I am not at all sure that the hypothesis involved in this proposition is true. No attempts have been made, that I know of, to introduce the poison artificially through a wound in any mucous surface. I should rather guess that the small quantity of the poison conveyed by inoculation into the blood may make the difference. But whatever the explanation, the fact is unquestionable, and obviously of the highest importance. By what accident it was first learned (for it evidently could not have been reasoned out) we do not know. The Chinese claim to have been in the habit for many centuries, of *sowing* the disorder, by putting some of the crusts into the nostrils. But this is a different thing from inoculation, the surface being entire, and the effluvia from the crusts being drawn into the lungs by the act of inspiration. It is said that a true engrafting of the virus has been in use by the Brahmins in India, time out of mind. It certainly was practised in Turkey at the very beginning of the last century, and perhaps somewhat earlier. In 1713, Dr. Emanuel Timoni, an Oxford graduate, who had settled at Constantinople, wrote to Dr. Woodward, in London, giving him an account of the new process, and testifying to its success. This account was communicated to the Royal Society, and published in its Transactions the following year.

¹ "In the present century there have been no fewer than six epidemics; the records of the Small-pox Hospital show that they occurred in the years 1825, 1838, 1844-45, 1848, 1851-52, and 1854-55-56. The last epidemic, which extended over parts of three years, was unusually severe as well as protracted, and reached its acme in May, 1855."—Report of the National Vaccine Board for 1857.

In 1715, Mr. Kennedy, an English surgeon who had travelled in Turkey, gave similar information to the English public in his *Essay on External Remedies*. And in the *Philosophical Transactions* for 1716 you may see a notice of the same process, as described by M. Pylarini, the Venetian consul at Smyrna. But these statements were neglected, or had no practical result. We owe the actual introduction of the practice of inoculation into Great Britain to the good sense and courage of an English lady, whose lively epistles have taken their permanent place in our country's literature. Lady Mary Wortley Montagu, the wife of our ambassador at the Ottoman Court, writes thus, from Adrianople, in the year 1718: "The small-pox, so fatal and so general amongst us, is here entirely harmless by the invention of *engrafting*, which is the term they give it. Every year thousands undergo the operation; and the French ambassador says, pleasantly, that they take the small-pox here by way of diversion, as they take the waters in other countries. There is no example of any one who has died in it; and you may believe I am well satisfied of the safety of this experiment, since I intend to try it on my dear little son. I am patriot enough to take pains to bring this useful invention into fashion in England." In fact, she recommended it by her own example. The first person inoculated with the small-pox in England was her daughter. Then a child of a physician, Dr. Keith, who had visited Miss Wortley; afterwards some condemned felons, who were pardoned on condition of their submitting to the experiment; and at length, some of the royal family. But the practice was not thoroughly established, nor properly appreciated, by the English public, until the middle of the century.

Its efficacy in mitigating the severity and danger of the disease, in saving life and preventing deformity, was signally great. The mortality in the natural small-pox was estimated at one in five. It is really higher. Mr. Marson infers, from the records of the Small-pox Hospital (where, however, the mortality is likely to be above the average) that the natural small-pox destroys about one-third of all whom it attacks.¹ But Baron Dimsdale, a great inoculator, declared that not one in fifteen hundred died of the engrafted disease. Two brothers, named Sutton, who had introduced, or rather revived, a very improved method of treating the disorder, professed to have inoculated 20,000 persons, without fairly losing one. But these, doubtless, were vastly exaggerated statements. Dr. Gregory says, "the average number of deaths at the Inoculation Hospital was only three in a thousand." The National Vaccine Board speaks decidedly of "one in three hundred" as the proportion of the inoculated that "will surely die" from the operation.

In the inoculated disease the period of incubation is comparatively short; the pustules are seldom numerous, and still more seldom confluent; and the secondary fever is generally slight or wanting.

I may mention here, also, that the eruption is not unfrequently preceded by a rash, something like that of scarlet fever, and called by Willan the *roseola variolosa*. It fades in the course of a day or two, and then the small-pox pustules are seen emerging just in the same state that they would have been in, at the same period, if no such rash had appeared. The efflorescence happens oftener in the inoculated than in the casual disease. In the former it is looked upon as rather a favourable sign; in the latter, especially if the rash be of a dark red colour, it is considered unfavourable, and as the herald of a severe confluent disorder.

A far superior expedient has since been discovered, in the practice of *vaccination*, which has rendered the inoculation of small-pox not merely unnecessary, but, in most cases, perfectly unjustifiable. Yet circumstances do sometimes arise, even now, in which it may be allowable and right to engraft the matter of small-pox; as when an unprotected person is unavoidably exposed, or has recently been exposed, to the contagion of that disease, and there is no vaccine matter at hand. The advantage of inoculating in such a case is, that the inoculated or milder form gets the start of the natural and severer; the fever commencing sooner than it would otherwise do. To show you the value of the practice in such cases, and the degree of protection which

¹ "Natural small-pox is a most fatal disease at all periods of life; the most so in infancy and advanced life; the least so from 10 to 15 years of age; under 5 years it is 50 per cent.; still greater, however, under 2 years; the mortality after the age of 20 rises suddenly, and increases gradually; at 30 it exceeds the mortality of infancy, and after 60 hardly any escape."—Marson, in *Med.-Chir. Trans.*

it affords to individuals whom we cannot vaccinate, I may mention a fact which Professor Gregory, of Edinburgh, was in the habit of relating, and which was told him by a naval surgeon. The small-pox was introduced among the crew of a man-of-war, in a tropical climate, where no vaccine matter was to be procured. The men were almost all unprotected. Sixteen of them took the disease in the natural way; and of these, nine, or more than one-half, died. Of 363 who were inoculated, under the disadvantages of a hot climate, and no preparation, not one perished.

That a disorder communicated to the human animal from one of the brutes should protect the former against the contagion of small-pox, is one of the most interesting facts in the whole history of medicine. How glimpses of a truth so remarkable were first revealed to the casual observation of certain peasants, and how the result of this chance observation was gradually "matured into a rational and scientific form by a mind deeply imbued with the best principles of sound philosophy," I have not leisure to tell you in detail. And it is the less necessary that I should do so, as you may find the whole subject thoroughly narrated and discussed by Dr. Baron, in his interesting biography of Edward Jenner.

Dr. Jenner found among the great dairy farms in Gloucestershire a popular belief that no person who had had the *cow-pox* (an eruptive vesicular complaint communicated from the udder of the cow to the hands of the milkers) could "take the small-pox." Satisfied, by inoculating with small-pox matter several individuals who had had the vaccine eruption, that this was not an unfounded notion, he at length conceived the great and happy idea of propagating the cow-pox from one human being to another, and so preventing, in all cases, the perilous and disfiguring distemper of small-pox, which he hoped might thus be finally expelled from the earth.

By degrees, Dr. Jenner ascertained that some persons, who had had sore hands from milking, were not thereby rendered proof against the contagion of small-pox; but this difficulty was soon cleared up by the discovery that the teats of cows were liable to different kinds of eruption, and he learned, by close observation, which of these was the peculiar eruption that produced in the human frame the protecting disorder.

Dr. Jenner set himself to trace, if possible, the origin of the disease of the cow. First, he found that it was peculiar to certain dairies; then, that in those dairies *men* were employed in milking. Following up this clue, he further made out that those men had also the charge of the farm-horses. Next, he learned that the teats of the cows generally began to exhibit the specific eruption at that time of the year when a complaint called "the grease" chiefly prevailed among the horses. Hence he concluded, that the malady was conveyed to the cows by the hands of the men who had been dressing the heels of horses affected with the grease. Subsequent inquiries have, however, shown that this conclusion was not strictly correct.

Another difficulty which lay in Dr. Jenner's way, and which his patience and sagacity surmounted, was this. He found that some who were casually infected from the true complaint in the cow were not protected. This depended, as he afterwards ascertained, upon the period of the disease in the cow, at which the virus was communicated to the milker. The thick matter proceeding from the vesicle late in its progress produced indeed a severer local sore than the thinner matter of its earlier state, but it did not confer the desired protection. The same thing is observed in respect of small-pox. If the matter used for inoculation be taken from a fully matured pustule, it does not so surely excite the disease as when taken from a more crude one.

The next important step in this most interesting investigation was to determine whether the vaccine disease could be transmitted, by engrafting, from one human being to another, and whether, if so transmitted, it retained its protecting power. The 14th of May, 1796, was the birthday of vaccination. "On that day, matter was taken from the hand of Sarah Nemes, who had been infected by her master's cows, and inserted by two superficial incisions into the arms of James Phipps, a healthy boy of about eight years old. He went through the disease apparently in a regular and satisfactory manner; but the most agitating part of the trial still remained to be performed. It was needful to ascertain whether he was secure from the contagion of small-pox. This point, so full of anxiety to Dr. Jenner, was fairly

put to issue on the 1st of the following July. Variolous matter, immediately taken from a pustule, was carefully inserted by several incisions, but no disease followed."

It is scarcely necessary for me to notice the objections which were made to the practice of vaccination. Some of them were merely foolish—as, that it was unnatural and impious to engraft the diseases of a brute upon a Christian. Others were untrue—as, that it introduced into the system new, unheard of, and monstrous disorders, distinct from the cow-pox itself. It triumphed over all these cavils; and in six years from its first promulgation the discovery was known in every region of the world.

It was soon found, however, that some, who had apparently had the cow-pox by inoculation, were nevertheless not incapable of taking the small-pox; and that these failures were, many of them at least, attributable to the mistakes that were made in the time or manner of performing the operation. It became necessary, therefore, to ascertain precisely the conditions requisite for the production of the genuine disease. And these conditions have been successfully investigated by Dr. Jenner and by subsequent observers.

You will learn to recognize the true vaccine vesicle only by repeatedly examining it for yourselves. Yet a brief description of its characters and progressive changes may be useful to you.

On the second or third day after the insertion of the vaccine matter into the arm, the puncture looks red and inflamed; and on the fourth or fifth day the vesicle becomes perceptible; a pearl-coloured elevation of the cuticle enclosing a minute quantity of a thin transparent liquid. It gradually increases in magnitude till the eighth day, when it should measure from a quarter to half an inch across. Like the pustule of small-pox, it is more prominent at its circumference than at its centre, and it consists of small cells, from ten to fourteen in number. By puncturing carefully one of these cells, a drop of the virus may be let out, the other cells remaining full. Up to the seventh, or eighth, or even to the beginning of the ninth day, the inflammation around the vesicle should extend to only a very small distance from it. After this, it spreads, and what is called the *areola* is formed; a circular red border, which continues to increase during the ninth and tenth days, and begins to fade on the eleventh, passing through shades of blue as it declines, and leaving a degree of hardness behind for two or three days more. By this time, a brown or mahogany-coloured crust has formed over the vesicle, of a nearly circular shape; this becomes gradually harder and darker, and finally detaches itself about the twentieth day. The cicatrix which it leaves should be distinct, somewhat less than half an inch broad, circular, slightly depressed, marked (sometimes) by radiating lines, with a well-defined edge, and dotted with little pits which seem to correspond to the cells of the vesicle.

About the eighth day there is usually some slight febrile excitement manifested, which soon subsides. This is analogous to the secondary fever of small-pox: and it appears to furnish the condition of the desired protection.

Of course it is of much moment to determine whether the cow-pox has run its proper course or not; and it is not always easy to say how far the progress of the vesicle may deviate from that which has just been described, without failing of its protecting influence. A very ingenious test of this, free from all ambiguity, has been devised by Mr. Bryce. His plan is this. He vaccinates the other arm, or some other part of the body, four or five days after the first vaccination. If the constitution have been properly affected by the first operation, the inflammation of the second vesicle will proceed so much more rapidly than usual, that it will be at its height, and will decline and disappear, as early as that of the first: only the vesicle and its areola will be smaller. In fact, from the time of the formation of the areola, the second vesicle is an exact miniature of the first. If the system have not been duly influenced by the first vesicle, the second will run its own course, increasing up to its eighth day, and so on. Should this be the case, the second vesicle should be tested by a third.

We find the germ of this criterion in the early history of vaccination. Dr. Jenner vaccinated the children of his friend, Mr. Hicks, the first gentleman who consented to adopt the practice. This Mr. Hicks became afterwards an expert vaccinator himself, and it was his custom, in a doubtful case, to perform a second vaccination a

few days after the first: and he remarked that the second vesicle made "immense strides to overtake the first."

After some time it became apparent that Dr. Jenner's estimate of the protecting power of the vaccine disease had been set too high. He had hoped and believed, as others also had, that the cow-pox would in all cases prove a perfect and permanent protection against the small-pox; but those hopes have been disappointed. Doubtless complete protection is the rule; but — how thoroughly and regularly soever the vaccine malady may have proceeded—it is most certain that very many exceptions to this rule have taken place, and are daily taking place around us.

And this fact, which has long been too glaring to be denied or explained away, has depreciated the value of the process of vaccination in the public esteem, far more than, if rightly considered, it should have done. For it is a remarkable and most important truth that the disease which, in some duly vaccinated persons, follows exposure to the contagion of small-pox, is much milder and shorter even than the inoculated, and *a fortiori* than the natural small-pox. The disorder thus occurring is, therefore, denominated the *varioid* disease, or (more conveniently, in my opinion,) the *modified* small-pox, or *post-vaccinal* small-pox.

The constitutional symptoms of this modified disease are, in general, at the outset, and for several days, much the same with those of the regular small-pox. The eruptive fever is of equal length and intensity. There is frequently much headache, and sickness, and sometimes even delirium. The eruption begins about the third day: it is often copious, and sometimes confluent; and in the confluent cases the eruptive fever does not entirely subside so soon as the crop of pimples has come out.

It is in its subsequent progress that the complaint is modified: in respect both to the appearances presented by the skin, and to the constitutional symptoms.

Three distinct kinds of eruption have been observed —

1. The eruption sometimes approaches in its character and course very nearly to that of the ordinary small-pox. The pustules fill up, have the central depression, and ultimately crust over, and the face swells. But this course is performed in a shorter time than that of the ordinary disease, and the pustules are usually smaller. This is the severest and the least common form of the modified small-pox.

2. Sometimes the papulæ show a little fluid on their tops only, but never fairly suppurate, nor break; but the vesicles dry up, and hard prominences remain, with livid bases and horny summits.

3. There are other cases in which a great part of the eruption consists of red pimples, which soon become livid, but contain from first to last no fluid whatever.

In the majority of instances of modified small-pox, all these forms of eruption co-exist. Some of the papulæ go on to suppuration, others become crowned with a horny summit, and others never exhibit any fluid at all.

But the most important characteristic of the modified disease, is the total absence of secondary fever. The constitutional disturbance which, for the first week, may have been as severe as in the ordinary small-pox, generally subsides entirely when the eruption has reached its acme. The patient is convalescent just when, in the unchecked and regular form of the malady, his danger is beginning to be most urgent.

These two circumstances, then — the short duration of the eruption, and especially the absence of secondary fever — furnish the broad distinctions between the regular and the modified small-pox: and almost always, when vaccination has been thoroughly effected, and small-pox occurs afterwards, it occurs in this modified form; and the modified form of small-pox is seldom fatal, though instances of death resulting from it now and then happen.

In relation to this modified or post-vaccinal disease, several questions of the highest practical moment and interest have arisen, which by slow degrees, and under careful and multiplied observation, may now be said to have found their solution.

The first is, whether the protecting influence of cow-pox upon the human frame diminishes by lapse of time, and at length wears out. There is ample evidence to show that, sometimes at least, it does. Certainly in many, but not in all, of those who have gone through the vaccine disease, vaccination repeated at a distant period reproduces, in a greater or less degree, its primary effects. A friend of mine, who was vaccinated in 1799, has a son nine or ten years old, who was vaccinated at the

age of three weeks. Both of them have lately been revaccinated. The boy was somewhat affected by the renewal of the operation; the father not at all.

It may well be doubted whether *all* those who are susceptible of some impression from a second vaccination would become infected with small-pox under ordinary exposure to its contagion. That many of them would so contract the disease, and that all of them would be *endangered* by such exposure, is too certain. And a second question immediately presents itself; namely, whether this repetition of the operation of engrafting the cow-pox renews, or adds to, their security against small-pox. Happily, this question may also be answered in the affirmative; and answered by statistics of the amplest comprehension. In his able and most conclusive digest of the whole subject, published by the General Board of Health, Mr. Simon shows that during the five years, 1833-7, though small-pox infection had been sixteen times imported into different regiments of the army of Wirtemberg, there had ensued among the 14,384 revaccinated soldiers, one single instance only of modified small-pox. Still more satisfactory experience is that of the Prussian army. "In Prussia (as in Wirtemberg) the practice of revaccination grew out of the knowledge that small-pox would attack a certain proportion of those who had been vaccinated only in infancy. During the ten years preceding 1831, cases of post-vaccinal small-pox were increasing in number and fatality, and within the three years 1831-33 there had occurred no fewer than 312 *deaths* by small-pox. For the last twenty years the Prussian army has represented an almost entirely revaccinated population. And what has been the contrast? 104 annual deaths by small-pox was the last experience of the former system; two annual deaths by small-pox has been the average for the revaccinated army. Analyzing moreover the forty fatal cases of small-pox which during the last twenty years have occurred in the Prussian army, we find that only four of the number were of persons who (it is said) had been successfully revaccinated."

Similar facts, equally cogent with these, may be gathered from the experience of other countries. Mr. Marson, the resident surgeon to the Small-pox and Vaccination Hospital, London, states "that not one of the nurses or servants of the hospital has had small-pox, for the last twenty years. They have all been either vaccinated or revaccinated on coming to live at the hospital." The same gentleman holds "that as a matter of safety, it is well for all persons who were vaccinated in infancy to be revaccinated at puberty, this measure being more especially requisite for those who, though vaccinated, have no cicatrix remaining." He recommends also, as a matter of precaution, that all persons should be revaccinated on the appearance of small-pox in the house wherein they are residing.

But, thirdly, is there any ground for supposing that the wished-for protection ever fails to be conferred, because the operation is performed too early? None whatever that I know of. In fact there is unquestionable evidence that, for the full attainment of its defensive purpose, *gratuitous* vaccination at least is, in this country, performed too late. It appears from official tables published under the authority of the Registrar-General, that no less than one-fourth—*i. e.* twenty-five per cent.—of the whole mortality from small-pox in England and Wales happens in infants less than one year old; and as much as eleven per cent. within the age of four months. Within the fifth year the proportion reaches the enormous amount of from seventy-five to eighty per cent. These facts proclaim the necessity of *early* vaccination. It should be as early as is consistent with the safety of the child. Certainly it should never be delayed, except under special circumstances of excuse, beyond the third, or at most the fourth month after birth.

A fourth question is how far the frequent failure, in late years, of complete protection can be ascribed to the circumstance that the vaccine virus has been repeatedly transmitted from one human being to another, and its supply thus kept up, without any fresh recurrence to the cow, the original source of the disorder. Dr. Jenner was, himself, not without apprehension that this might prove a cause of failure. For one year I had a seat, as the Senior Censor of the College of Physicians, at the National Vaccine Board, and I then had opportunities of satisfying myself that lymph which had been transmitted without interruption from person to person ever since the time of Jenner, continued to generate what seemed a very perfect cow-pox vesicle. And it is the expressed opinion of the permanent members of that Board, "that the

vaccine lymph does not lose any of its prophylactic power by a continued transit through successive subjects." Mr. Simon has, however, stated some strong grounds for suspecting that the "occasional impermanence of protection may depend upon impairment in the specific power of vaccine contagion,—an impairment arising in the transmission of that contagion through many generations of men." It was alleged by M. Brisset, in France, as early as 1818, that the past ten years had made a marked difference in the visible characters of the vaccine vesicle: that it had become necessary to establish, instead of Jenner's two vesicles, eight or ten points of infection. Dr. Meyer, of Kreutzburg, states that on examining, in 1824-25, nearly four thousand vaccinated persons of all ages, he found the older scars much better marked than the recent ones; that, according to the testimony of many vaccinators, the proportion of unsuccessful to successful vaccinations was every year increasing; and that the cicatrices resulting from his own use of lymph recently obtained from the cow, were again after the old normal type. Dr. Gregory and Mr. Estlin, in this country, have adduced similar facts in evidence "that the vaccine-lymph, by passing through the bodies of many persons, loses, in process of time, some essential part of its activity."

This suspicion gathers force from a very curious result of the experience furnished by the Prussian army. It appears that where the vaccine supply has seldom or never been renewed from the cow, the proportionate resusceptibility of vaccine disease at a given age (and therefore it may fairly be presumed the susceptibility of small-pox also) has undergone a progressive increase: just as post-vaccinal small-pox has undergone a successive increase. "And (argues Mr. Simon) it is difficult to conceive how the infantine generations of a country could, crop by crop, successively derive less permanent constitutional impressions from vaccination, unless the efficient cause of those impressions—the vaccine contagion itself—had year by year undergone enfeeblement of its powers."

On this point, as well as on others, the statistical experience of the Prussian army is immense. The revaccination of recruits "extends annually to some forty or forty-five thousand operations. It is reported upon annually. Its records run back twenty-four years. Its subjects are of like age, in like proportions, and under like circumstances. When this system of revaccination commenced, in 1833, the proportion of successful results (of those, that is, who again *took* the disorder) was thirty-three in every hundred. Now the annual per-centages of successful results, for the whole time during which revaccination has been practised in that army, runs thus: 33, 39, 42, 46, 49, 50, 51, 54, 57, 58, 57, 57, 58, 60, 64, 64, 64, 61, 64, 69, 69, 69, 69, 70. *The last proportion of success exceeds the double of that with which the series commenced.*" Supposing the first vaccination to have preceded the second by twenty years, "the vaccinations of 1836—tested by eventual resusceptibility to cow-pox—were not half so stable as the vaccinations of 1813."

In the fifth place, there are yet moot points respecting the number of vesicles, and the degree of constitutional disturbance, which are requisite to insure, and to prolong, the protective power of vaccination. The constitutional effect will bear some proportion to the number of vesicles; and of these, it would seem, there should be several; and one or two of them, at least, should be suffered to pursue their entire course untouched. Mr. Marson believes that "vaccination may be relied on, when four or more vesicles have formed which have left good dotted cicatrices." During the five years of 1852-56, of 353 patients admitted with small-pox after vaccination into the Small-pox and Vaccination Hospital, having each four or more vaccine cicatrices, one only died of small-pox.

The same gentleman, who tells us that he has vaccinated between forty and fifty thousand persons, asserts that "one of the principal causes of failure in vaccinating, and of subsequent insecurity of the individual, even when the vaccination does take effect," is the want of care in the selection of the vaccine lymph. "Lymph for use is in its best state on the seventh day of the progress of the vesicle it is taken from—the day week from the vaccination. It should be taken when the vesicles are plump, and just before the formation of the areola. Under no circumstances should it be taken for use later than twenty-four hours after the areola has begun to form." If this rule were invariably observed, there would be, Mr. Marston believes, very few cases of severe small-pox after vaccination. "A serious error in vaccinating is the use of blunt lancets. It is impossible to have a lancet too sharp for vaccinating."

"The lymph should be introduced by a puncture of a valvular shape, from above downwards, so managed that the lymph at each puncture may gravitate into the wound. In this way, the lymph may be introduced in five punctures—the number I recommend—from half to three-fourths of an inch apart, without recharging the lancet: care being taken that the punctures are not bruised." "With good lymph, and the observance of all proper precautions, an expert vaccinator should not fail of success, in his attempts to vaccinate, above once in one hundred and fifty times; yet a large number of those who take upon themselves the duty, think they do very well if they succeed, however imperfectly, five times out of six."

With regard to a sixth question, the most important of all, we may speak very decidedly; and it is a question concerning which it is of the utmost consequence that medical men should form, and disseminate among the public, correct opinions: I allude to the comparative merits and advantages of *inoculation with small-pox*, and *vaccination*.

The advantages of the practice of inoculation to the individual, supposing him doomed to have small-pox, were great and obvious; to the community at large they were very doubtful. It gave the undoomed individual, for certain, an ugly disease, which was comparatively free from danger, in exchange for the chances, on the one hand, of contracting a very hazardous form, and on the other, of escaping altogether from any form, of variola. We need not inquire which is the most eligible branch of this alternative; we know which was by most men actually chosen. But the practice of inoculation, by carrying the virus and the disease into every village throughout the length and breadth of the land, filled the country with contagion; insured the disease to all who were subjected to the operation, and diminished to all who were not, the chances of escaping it. No doubt the distemper was produced artificially in many more persons than would have caught it naturally, had inoculation never been thought of. So that while the relative mortality, the per-centage of deaths from small-pox, was lessened by this practice, the absolute mortality was fearfully increased. Such at least is the judgment expressed by most who have thought and written on the subject. Dr. Heberden compared the number of deaths ascribed in the London bills of mortality to small-pox during the first thirty years of the last century, with the number during the same period of years at the close of the century, and he found that they had increased from 7·4 per cent. to 9·5 per cent. To be sure, some allowance must be made for the increase in the whole population of London during that interval; but on the other hand we must take into account the deaths (not noted in those bills) which followed the inoculation of small-pox in secluded villages, where, but for that practice, the poison might seldom have been found. It is right, I say, that this matter should be steadily contemplated, in all its lights, and with all its shadows, in order that the unspeakable blessing conferred upon mankind by the researches of Dr. Jenner may be fairly set forth, and adequately appreciated. The vaccine virus produces a slight disorder, which is attended with no risk, and which (unluckily I may say) is not communicable except by direct engrafting. It not only does not disseminate a dangerous and deadly poison, but if rightly used, it affords the means of eradicating from a well-regulated community, or at least of confining within narrower limits, the most loathsome pestilence which the world has known. Where vaccination is, the contagion of small-pox need never come. In Denmark, as I told you, variola had at one time disappeared before the defensive influence of compelled vaccination. Chance, and a careless security, engendered by the absence of the pest, have led to its reintroduction there. It is much to be regretted that the vaunted liberty of this country has hitherto rendered it almost impossible to enforce by law a practice which would be so conducive to the public weal. Some good might be done by enacting that no person should be eligible to even any parochial office of trust, honour, or profit, who could not produce a certificate that he had been duly vaccinated. Compulsion in some form is clearly defensible and just; and compulsion by *fine* is perhaps the least objectionable form. It is well remarked, in an instructive report on this subject, by a Committee of the *Epidemiological Society*, that "though it may be doubtful how far, in this free country, it is justifiable to compel a person to take care of his own life, or of that of his offspring, it can scarcely be disputed that no one has a right to put in jeopardy the lives of his fellow-subjects. The principle of so using one's own as not to injure

another's, is one which has always been acted upon in our legislation as regards property and personal nuisances; and it is but an extension of the principle to apply it to questions of life and health." Statistical returns have shown that the proportionate mortality from small-pox in England and Wales, is considerably more than double what it is in any of those continental States in which vaccination is more or less stringently enforced.

A so-called *compulsory* Act was indeed passed in 1853; but as no public officer of any kind was appointed whose duty it should be to warn or to proceed against offenders, the Act has become nearly a dead letter.

The benefits which this safeguard, of vaccination, confers on the individual are scarcely inferior to those which it is calculated to bestow upon society. It unfortunately does not give complete protection against small-pox to all, but it gives complete protection to many. And you must recollect that small-pox itself is not a universal and absolute assurance against its own return. But the cow-pox relieves all from the necessity, imposed by inoculation, of coming within the sphere of the variolous contagion. It renders many, I repeat, impregnable to that poison, if they do chance to be within its range; and its advantage to the comparative few who suffer the double misfortune of being exposed to the contagion of small-pox, and of being affected by it, is this, that it gives safety, though not exemption; that it takes away the sting and peril of the variolous disease, by curtailing it of the secondary fever. At the very worst, it leaves the individual liable, by a twofold ill luck, to contract a form of small-pox not more dangerous than that which he would voluntarily accept by submitting to the operation of inoculation.

It is not difficult to adduce authentic evidence in illustration of this reasoning; indeed I have already put before you incidentally much and striking evidence of that kind.

The following tables were compiled by the committee just now mentioned, to show the gradual diminution in the mortality from small-pox in London, as compared with the mortality from all causes, since vaccination has been introduced; notwithstanding its hitherto imperfect employment.

Table showing the average of deaths from small-pox out of every 1000 deaths from all causes within the bills of mortality during the last half of the last century—the half century preceding vaccination.

For the 10 years ending 1760	100
“ “ 1770	108
“ “ 1780	98
“ “ 1790	87
“ “ 1800	88

Table showing the same during the first half of the present century—the half century succeeding the introduction of vaccination.

For the 10 years ending 1810	64
“ “ 1820	42
“ “ 1830	32
“ “ 1840	23
“ “ 1850	16

From the same source I take the following significant statement respecting the prevalence of small-pox as an epidemic in London.

The frequency of epidemics in London has been:—

Before protection as 42;
During inoculation as 54;
During vaccination as 14.

It is shown in Mr. Simon's Blue-book that the fatality of small-pox in Copenhagen is but an eleventh part of what it was before the introduction of vaccination; in Sweden, little over a thirteenth; in Berlin, and in large parts of Austria, but a twentieth; in Westphalia, but a twenty-fifth.

An able paper, to which I am desirous of directing your attention, has been published on this subject, since I previously addressed you, by Dr. Stark, in the 64th volume of the *Edinburgh Medical and Surgical Journal*.

His object is to prove that the increased mortality from small-pox in this country

of late years, is owing more to the *neglect* of vaccination, than to any failure of its protecting power.

He shows from the reports of the Registrar-General, and from other authentic documents, that in London, Manchester, Liverpool, Edinburgh, Glasgow, Perth, and Dundee, more than 80 per cent. of all the deaths from small-pox, happen in children under five years of age.

Now it is in this very class, *if duly vaccinated*, that the mortality ought to be the least; whether we suppose the protecting power of the vaccine virus to be lessened and short lived in consequence of continual transmission through a series of individuals; or whether we suppose it gradually to wear out in each individual subjected to its action.

Dr. Stark adduces also ample statistical evidence that in many of the great European States, a large proportion of the children born, remain unvaccinated.

He brings forward statements which confirm the fact, already mentioned, that small-pox itself is by no means a sure protection against a recurrence of the same disease.

Small-pox after small-pox is supposed to be much less common than small-pox after cow-pox. But in estimating this proportion we must bear in mind the comparative rarity, now-a-days, of primary small-pox. If, indeed, we could trust to the test of revaccination, we might conclude that small-pox is not a more effectual safeguard against small-pox, than the cow-pox is found to be. In the Hanoverian army, in the years 1837, '38 and '39, revaccination produced the true cow-pox vesicle in 11 persons out of every 100, was partially efficient in 27, and failed altogether in 62. A number of the men who had gone through small-pox, were also subjected to the general vaccination, and, curiously enough, its various effects upon them were in precisely the same proportions, as among those who had previously been vaccinated.

So Heim gives the following comparative view of the results of revaccination in the army of Wirtemberg.

Among 100 men who were vaccinated after having had small-pox, the operation succeeded in 32, produced a modified effect in 26, and no effect at all in 42.

And among 100 men who were vaccinated for the second time, the operation succeeded in 34, produced a modified effect in 25, and no effect at all in 41.

But the most striking part of Dr. Stark's paper is that in which he collects and exhibits evidence, which seems perfectly conclusive, of the immunity conferred, for the most part, by vaccination, from the subsequent occurrence of fatal small-pox.

In a general population like our own; all observation on this point is vitiated and made valueless by the uncertainty that exists respecting the ratio of the vaccinated to the unvaccinated portion of the community. It is different in our armies. Every recruit is closely examined: and if he have not previously undergone cow-pox, or small-pox, he is forthwith vaccinated. Mark now some ascertained facts relating to men thus cared for, of nearly the same age, living in the same place, surrounded by similar external circumstances, and subjected to constant and vigilant supervision.

From the Government "*Statistical reports of the sickness, mortality, and invaliding, among Her Majesty's troops*" for 20 years, viz., from 1817 to 1836 inclusively, we learn that

In Dragoon Regiments and Guards, with an aggregate strength during that period of 44,611 men, and a total mortality of 627, there were but three deaths from small-pox.

Among the troops at Gibraltar, one death only from small-pox occurred, the aggregate strength being 60,269, and the whole mortality 1291.

In the West Indies, although several epidemics of small-pox had ravaged the islands within that period, not one person died of the disease among the British or white troops, with an aggregate strength of 86,661, and a total mortality of 6803: while among the black troops on the same station, with an aggregate strength of 40,934, and a mortality of 1645, there was not even one case of small-pox.

"At Bermuda, Nova Scotia, New Brunswick, Cape of Good Hope, and the Mauritius, not a single death from small-pox occurred during those 20 years; and even the white troops of Western Africa wholly escaped this disease, which was carrying off hundreds of the black unprotected population."

In Malta, from 1818 to 1836 inclusively (a period of 19 years), the aggregate strength of the British troops was 40,826, the total mortality 665, and the mortality from small-pox 2. Yet in the years 1830 and 1831, small-pox raged there as an epidemic, and destroyed 1169 persons: for in 1830, there died of small-pox 1048, the total mortality being 3407; and in 1831 there were 121 deaths from small-pox, out of an aggregate mortality of 2583.

Again: in Ceylon three epidemics of small-pox occurred during the 20 years included in the Government reports; namely,

In 1819, when of the natives 7874 took the disease, and 2945 died.					
" 1830,	"	"	806	"	" 169 "
" 1834,	"	"	425	"	" 94 "

Yet in the same island, during the very same period, there were among the white troops, with a total mortality of 3000, 4 deaths from small-pox (out of 8 cases); among the Malay troops, with a total mortality of 858, 9 deaths from small-pox; among the pioneer corps, with a total mortality of 647, 1 death from small-pox. And in the last of these epidemics, Dr. Kinnis states that not one instance of the disease appeared among the white or native troops.

Facts equally conclusive are to be found in the statistics of our armies in India. But I need not go into further detail.

Surely we may perceive—in authenticated statements like these—how inadequately the great preventive remedy of small-pox has hitherto been for the most part applied in this country; how successful might become its thorough application. Surely now that the maxim *salus populi suprema lex* is beginning to be acknowledged by our legislators, any general sanitary enactment must be held defective which does not provide for and compel effectual vaccination, and punish variolous inoculation, and shut up in strict quarantine every case of small-pox as soon as its existence in the community is discovered. By these three provisions—and probably by nothing less than these—the seeds of that dire distemper would gradually become scarce and finally disappear; and the soil upon which they might still chance to light would be made unfruitful of the deadly harvest.

There yet remains a highly interesting, but a less practical question. Dr. Jenner, as I stated before, believed that he had traced the cow-pox to its origin in the heels of the horse affected with *the grease*. It has since been made out, that the disease which, in the horse, corresponds with and produces the specific malady of the cow, is a vesicular eruption, having no necessary connexion with the grease, but extending sometimes all over the animal's body. Now the question is, whether these two distempers, occurring in the cow and in the horse, are identical in their essence and nature with the small-pox of man. If so (as Dr. Jenner believed, and Dr. Baron strongly maintained), a part of the mystery attending the whole subject vanishes. The protection furnished by the cow-pox resolves itself into the more familiar law, that certain diseases engendered by animal poisons, happen to the same individual but once, and shield the body against their own recurrence. In conformity with this theory, Dr. Baron named the disorders respectively, *variola*, *variola vaccina*, and *variola equina*.

The notion, you see, was this, that the vaccine disease is in truth small-pox, rendered mild by passing through the system of the cow. The great object of inoculating the small-pox is to produce a benignant form of that disease, by diminishing the number of pustules. The cow-pox diminishes the number to one; and while it reduces the severity of the disorder to a *minimum*, it absolutely takes away its power of propagating itself, except by a direct engrafting of the visible virus. The disease is not sufficiently intense to taint the air with poisonous effluvia. At the same time it affords (*perhaps* somewhat less surely and less permanently) the customary protection. Such was Jenner's theory, which was intelligible and plausible, and supported by strong facts and persuasive reasoning; for all which I may again refer you to Dr. Baron's book.

This plausible and attractive theory has become demonstrated fact. Researches subsequent to Jenner's have made it "matter of almost familiar experiment" (I state the whole proposition in Mr. Simon's condensed but clear language) "that the infection of small-pox may, by inoculation, be communicated from man to the cow; that

its result is an eruption of vesicles presenting the physical characters of cow-pox; that the lymph from these vesicles, if implanted in the skin of the human subject, produces the ordinary local phenomena of vaccination; that the person so vaccinated diffuses no atmospheric infection; that the lymph generated by him may be transferred, with reproductive powers, to other unprotected persons; and that, on the conclusion of this artificial disorder, neither renewed vaccination, nor inoculation with small-pox, nor the closest contact and cohabitation with small-pox patients, will occasion him to betray any remnant of susceptibility to infection."

To Dr. Gassner, of Günzburg, to Dr. Thiele, of Kasan, to Mr. Ceely, of Aylesbury, and to Mr. Badcock, of Brighton, belong the praise of having worked out, by careful and repeated experiments, this most important truth.

If, as Dr. Heim asserts, there are no less than five kinds of spurious cow-pox, all communicable by inoculation from the teats of the animal to the human body, it follows that, in having recourse, from time to time, to lymph recently obtained from the cow, it should be such lymph only as results from inoculation of the cow with small-pox.

[The following presents a very fair exposition of the views of a large portion of the physicians of the Continent of Europe in relation to the protective powers of vaccination. It is the substance of a report made to the Academy of Sciences of France, on the 25th of February, 1845, by the Committee on Vaccination, to which were referred the various essays sent in by the competitors for the prize proposed by the Academy for the most satisfactory solution of the following questions:—

1. Is the preservative power of vaccination absolute, or merely temporary? If it is temporary only, determine, by accurate experiments and authentic facts, what is the period for which the vaccine matter exerts its protective influence against small-pox?
2. Has vaccine matter, taken directly from the cow, a more certain and durable protective power than vaccine matter transmitted a greater or less number of times through the human subject?
3. If the protective power of vaccine matter becomes enfeebled, should it be renewed; and if so, how?
4. Is it necessary to vaccinate the same individual several times, and if so, after how many years should the vaccination be repeated?

The portion of the report of which the following is an abstract, relates to the first two questions only.

*The protecting power of vaccination being definitely established, the question arises—*is it possible, after forty-five years' experience, to determine the limits of that power? The answer to this question is extremely difficult, as it embraces inquiries not in France alone, but throughout the whole world: in fact, a general investigation of every case in which vaccination had been formed could alone supply the fundamental elements of the problem to be answered. The competitors for the prize have particularly examined how vaccinated persons are circumstanced during the prevalence of epidemic small-pox; in other words, what is the proportion of vaccinated persons in the entire number of those attacked with small-pox. The protective power of vaccination is, by this mode of investigation, reduced to a numerical question. An attentive examination of what occurred during thirty epidemics of small-pox in France, shows two important facts:—First, that somewhat more than one-third of the entire number of persons attacked with the small-pox had been vaccinated; secondly, that the mortality among the vaccinated persons was very small. According to the author of one of the memoirs, more than one-third of those attacked in the epidemics which occurred at Montbeillard had been vaccinated, but there was no corresponding increase in the amount of mortality amongst the vaccinated patients; and the same result was observed in the epidemic of 1828, at Marseilles. The same results follow, also, from the examination of the epidemics that have occurred in England, Sweden, Denmark, Italy, Malta, Geneva, &c.

The fact, then, being established, that vaccinated persons can become affected with small-pox, and the proportion so attacked during the epidemics being nearly determined, a most important problem remained to be solved—viz., *What was the condition of the vaccinated persons affected as regarded the mere fact of their vaccination?* The authors of all the memoirs agree in stating that vaccinated persons were not affected indiscriminately or by chance, as it were; on the contrary, the small-pox seems to make a kind of selection from amongst them. With some exceptions, the

small-pox attacks those who have been vaccinated since a long period, and spares those who are recently so. An examination of the tables published in various parts of Europe proves positively, that children are seldom attacked with small-pox before the ninth year after vaccination; and also the converse fact, namely, that it attacks in preference persons who had been vaccinated fifteen, twenty, thirty, or even thirty-five years previously. A general fact, which may be anticipated from the history of eruptive complaints is, that after the age of thirty-five years, the aptitude of vaccinated persons to contract small-pox becomes so slight that it may be considered as having vanished.

An investigation of the facts relative to the concurrence of small-pox in vaccinated persons leads to the three following conclusions:—First. The protective power of vaccination is absolute and general for the first five or six years, and even to the eleventh or twelfth year, to judge from the experiments on revaccination. Second. After the foregoing period, a part, but a part only, of those vaccinated again become liable, especially under the influence of an epidemic, to contract small-pox. Third, The greater number of those vaccinated probably remain completely protected from small-pox during their entire life.

Has the cow-pox, taken directly from the cow, a more certain and permanent protective power than vaccine matter that has been transmitted more or less frequently through the human constitution? The experiments contained in several of the memoirs confirm the observations made by the Committee on Vaccination, at Paris. The greater intensity of the new vaccine matter, as compared with that long in use, is a fact definitely established by experience in England, Germany, Italy, and France. But is this greater intensity coupled with a greater preservative power? or, as the report puts the question—Is there any relation between the lesser or greater intensity of the local phenomena and the protective power of the *variolous* matter? The experiments made on this point show that the protective power of vaccine matter is not proportionate to the intensity of the local symptoms, but that vaccination with matter taken from the cow is more certain than with old vaccine matter. *Admitting that the protective power of vaccine matter diminishes with time, should it be renewed, and if so, how?*

As to the means of renewal, the first mode employed was the transmission of the vaccine matter from man to the cow—an experiment frequently performed as a matter of curiosity, but only recently sought to be rendered a means of restoring to the vaccine matter its lost energy. The authors of several of the memoirs maintain that the cow, when thus vaccinated, restores the vaccine matter unaltered, and therefore unregenerated; but the commission of the Academy thinks this conclusion too absolute: in fact, it has been established by the experiments of the author of one of these memoirs, that vaccine matter taken from man *is* regenerated during its transmission through the cow. The same fact results from thousands of experiments made in Bavaria under the direction of the government. Vaccine matter thus regenerated failed in less than *one case in a hundred*, while the failures of the old vaccine matter were nearly *three per cent*. Would it not be better to transmit the vaccine matter through several cows in succession than through one only? The mode, however, which should be preferred to all others—the only one on which we can entirely rely—is, as recommended by Jenner, to obtain vaccine matter from its original source. Several circumstances seem to show that the cow-pox is perhaps of less frequent occurrence than is commonly supposed, and the commission suggests, that those who happen to meet with it should not content themselves, as has been done hitherto, with transmitting it to man, but transmit it to other cows, and thus regenerate the infection.

Is it necessary to vaccinate the same person several times? and if so, after the lapse of how many years should the revaccination be performed? On this head the report first refers to the fact that the revaccinations, performed for a considerable period after the discovery of vaccination, did not succeed, except in some rare cases, because they were performed too soon after the primary vaccination. But when, at a later period, experience showed that the protective power of vaccination diminished with time, the practice of revaccination was resumed, and then succeeded beyond expectation. In some parts of Germany, especially, revaccination has been practised universally in the army, and even in civil life. Physicians, also, who had had small-

pox, in some instances revaccinated themselves with success, of which Dr. Heim is a remarkable example. He attended on his brother for three weeks, while he laboured under confluent small pox, and three weeks after having gone through this decisive trial, he vaccinated himself, and had pustules almost of the ordinary size. M. Moreau, the celebrated accoucheur, who had small-pox in early life, revaccinated himself three times with success.

A document published by the Government of Wurtemberg, which showed that, of 1677 persons affected with small-pox, between the years 1831 and 1836, 1055 had been vaccinated, contributed greatly to extend the practice of revaccination in Germany, and in the north of Europe. In France, the statistics of epidemic small-pox show that the number of vaccinated persons attacked with small-pox constitute more than a third of the whole number of patients affected. It is impossible, therefore, to doubt the propriety of practising revaccination. It is during epidemic small-pox, especially, that the utility of revaccination becomes obvious. Not only have individuals been thus protected, but the spread of the epidemic has been arrested. In Prussia revaccination has been practised in the army since 1833, and the small-pox has been almost entirely extirpated. In Wurtemberg, but one case of variola occurred in five years, among 14,384 revaccinated soldiers, and three only among 29,684 revaccinated civilians. Epidemic small-pox has not appeared in France since 1830, the period when revaccination was commenced. The authors of the memoir agree that during epidemics it is prudent to revaccinate after about the eighth or ninth year.

The answers given by the competitors for the prize to the questions proposed by the Academy, may be thus summed up:—

1st. The preservative power of vaccination is absolute for the majority, and temporary for a small number; and even in the latter it is absolute until adolescence.

2d. Small-pox rarely attacks those who have been vaccinated in infancy before the age of ten or twelve; from which age, however, until thirty or thirty-five, they are particularly liable to small-pox.

3d. Even in those cases in which it fails to protect the individual from an attack of small-pox, vaccination, nevertheless, so modifies the animal economy, as to render the symptoms of small-pox milder, abridge its duration, and considerably diminish its danger.

4th. Vaccine matter taken directly from the cow causes local symptoms of greater intensity; its effects are also more certain than those of old vaccine matter, but after being transmitted for a few weeks through the human subject, the local intensity disappears.

5th. The preservative power of vaccine matter does not seem to be intimately connected with the intensity of the symptoms of vaccination; nevertheless, it is prudent to regenerate vaccine matter as frequently as possible, to preserve its protective power.

6th. The only mode of regenerating vaccine matter deserving of confidence is to procure it from the cow.

7th. Revaccination is the only known method of distinguishing those vaccinated persons that remain fully protected from those that do not.

8th. The success of revaccination is not a certain proof that the person in whom it succeeds was liable to contract small-pox; it merely establishes a tolerably strong presumption that they were more or less liable to the variolous infection.

9th. In ordinary periods, revaccination should be practised after fourteen years, but sooner, as already remarked, during an epidemic.

See also, on this subject, Condie on Diseases of Children, 4th. Ed. — C.]

To avoid breaking the thread which connects the different parts of the main subject, I have postponed to the last what I have to say respecting the *treatment* of small-pox.

This, for a long time, was conducted upon an erroneous principle, and was eminently disastrous. The older physicians attempted to force out, through the skin, the morbid matter existing in the blood. The eruption they considered to be the natural and only cure: and adopting the vulgar maxim, that "it was better out than in," they did all they could to promote a copious eruption, by a hot regimen, by covering the patient with bed-clothes, by keeping the doors and windows jealously closed, and

excluding every breath of fresh air, and sometimes by administering wine and cordials. The celebrated John of Gaddesden, the author of that curious book the *Rosa Anglica*, improved even upon this. He surrounded the half-suffocated patient with red curtains, red walls, red furniture of all kinds; every thing he saw was to be red; for in that colour there was, John pretended, a peculiar virtue. This John of Gaddesden, by the way, was a very sad knave, and the first Englishman, I believe, who had the luck to be made Court physician. He had one medicine so good as to be fit for the rich only; and he recommended a double dose for the wealthy. "Duplum sit, si pro divite." He flourished in the fourteenth century.

Sydenham was the first, in this country, to employ the opposite or cool regimen in small-pox; and although his prejudiced contemporaries refused to follow his example and adopt his practice, he confidently predicted its final triumph—"obtinebit demum me vitâ functo."

But it was subsequently to the introduction of the method of inoculation that the cooling treatment was fairly established by the Suttons — two brothers, one of whom, Robert, lived at Bury St. Edmunds; the other, Daniel, at Ingatestone, in Essex. These men, wiser in their generation than the regular physicians, had the good sense to pursue the same plan of general management which had been so prosperous in the East, whence the practice of engrafting was originally imported. Daniel, in particular, became famous for his successful inoculations: and the great secret of his success seems to have consisted in his making one puncture only; exposing his patients much and often to a cool atmosphere; supplying them freely with refrigerant drinks; and restricting them to a spare diet. Under this course, Cullen, who adopted it from the Suttons, declares that ninety-nine times in the hundred, inoculation imparts a distinct small-pox, and very generally of the mildest form.

Now the same principle applies to the casual disease when we have reason to suspect that it is impending, or have the opportunity of treating it at its commencement. The object is to prevent, if possible, a copious eruption; upon which, as we have seen, the severity and peril of the disorder entirely depend. It has been thought that venæsection, by its antiphlogistic power, and, perhaps, by letting out, with the blood, some portion of the regenerated virus, might lessen the number of the forthcoming pustules. But you cannot ensure this effect by blood-letting: and you must bear in mind that, should the eruption prove confluent, suppuration, to a large amount, is inevitable, and — like that of an extensive burn — will require, in order to go on favourably, a certain degree of constitutional vigour.

You may abate the force of the eruptive fever, and keep down, it is believed, the number of pustules, by saline purgatives, so exhibited as to produce two or three loose stools every day, and by free ventilation of the surface of the body. The skin may even be sponged with tepid water, if the temperature be very high.

When the eruption is all come out, if the pimples on the face are very few and distinct, the danger is over, and there is no more to be done. At this period Cullen dissuades the further use of purgatives, as being sometimes hurtful.

But if the pimples on the face are many, and confluent, the patient will still require a great deal of attention. Our business is to look out for, and to meet, untoward symptoms.

About the eighth or ninth day, wakefulness, and restlessness, and sometimes tremors, are apt to come on; and the proper remedies for this set of symptoms, in small-pox as well as in continued fever, are opiates. In variola, when given in full doses at bedtime, their good effects are often very conspicuous the next day.

If the maturation of the pustules should proceed tardily, if they should not fill up properly nor their contents become purulent, then strong broths may be of use, or even wine. But the effects of these must be carefully watched, and their amount adjusted to the necessities of the case.

When the pustules are livid, and intermixed with petechiæ, and putrid symptoms occur, the disorder generally proves fatal. In such cases it is customary to prescribe bark and acids, in addition to the wine and opiates.

The proper plan of managing the patient during the continuance of the secondary fever, is to keep his bowels moderately open by gentle laxatives, or by enemata; and to give opiates once or twice a day. These are the more necessary on account of the irritation of the skin. The cooling regimen must now be given up; and the strength

must be supported by a nourishing diet. Wine and cordials are indicated if the pulse be feeble; but the swelling of the hands and wrists often makes it difficult to feel the pulse.

Various external applications have been tried, with the view of relieving the intolerable itching; which often induces the patients to scratch and tear their faces, and to insure the formation of scars. Cold cream is used for this purpose; or a solution of common salt, applied luke-warm; or a lotion made by mixing a drachm of the *liquor sodæ chlorinatæ* with half a pint of water; or a liniment composed of equal parts of olive oil and lime-water. This may be smeared, from time to time, over the itching surface, by means of a soft camel's hair brush.

Other methods have been devised, having a more direct aim towards the prevention of that pitting or seaming of the face which is only less dreaded by many patients than the threatened extinction of life itself. Finely-powdered camphor, dusted over the surface, is believed by Mr. George to obviate the disfigurement. Mr. Startin produces a little spot of vesication by touching the apex of each pustule on the exposed surfaces of the body with the *acetum cantharidis*, by the help of a camel's hair pencil. Mr. Higginbottom touches each distinct papula with the solid stick of lunar caustic previously moistened; but when the spots are confluent he washes the whole face, about the third day of the eruption, with a very strong solution of the nitrate of silver, using eight scruples to the ounce of water. A similar practice has been adopted, independently it would seem, by Dr. Alexander Howard of Quebec, except that his solution is somewhat less strong, an ounce of water containing a drachm only of the salt. This application, which is repeated from time to time, is said to be not only effectual for its primary purpose, but to be grateful also to the feelings, and even conducive to the safety of the patient; to allay the heat, itching, and tension of the face and scalp, and to abate the cutaneous inflammation. It is said also to give no pain, and to leave the features perfectly free from pitting; while it has the further recommendation of requiring no great skill or care in its use. Its management may be entrusted to a nurse.

The dyspnœa which sometimes comes on late in the disease, is a very ugly symptom. I know of nothing that can be done for it beyond blistering the throat and chest.

LECTURE LXXXVIII.

Chicken-Pox. Measles. Scarlet Fever.

I MUST not omit a short notice of the disorder called *chicken-pox*; for although a very unimportant complaint, it has given rise to many disputes. Other names which it has borne are *varicella*, *crystalli*, *variolæ pusillæ*.

Connected with the small-pox, and arising from the same contagion, there are several forms of eruptive disease. I mentioned the chief of them in the last lecture, as varieties of *modified* small-pox. Now these mild and irregular forms of variola, both parents and medical men, wishing, I suppose, to believe nothing in disparagement of the protecting power of vaccination, are very apt to consider, and to call, chicken-pox; and this error having been discovered, some persons have rushed to, or rather revived, the opposite opinion—equally erroneous in my judgment—that there is no such substantial disorder as chicken-pox; but that all the eruptions which have passed under that name have really been forms of modified small-pox. Dr. John Thompson of Edinburgh, was one of the stoutest maintainers of this doctrine. No doubt an eruption of short duration, and vesicular through the greater part of its progress, is often caused, especially in persons who have been vaccinated, by the contagion of small-pox; but a similar eruption proceeds also from another distinct contagion, that, namely, of chicken-pox.

The best description of the true chicken-pox that I am acquainted with has been given by Dr. Gregory. The disorder is almost peculiar to infants, and children of tender years. Willan has, however, described one unambiguous example of it, in a gentleman thirty years old: and another genuine instance was seen by Dr. Gregory, at the Small-pox Hospital, in the person of an adult female. The eruption is preceded by little or no premonitory fever, commencing usually on the shoulders, neck, and breast, affecting almost always the scalp, but sparing very much the face—which, in small-pox, never escapes.

The eruption is composed from the very first, of perfectly transparent vesicles, surrounded by a very slight degree of superficial redness. They are usually numerous, but distinct. Dr. Gregory says that when the eruption is very copious, the body has the appearance of having been exposed to a momentary shower of boiling water, each drop of which had occasioned a minute blister. Crops of vesicles appear in succession for two or three days; and while new ones are forming, the first are beginning to shrivel. The vesicles that remain after the second or third day become slightly opaque, and like pearls. When irritated by friction, they sometimes take on so much inflammation as to be converted into pustules. The scabs are small and gummy, dry quickly, and crumble off, instead of being detached in one mass. In a few instances, shallow cicatrices are left by the vesicles. During the short progress of this eruptive disease there is no constitutional disturbance of any consequence.

It has been ascertained of this genuine chicken-pox, or *varicella lymphatica*, that it occurs once only to the same person; that it spreads by contagion; that, nevertheless, it is not communicable by inoculation—whereas the matter of modified small-pox, when engrafted, produces genuine variola; that it occurs equally among those who have, and those who have not, been vaccinated; that its course is not affected by antecedent vaccination; and that the vaccine vesicle and disease proceed with perfect regularity after the occurrence of chicken-pox. Now this does not happen after small-pox.

It appears, from Möhl's work, *De Varioloidibus et Varicellis*, that from the year 1809 to 1823, chicken-pox was annually observed at Copenhagen without concomitant small-pox; and that both diseases have since prevailed at intervals epidemically, but always under circumstances which satisfied the physicians of the town that their sources were distinct.

It must, therefore, I think, be admitted, that there is a separate disease, called chicken-pox, which springs from a specific poison; produces a vesicular eruption; runs a definite course; has no tendency, when undisturbed, to suppuration; occurs but once; and affords no protection against small-pox, while, on the other hand, small-pox affords no protection against it.

The main point of practical importance is, however, this; that if we meet with any eruption which is at all equivocal, we should use the same precautionary measures for preventing the extension of the disease as if we were sure that it was modified small-pox. But this salutary rule is often, I say, neglected or infringed, to the danger and detriment of those unprotected persons who happen to be in the vicinity of the sick child.

The treatment required in chicken-pox is abundantly simple; it is the same, in fact, which has been already recommended for the mildest cases of the discrete small-pox.

Another of these blood diseases is *the measles*: called, also, by nosologists, *rubeola*, and *morbilli*.

Like different human faces, all the complaints belonging to this group have the same set of features, and therefore a mutual resemblance, while the separate lineaments differ so much in their character and relative circumstances, as to give to each disease its distinctive aspect. There are also minor shades of difference between individual cases of the same specific malady.

Measles, accordingly, has its stage of incubation, its introductory fever, its period of eruption, its peculiar kind of eruption, its course by stages. It is communicable from person to person, and it generally occurs but once to the same person. On some of these points I spoke before.

The introductory fever is sometimes severe, and nearer in its type to synocha than to typhus. Like all fevers, it begins with lassitude, and shivering, which are soon

followed by heat of skin, acceleration of the pulse, anorexia, and thirst. But the peculiarity in the fever which precedes the eruption of measles is, that it is very constantly attended with an inflammatory condition of the mucous membranes; especially of those which are proper to the air-passages. The eyes become vascular and watery, the eyelids, heavy, turgid, and red. The membrane which lines the nasal cavities, the fauces, the larynx, trachea, and bronchial tubes, is affected. Hence we have, generally, as symptoms, much sneezing, as well as lachrymation, a copious defluxion from the nostrils, soreness of the throat, and an obvious redness of the fauces, and most commonly a dry, hoarse, peculiar cough. In short, the symptoms which usher in an attack of measles are the symptoms of coryza and catarrh. In some instances there is diarrhoea also, indicating a simultaneous affection of the mucous membrane of the intestines; and not unfrequently vomiting: but the vomiting, as in small-pox, ceases upon the coming out of the eruption.

The regular period for the appearance of the eruption is the fourth day of the disease; seldom earlier, frequently later: sometimes as late as the eighth or tenth day from the commencement of the catarrh. The eruption itself is a rash, consisting, at first, of minute papulæ, which, as they multiply, coalesce into blotches that have, more or less, a horse-shoe or crescentic shape, and leave the intermediate portions of skin of their natural colour. It is two or three days in coming out, beginning, on the face, neck, and arms, then reaching the trunk of the body, and so travelling down to the lower extremities. In this course it resembles the eruption of small-pox. It fades in the same order, standing out three days at least on the face before it begins to decline; so that its whole duration comprises a space of six or seven days. It becomes browner as it fades. You may feel that it is slightly elevated above the general surface of the skin, especially upon the face, which is somewhat bloated and swollen. The parts which the rash has recently occupied are left covered with a dry, small scurf. The cuticle does not peel off in large flakes, as I shall have to tell you that it oftentimes does in scarlet fever, but a great part of it crumbles away in a fine branny powder. Occasionally, yet very seldom I believe, the rash is intermixed with a few small and short-lived vesicles.

This termination of the papulæ is very unlike what happens in variola: and connected with the eruption there are two other important particulars in which the measles differs essentially from the small-pox. In the first place, the fever does not cease, nor even abate, upon the emergence of the eruption; but sometimes increases in intensity. And in the second place, the disorder is not more severe, nor more dangerous, because the eruption is plentiful, or early. So far from it, indeed, that in some of the worst and most perilous cases the eruption is apt to be partial, and to appear late and irregularly.

The eruption is the *distinguishing* feature of measles, but the catarrhal affection is, in every way, the *most important*. Indeed the rash may, and sometimes does, happen without the fever and the catarrh; and nosologists recognize a variety of the disorder under the title of *rubeola sine catarrho*. But it is observed of this variety, that it confers no protection whatever against the recurrence of the malady: in truth, it is most commonly succeeded in a few days by an attack of measles in its regular and complete form.

I need not stop to repeat what I told you in a former lecture about the other general features of this eruptive complaint. The period of incubation is from ten days to a fortnight. The contagion is active enough, though certainly it is less strong and diffusive than that of small-pox. When once introduced into a family or school, the disease rapidly spreads to those individuals who have not already had it. It is capable, though with much less readiness and certainty than small-pox, of being propagated by inoculation; but as the disorder is not rendered milder by being so introduced into the system, this process has no utility or interest, and is never resorted to. Occasionally rubeola visits the same individual twice; but this is an exception to the general rule. Perhaps, in some reputed instances of its recurrence, the first accession may have been without fever and catarrh, and therefore an ineffectual safeguard for the future. I myself know, however, two large families in which most of the children have suffered a repetition of the genuine unmitigated disease.

The measles resembles the other diseases of the group in this also, that at times it pervades a community as an epidemic; at times occurs here and there only, sporadically.

cally. The general character of the symptoms varies considerably in different epidemics. Morton and Sydenham, and after them Sir William Watson, have described visitations of what they call *putrid* measles. Sir William Watson was physician to the Foundling Hospital, and he witnessed two epidemics of this putrid kind among the children in that institution. He states that the eruption appeared unusually early, so early as the second day of the disease; and that, besides cough and dyspnoea, the complaint was marked by extreme debility, and attended with dysenteric diarrhoea. More seemed to die of the intestinal affection than of the pectoral. He lost, in one of these epidemics, nineteen out of one hundred and eighty-three patients. The malignant character of the disorder was manifested by the frequent occurrence of gangrene, both externally and internally. In this low form of measles, the rash is often irregularly and imperfectly developed, and of a livid colour.

Sydenham found that measles of an unusually bad kind prevailed in London in the years 1670 and 1674; the very same years in which small-pox was also remarkably malignant and fatal. This illustrates what I have stated before; viz., that the putrescent tendencies of these and other febrile disorders depend less upon any peculiar virulence in their *exciting* causes, than upon some change previously effected in the human body by the silent and gradual influence of certain *predisposing* causes.

The diagnosis of measles is seldom difficult. In the outset of the fever you may guess what is coming by the coryza, catarrh, and hoarse cough; especially if the disease be about. On the very first day of the eruption, the small, red, and hitherto separate spots are very like the incipient pimples of small-pox. Do not, therefore, at this period, express too confidently your opinion respecting the nature of the complaint. Parents and nurses might be charitable enough to attribute your mistake to inexperience or ignorance. The progress of the disease will soon remove all doubt. The eruption of small-pox presently exhibits some fluid, while that of measles has none—unless, indeed (what is uncommon), a few miliary vesicles mix themselves with it. But these make no advance in twenty-four hours. Ordinarily the isolated pimples visible upon the first day soon augment in number, and collect themselves into semicircular groups; and if any question at all arise, it is whether the disease be measles or scarlet fever. I shall presently describe the latter disorder; and then I will point out the marks of distinction between the two.

The prognosis in measles is governed chiefly by the mildness or the severity of the pectoral symptoms. The most common cause of death, in the fatal cases, is inflammation of some one or more of the textures that compose the lungs. And even when this immediate danger has passed by, the disease too often leaves chronic pulmonary mischief behind it. In scrofulous children, and young persons, it frequently awakens the slumbering germs of consumption. And when that specific effect is not produced, it is apt, in adults, to inflict upon the constitution a blow which is never thoroughly recovered from; the patient becoming, from that time forwards, delicate and valetudinary. The prognosis is always unfavourable when the eruption does not stand out well, is of a livid colour, and accompanied with putrid symptoms, or with a disposition to gangrene.

We augur favourably of the case when the thoracic symptoms are not severe; when the fever moderates upon the coming out of the rash; and when the rash is steadily persistent, and there is no excessive prostration of the strength.

Being contagious, and occurring for the most part but once, measles is principally seen in children, although no period of life is exempt from its attacks. In many children the disorder is so slight as to require little more than judicious domestic attentions. The free application of cold air to the surface, which is so beneficial in small-pox, would in measles be unsafe, on account of the pectoral symptoms. For this reason the patient should be kept in bed; with no more clothes, however, or warmth of the apartment, than he is accustomed to in health. The antiphlogistic regimen must be adopted; and when the bowels are not quite open naturally, gentle laxatives should be given. It may be well, also, to prescribe some diaphoretic medicine; a draught, for example, containing two or three drachms of the *liquor ammoniæ acetatis*, with half a drachm of the *spiritus ætheris nitrici*, and an ounce of camphor julep, to be taken three or four times in the twenty-four hours.

The most important part, however, of the treatment relates to the remedies to be

employed for the pulmonary symptoms, which in the outset depend, almost always, upon bronchitis. But the inflammation is apt, in severe cases, to spread insidiously from the mucous to the other tissues — the bronchitis becomes pneumonia — and we find, after death, some portions of the lungs hepatized; usually small portions. For the most part, however, it is extensive inflammation of the bronchial mucous membrane that we have to dread. And really I cannot give you any better or fuller directions with respect to the management of these inflammatory affections, than I endeavoured to lay down when I was speaking of *bronchitis* and *pneumonia*, as they occur idiopathically. You will judge of the extent and severity of the inflammation, partly by the common symptoms, partly by the help of your ear; and you must apportion your remedies to that intensity, so judged of. You will take blood by leeches from the chest, apply a mustard poultice or a blister, and give tartar emetic. And it is of importance that whatever kind or amount of depletion is adopted, should be resorted to *early*.

When the rash is about to decline, a spontaneous diarrhœa often sets in, and appears to have a beneficial effect in abating the febrile symptoms. If this natural curative process should fail to occur, it may be imitated by the exhibition of gentle aperients.

In weakly children blisters are apt to cause troublesome sores; and in some epidemics of measles, the sores thus produced show a disposition to become gangrenous. When any such tendency is noticed, blisters had better be avoided altogether. At other times, the inconvenience to be apprehended from a blister may be prevented by one of two plans; either by interposing a piece of silver paper between the blistering plaster and the skin; or by suffering the blister to remain upon the part three or four hours only, then taking it off, and applying a poultice. The cuticle will rise under the poultice, and the sore will not, in general, be a troublesome one.

If the eruption disappear prematurely, it may sometimes be restored by putting the patient into a warm bath. And if he be at the same time, in a low state, especially if what I have called putrid symptoms threaten or show themselves, you must treat the case upon that indication, just as you would in continued fever; giving wine and animal broths, and watching the effects of these, and apportioning their quantity accordingly.

It is of considerable importance to protect the patient from danger *after* the disease has subsided; by warm clothing, by preventing him from going out of doors too early, or being in any way exposed to cold. Pneumonic inflammation, and dysenteric purging, are frequent consequences of the want of prudence in this respect.

In the year 1846, an epidemic of measles spread itself through the group of small islands between Shetland and Iceland, called the Ferœ Islands. The disease was so serious and general, that the Danish Government thought it necessary to send two physicians from Copenhagen, Dr. Manicus and Dr. Panum, to the relief of the Islanders. You may read a short but interesting report upon this epidemic, by Dr. Panum, in the *Archives Générales de Médecine* for April, 1851. It so well illustrates several of the points on which I have been speaking, that I am tempted to extract some of its statements.

In these islands — which are separated from each other by narrow but dangerous channels, and which are debarred from much intercourse with the world, both by their geographical position, and by their having no external commerce — measles had been totally unknown from the year 1781. The disorder was brought to them in 1846, by a man who left Copenhagen on the 20th of March, arrived at the Island of Thorshavn, apparently well, on the 28th, and sickened on the 1st of April. In October, the disease had again disappeared from the islands. During that interval of about six months, of 7782 inhabitants of the seventeen islands, 6000 underwent the disease.

You will notice here the entire exemption from this contagious complaint for sixty-five years, and its immediate and rapid diffusion upon the introduction of the contagion.

In our own island we see the measles chiefly among children and young persons. There it affected persons of every age. In a village containing one hundred dwellers, eighty were laid up with it at the same time.

All the old people who had had the complaint in the epidemic of 1781, escaped it in 1846.

This shows two things — 1st. That subsequent immunity from the disease is the rule. This rule was not broken in a single instance. 2ndly. That the protection afforded by one attack does not wear out as life advances.

Again, of the older persons who had been alive in 1781, and had not been exposed to the contagion (and Dr. Panum could reckon one hundred such persons), all took the disease in 1846; whereas a few youths, though they mixed with the sick, were untouched by it.

From this we learn that the susceptibility of the disorder does not decrease as the age increases; and that the reason why it is so seldom witnessed in adults in this country is the same which I before assigned. The great majority have had it during early life, and are therefore incapable of taking it later.

Dr. Panum found — after careful and extensive observation, conducted under circumstances of unusual freedom from the fallacies and uncertainties which beset such inquiries in larger and more complex communities — that a period of thirteen or fourteen days intervened very regularly between the time of exposure to the contagion, and the time of the eruption of the characteristic rash. Take a single example of this from among many.

One of the smaller islands, called, however, Le Grand Dimon, contained eighteen persons only, who all belonged to the same family. A boat manned by a few of them made a voyage to Tveraa, where the disease was rife, and returned after staying there some hours. Up to the tenth day from that time all these voyagers seemed perfectly well. On the 14th day the eruption appeared upon them all; and fourteen days after that, it showed itself with the same regularity in all the other members of the family.

He noticed that the precursory symptoms were of uncertain duration; sometimes they occupied six or eight days, sometimes from four to six, but generally from two to four days.

The disorder proved very catching at the outset of the eruption and during its whole continuance. Dr. Panum holds it to be doubtful whether it is contagious during the preceding stage of catarrh, or the subsequent stage of desquamation.

Isolation was the only sure defence against the disease. Dr. Panum thinks that 1500 persons escaped it, by establishing regulations equivalent to those of quarantine.

I proceed, in the next place, to the consideration of *scarlet fever*.

This also is a contagious febrile disease, attended almost always, during a part of its course, by a rash, and by *sore throat*. It seldom comes on a second time.

There are some distinct varieties of this disorder, concerning which it is necessary that I should say a few words.

The two striking and important features of the disease are the *affection of the throat* and the *affection of the skin*. They may both be well marked; or only one of them may be well marked: and this circumstance has led nosologists to divide one and the same complaint into two independent maladies; to which Cullen and others have assigned the respective names of *cynanche maligna*, and *scarlatina*. When, in an earlier part of the course, I was treating of the diseases of the throat, I purposely omitted the *cynanche maligna*; because that is only another name for a particular form of scarlet fever. If you look to Cullen's definitions of these complaints, you will see how very much alike they are. They both specify inflammation of the fauces, a cutaneous rash, and fever. But in the definition of *scarlatina*, the rash is dwelt upon and described, and the fever is called *synocha*; while in that of *cynanche maligna*, the ulceration of the throat is more insisted on, and the fever is said to resemble typhus. The truth is, that these two kinds of disorder are both caused by the same contagious poison. The malignant sore throat may be caught from a patient who has mild scarlet fever; and mild scarlet fever may, in like manner, be contracted from one who is suffering under the malignant sore throat. The two forms graduate insensibly, in different cases, towards each other; and it would be impossible, even if it were desirable, to draw any strict line of separation between them. Many would say, and probably with truth, that their difference is this; in the one form the

poison of the disorder is seeking its vent principally by the throat, in the other by the skin.

For convenience, however, of description, and for the better direction of the treatment, authors generally make three *varieties* of scarlatina. *Scarlatina simplex*, in which there is a florid rash, and little or no affection of the throat; *scarlatina anginosa*, in which both the skin and the throat are decidedly implicated; and *scarlatina maligna*, in which the stress of the disease falls upon the throat. The epithet *maligna* marks truly the fearful character of this form of the malady.

To these three varieties, Dr. Copland has added a fourth, which he names *scarlatina latens*. This addition is warranted by the fact (certified now by the testimony of several observers) of the manifestation of certain well-known and remarkable sequelæ of scarlet fever, in persons who had been living with others sick of that disease, but in whom its primary and diagnostic symptoms had not occurred, or had occurred in so slight a degree as to escape notice.

I need scarcely remind you of a sort of mystification which prevails among the public about this complaint, and which many practitioners, for no good reason that I can perceive, seem disposed to encourage. Mistaking the Latin and scientific name of the disorder for a mere *diminutive*, you will hear mamma say, "Oh, my children have not got the *scarlet fever*, but only the *scarlatina*." I always disabuse them of this absurd error, when the opportunity of doing so occurs. It can produce nothing but confusion, and the disregard of requisite precautions.

Like measles, and for the same reasons, scarlet fever, though persons of all ages are susceptible of it, is eminently a disease of children; but it is much more to be dreaded than the measles.

It is somewhat strange that scarlet fever was not recognized, in this country at least, as a distinct disease, till about two centuries ago. In all probability it had long existed, and had been always confounded with measles. Morton speaks of it under the name of *morbilli confluentes*; and Hoffman calls it, by a similar mistake, *rubeola rossalia*. The febris scarlatina described by Sydenham, must have been of a very mild kind; for he does not mention any ulceration of the throat. Dr. Fothergill, in 1748, was the first to describe, as a new and separate disorder, that perilous form of the complaint which Cullen designates *cynanche maligna*; and it was long called the Fothergill sore-throat. The identity of this affection with genuine scarlet fever has been slowly established by subsequent observers. The characteristic differences between scarlet fever and measles were first fully specified by Dr. Withering.

The disease begins, as the exanthemata in general begin, and as continued fevers which I have grouped with them are apt to begin, with shivering; lassitude, and rapidly augmenting debility; headache, frequently severe, sometimes with delirium, occasionally with nausea and vomiting. Then, generally on the second day (and Cullen is wrong when he says it is generally on the fourth), the eruption begins to come out. In some of the worst forms of the disease it may, indeed, be deferred till the fourth day.

Although scarlet fever and measles were so long confounded together, the differences between them are well pronounced, and, when once pointed out, are easily enough recognized.

Rubeola is distinguishable, then, from scarlatina—

1. By the presence, at the outset, of catarrhal symptoms—by the sneezing, the cough, the defluxion from the eyes and nose, which precede the rash. There is, doubtless, in many cases of scarlatina, a running from the eyes and nose, but not till late in the disease; at any rate not prior to the eruption.

2. By the absence of severe inflammation and ulceration of the throat; symptoms which always accompany severe cases, at least, of scarlet fever.

3. By the characters of the eruption itself. The rash in measles is more elevated above the surface than in scarlatina, and of a darker colour. In measles it is said to present somewhat the tint of a raspberry, and in scarlet fever to have that of a boiled lobster. In measles the papulæ are collected into semi-lunar groups, leaving interstices between them of healthy skin. The redness of scarlatina commences in minute points, which speedily become so numerous and crowded, that the surface appears to be universally red. They begin on the face, neck, and breast, and extend to the extremities, pervading at last every part of the skin. The scarlet colour is deeper, in

general, about the groins, and in the flexures of the joints, than elsewhere. Lastly, the rash of measles, in its most regular form, appears on the fourth day of the disease; that of scarlet fever on the second.

On the arms and legs the eruption of scarlatina occasionally differs somewhat from that which is visible on the trunk; is more spotty, more papular, and the papulæ are somewhat prominent, while over the body there is a general punctuated blush.

In some cases of scarlet fever (probably in some epidemics, for I observed the phenomena I am about to mention in four or five cases in succession which were brought into the Middlesex Hospital within the space of a month or six weeks), some parts of the red surface are closely studded with little transparent vesicles, containing a thin colourless liquid, and resembling what I described to you before as *sudamina*. In all the instances in which I have seen them, these minute vesicles have been most thickly set on the thorax, and on the front and sides of the neck. The liquid is soon re-absorbed, and the cuticle under which it had been enclosed shrivels up, turns white, and comes off in a thick white scurf: so that the part from which it separates looks at first sight as if it had been powdered. I have recently seen two cases of this vesicular form of scarlatina in private practice. I show you Rayer's delineation of the vesicles.

The eruption, in the most regular and favourable cases, stands out for three or four days, and then begins to fade and decline, becoming by degrees indistinct, and disappearing altogether, in the majority of instances, before the end of the seventh day. About this time desquamation of the cuticle begins to take place, in smaller scurf or scales from the face and body, in large flakes frequently from the extremities. The scarf-skin of the hands and of the feet sometimes separates almost entire. A glove or a slipper of cuticle comes away at once. You may see such things in most museums.

In that variety of the disorder which we call scarlatina *maligna*, the rash is apt to come out late, and imperfectly, and sometimes not at all; and instead of being bright and florid, to present a bluish or livid tint. Sometimes it suddenly recedes; and then, perhaps, appears again: and occasionally it is diversified by purple spots.

Willan and Bateman have given the name of *roseola* to an eruption which is also attended with inflammation of the throat, and between which and scarlatina it is certainly difficult, if not impossible, at first to discriminate. The roseola, however, is not contagious, and has more of a chronic character than scarlatina. It comes and goes, and has no settled or definite course. Dr. A. T. Thomson lays down *this* distinction between them; but I do not know that we can trust to it:—"In scarlatina (he says) the rash first attacks the face, and then extends to the trunk of the body, passing off by the extremities; whereas in roseola the extremities are *first* affected."

The appearances of the *tongue* in scarlet fever are also peculiar and characteristic. In the scarlatina *simplex*, and *anginosa*, it is often covered, at the outset, with a thick, white, cream-like fur, through which are seen projecting the red and exaggerated papillæ; the edges of the tongue being likewise of a bright red colour. The red points gradually multiply, and the white fur clears away, and at length the whole surface of the tongue becomes preternaturally red, and clean, and raw-looking: and after becoming thus clean, as well as red and rough, and like a strawberry, it will sometimes, when the disease goes on unpromisingly, get dry, and hard, and brown—as you know it is apt to be in certain species and stages of continued fever.

The first thing of which the feverish patient usually complains is sore throat, with some stiffness of the neck: and if you inspect the fauces, you will see, without in general so much swelling of the tonsils as occurs in common quinsy, a diffused redness, sometimes of a dark claret colour, including a large part of the palate. In a short time you may perceive that the tonsils and velum are covered irregularly with whitish exudations, or gray aphthous crusts: or, perhaps, you see a sloughy kind of ulceration left by the separation of these crusts.

The progress of the distemper, and its degree of severity and of danger, differ very greatly in different cases. Sometimes the deviation from the feelings and condition of health is so very slight as scarcely to deserve the name of a disease; sometimes the disorder defies all treatment, and the deadliest forms of plague are not more fatal.

In these malignant and terrible cases, the eruption, if it appear at all, is livid and partial, and fades early, and is attended with a feeble pulse, a cold skin, and extreme prostration of strength. Sometimes the patient sinks at once, and irretrievably, under

the virulence of the poison, and life is extinguished in a few hours. A gentleman called one day at my house, and not finding me there, followed me between twelve and one o'clock to the hospital. He wished me to visit his wife, four or five miles out of town, who had been taken ill that morning. He feared that she was about to have scarlet fever, but he was not much alarmed for her safety; for when he found that I could not be at his house before six, he said that that hour would not suit the general practitioner in attendance upon her, and he begged me to fix some time for seeing her *the next day*. I did so; but the same afternoon rapid sinking came on, and the patient was dead very soon after the hour at which I had first proposed to visit her.

In other cases of scarlatina maligna, the typhus-like symptoms rapidly deepen; and death, in children, is apt to occur on the fifth day of the complaint; and not uncommonly as soon as the third. The pulse becomes frequent and feeble; the tongue dry, brown, and tremulous; the debility extreme; the breath offensive; the throat is livid, swollen, ulcerated, and gangrenous; and the respiration is impeded by viscid mucus which collects about the fauces. Over this variety of the disease, medicine has comparatively little control.

The chance of recovery is much greater in the scarlatina anginosa, when the eruption is florid, and stands well out. But even in this form of the disorder there are many sources of danger, and various ways in which it may prove fatal.

In the first place many of the patients die, apparently from inflammation or effusion within the head. They have violent headache, with furious delirium, which is followed by coma and death.

And, secondly, the state of the throat is full of peril. As the disease proceeds, although the rash may be steadily persistent, the throat becomes foul and sloughy; an acrid discharge from the nostrils, which are so stuffed and swollen internally that the patient can scarcely breathe through them, runs over and frets the upper lip; the parotid and submaxillary glands swell, sometimes enormously; and fever is lighted up afresh. In this way many cases prove fatal in the second week of the disorder. The cervical swellings cause constriction of the fauces and stiffness of the neck; and sometimes, doubtless by interfering with the free return of the blood from the head through the jugular veins, they produce a tendency to coma. With these symptoms there is often also purging, and an excoriated anus.

The acrid matters furnished by the ulcerating and gangrenous throat irritate the nasal membrane in the one direction, and that of the alimentary canal in the other. We thus account for the running from the nose, the soreness of the alæ nasi and upper lip, and the smarting diarrhoea: and the swelling of the parotids and neighbouring glands is evidently caused by absorption of the irritating and poisonous matter from the ulcerated throat. There is just the same relation and dependency between these different local alterations, as between the enlarged mesenteric glands, and ulceration of the follicles of Peyer in typhoid fever; between a bubo in the groin, and a chancre on the glans penis. It is the condition of the throat that gives rise, in these cases, to the most formidable symptoms. The system is reinoculated from that source. Whenever I see the glands much enlarged at the angle of the jaw, and beneath the jaw, in a child labouring under scarlet fever, I augur ill of the case. Sometimes the mischief extends into the larynx, and so destroys the patient. But this is probably a very rare event. There is, however, still another, and a very common consequence of the throat affection—I mean inflammation of the eustachian tube, reaching sometimes the tympanum itself, and causing permanent deafness, either by closing up the tube, or by the destruction of the membrana tympani, and of the little bones belonging to it. In one case, which was under my own care, I observed that, for a short time before death, every time the child swallowed, a part of the fluid food ran out immediately at one of its ears. I had no opportunity of examining the state of the part after death, but the disorganization arising from the sloughing ulceration of the throat must have been frightful.

Scarlet fever sometimes befalls parturient women; and then it almost always proves fatal. I have seen three instances only of recovery from this perilous complication.

Scarlatina simplex is scarcely, I repeat, a disease. Sydenham has said of it that it is "fatal only through the officiousness of the Doctor."

Even when the patient has escaped from the complaint itself, he is often exposed

to great hazard and distress from its *consequences*. Children who have suffered a severe attack of scarlet fever are liable to fall into a state of permanent bad health, and to become a prey to some of the many chronic forms of scrofula: boils, strumous ulcers, diseases of the scalp, sores behind the ears, scrofulous swellings of the cervical glands and of the upper lip, chronic inflammation of the eyes and eyelids. The same afflicting results are very common after small-pox also, and measles.

I have several times, when the rash of scarlet fever was disappearing, known pain and swelling of the larger joints to supervene, simulating very closely the local phenomena of subacute rheumatism; and I have noticed that the painful joints were eased and benefited by friction; a circumstance which may help to distinguish this articular affection from true rheumatism. Another distinctive circumstance seemed to be that, although all these patients were children, the heart in no instance became implicated, in connexion with the tumid joints. Upon this point, however, my own experience may have been fallacious. Dr. Scott Alison has recently invited attention to the subject, in an interesting Essay "On Pericarditis, a complication and sequela of Scarlatina." Accepting his facts, I should ascribe the articular affection, and the cardiac affection, whether they occurred together or separately, to one and the same cause; namely to the retention in the blood of a poisonous excrement, by the default of the principal emunctories, and especially of the kidney.

But certainly the most common, and a very serious sequel of scarlatina, is *anasarca*, serous infiltration of the subcutaneous areolar tissue, accompanied often with dropsy of the larger serous cavities. So common is this that Cullen has even introduced the circumstance as a part of his definition of scarlet fever. He found the dropsy a very manageable complaint; but it really is, in many — nay, in most cases, if we look to its possible ultimate consequences — a most formidable one. This affection belongs to the class of *febrile dropsies*. It appears to have no relation, or, if any, an inverse relation, to the violence and danger of the preceding exanthem. It is much more common after a mild, than after a severe disease. This I believe to be chiefly owing to the circumstance that less care and caution are observed in the milder cases during the dangerous period of desquamation and convalescence; a period more dangerous, in that variety of scarlatina, than in any other. In the graver cases the convalescence is slower, and more doubtful; and accidental or careless exposure to cold is more guarded against, or takes place later: whereas, in the slighter kinds of the disorder, the patients are apt to go out while the new cuticle is still forming. The escape of the fever-poison through the large outlet afforded by the skin is checked or prevented. More of it is hurried through the narrower wicket of the kidneys, and gives rise, in its tumultuous and embarrassed outbreak, to what Dr. George Johnson has called "acute desquamative nephritis." If you carefully trace the histories of dropsy succeeding to scarlet fever, you will very frequently find that the fever had been trifling; and that the patient, considering himself well or nearly so, had heedlessly encountered a cold or damp atmosphere so soon as he felt himself strong enough to leave the sick chamber. Plenciz, who has written well on this subject, and who was quite aware of its importance, remarks that those patients who have had much desquamation of the cuticle are the most liable to the dropsy; that it is more frequent in winter than in summer; and in such as are early exposed to the open air after having passed through the fever, than in those who remain longer at home. When the desquamation is over, and the new surface has become in some degree hardened, the peril is past. According to the observations of Dr. Wells, the dropsical symptoms commonly show themselves on the twenty-second or twenty-third day after the commencement of the preceding fever. They have been known to begin as early as the sixteenth, and as late as the twenty-fifth day. When no dropsy took place before the end of the fourth week, Dr. Wells always ventured to state that it was no longer to be dreaded.¹

This anasarca is seldom observed except in children and young persons. The age of the oldest patient that Dr. Wells had known to be so affected was seventeen. Of ten instances of the disease seen by Dr. Blackall, six occurred in children not exceed-

¹ Dr. Tripe, in a paper contained in the *Medico-Chirurgical Review*, for July, 1854, assigns larger limits to the first appearance of the dropsical symptoms, and states that the fourteenth day of the disease is most frequently of all the day of their invasion.

ing the age of ten, and two others in persons who were respectively ten and sixteen years old.

We cannot infer, from this, that the susceptibility of this dropsical condition lessens as years increase. The great prevalence of this variety of dropsy in early life has no direct relation to age as a predisposing cause. The fact is explained by the accidental peculiarities of the antecedent disease. The contagion of scarlet fever is active and widely diffused. Few children escape its agency. Few are capable of taking the disorder a second time. It follows that scarlet fever is rare in adult life : and as dropsy succeeds that disease in a very limited number of instances only, dropsy arising in connexion with scarlet fever must, at the adult age, be still more uncommon.

Yet it is not unknown. One of Dr. Blackall's ten patients was thirty, another forty-two years old. Both of these were women.

In this, as in other species of febrile dropsy, the urine is very dark, olive-coloured, albuminous, and sometimes bloody ; and it contains fibrinous casts of the renal tubules, with epithelial cells intermixed.

Not only may one case of scarlet fever differ widely from another case, but very great differences are also observable in the general character of different epidemics. This is true indeed of all the exanthemata. In some epidemics the disease is almost uniformly mild, in some it is fearfully severe and dangerous. The inflammatory symptoms may in one epidemic run high, while throughout another the low or typhous type may predominate. We should learn from such differences not to dogmatize, in reliance upon our own experience of one or two epidemics, respecting the most fitting management of the disorder ; nor to criticise ungenerously and with ignorant arrogance the treatment recommended by others, who may have observed the disease elsewhere, or at some former time. These diversities are well illustrated in the histories given of scarlet fever, as it has prevailed epidemically in the charitable institutions for children which abound in and near Edinburgh. In such places the means of studying epidemic contagious disorders are singularly precious and instructive. The inmates are of nearly the same age, are all living under precisely similar circumstances as to food, clothing, shelter, and general habits, and are at the complete disposal, and under the frequent and close observation, of the attending physician. You may read in the *Edinburgh Monthly Journal of Medicine* some very interesting and valuable records of these visitations of scarlet fever from the pens of Dr. Gillespie, Dr. Newbigging, Dr. Andrew Wood, and Mr. Benjamin Bell. Much attention was paid by these gentlemen to the relations subsisting between scarlet fever, the presence of albumen in the urine, and the occurrence of dropsy. This subject has also been carefully investigated by Dr. Warburton Begbie, who had likewise large opportunities of witnessing the disorder in the Edinburgh Infirmary and elsewhere. I have much trust in his conclusions, which are briefly these :—

In most, if not in all cases of scarlet fever, the urine, at one period or another of the disease, contains more or less albumen. With a few exceptions, the time when it begins to appear is shortly after the commencement of desquamation of the cuticle. The albuminous condition is most often transient, and is by no means necessarily attended with anasarca. It lasts from a day or two to ten days, its average duration being four or five days. It may easily be overlooked if the urine be not very frequently examined. The amount of albumen is generally small. When once it disappears from the urine, it never reappears. The specific gravity of the urine remains high, and its quantity is usually plentiful. Here however comes in the connexion with dropsy. If the urine, thus albuminous, become scanty, then the supervention of anasarca may be looked for. While there is no anasarca, the urine may be albuminous, but it contains no casts. As soon as anasarca occurs, casts and epithelium, and sometimes even blood, are found associated with the albumen.

The majority of these cases of scarlatinal dropsy end in complete recovery. No permanent damage is sustained by the kidneys. Yet the instances are not few in which the chronic form of renal dropsy, manifesting itself at some distance of time, has been distinctly traced back to its source in the acute anasarca immediately consequent upon scarlet fever. There can be no doubt that one form at least of the organic renal degeneration described by Dr. Bright does not unfrequently date its origin from an attack of febrile anasarca : and in proportion as facts, accurately

observed, accumulate on this subject, the chain of connexion becomes more clearly visible between acute febrile dropsy, dropsy succeeding scarlet fever, and chronic renal dropsy. It is evident, indeed, that the first two of these three are, in their characters and exciting causes, nearly identical, the only difference between them consisting in the remarkable predisposition towards the second, impressed upon the body, through contamination of the blood, by the preceding exanthem. Both of them again are, in many instances, initiative of the third.

It is natural therefore to expect that, in the variety of febrile dropsy now under consideration, as well as in those which I formerly described, *inflammation*, and especially inflammation of the serous membranes, should be met with, and be evidenced by its unequivocal effects. And it is so. But the dropsy, I am persuaded, has no essential connexion with common inflammation of any part, unless the state of the kidney be of that kind. I have examined the body very carefully in fatal cases, and found the serous cavities full of clear liquid, without a trace of redness or of any of the unmistakeable products, or events, of inflammatory action.

The earliest threatenings of this formidable complaint demand attention. Its approach may often, I say, be detected, prior to any more obvious symptoms, by daily examination of the quality and the quantity of the urine. It is usually preceded for a day or two, or longer, by languor and peevishness; frequently by nausea and vomiting, and a costive state of the bowels. The pulse, in the outset, has been found slow, and beating with irregular intervals; but it afterwards grows frequent. The face becomes white and chuffy. Sometimes, as the disease proceeds, violent headache, drowsiness, dilatation of the pupils, convulsions, or palsy, denote effusion within the head. These symptoms may result however from the poisoned state of the blood. Much more frequently the pleuræ are the seat of the internal dropsical accumulation, and dyspnœa is a prominent symptom. Ascites, to any considerable amount, is rare.

The contagion of scarlet fever is active, but uncertain. It is not so strong, nor so uniform in its operation, as that of small-pox; but it seems to be peculiarly subtle and tenacious. Dr. Webster is of opinion that the risk of infection is diminished by frequent spongings of the patient's body with tepid vinegar and water. Fomites infected with the variolous poison soon lose their power to excite small-pox if they are freely exposed to fresh air. But the contagion of scarlet fever lurks about an apartment, or clings to furniture and clothes, for a very long time, even after some care has been taken to purify them. Of this I have known several remarkable examples. I will give you one. The disorder had attacked several persons in a large household. When it was fairly over, the house was left empty, and then (as was supposed) most thoroughly ventilated and purified. A year afterwards the family returned to the house. A drawer in one of the bed-rooms resisted for some time the attempts to pull it open. It was found that a strip of flannel had got between the drawer and its frame, and had made the drawer stick. This piece of flannel the house-maid put playfully round her neck. An old nurse who was present, recognising it as having been used for an application to the throat of one of the former subjects of scarlet fever, snatched it from her, and instantly burned it in the fire. The girl however soon sickened, and the disease ran a second time through the household, affecting those who had not had it on the first occasion. You will be asked at what period the danger of imparting the disease on the one hand, or of catching it on the other, is over; and I would recommend you to answer that you do not know. I am sure I do not: and therefore I always decline the responsibility of giving an oracular opinion on the matter.

I may arrange what I have to say of the *treatment* of scarlet fever, according to the three varieties of it already mentioned, the *scarlatina simplex* — *anginosa* — and *maligna*. Of the *scarlatina latens*, the sequelæ alone become the subjects of treatment.

The first of these requires nothing more than confinement to the house; and the observance of the antiphlogistic regimen in regard to diet; and regulation of the bowels.

With respect to the management of the severer forms of scarlet fever, great differences of opinion have prevailed. I should recommend you to look into Dr. R. Williams's book on *Morbid Poisons*, for some interesting and satisfactory information on

this head. Satisfactory to me at least it is, because the result of it goes to justify that kind of practice which I have always considered to be the safest and the best in this disorder.

In the scarlatina anginosa, the treatment I employ is very much the same as that which I consider proper for many cases of continued fever. If the heat of the surface be very great and distressing, I certainly should not recommend the cold *affusion*, but cold or tepid *sponging* will be very refreshing and beneficial. If delirium should come on, I would shave the scalp, and apply cold to it, and if the pulse were hard and strong, I would take away some blood by leeches: but I would apply the leeches behind the ears rather than to the *temples*. The tonsils, in this form of the disorder, are more swelled and inflamed, and probably a part of the head affection may arise from the disturbance of the balance of the cerebral circulation, produced by the tumefaction around the great veins that return the blood from the head. By leeching the mastoid processes you relieve, I think, both head and throat. If the fever were extreme and the delirium violent, I might take blood cautiously from the arm, while the patient was sitting up, and carefully note the result.

When none of these untoward head symptoms declare themselves, all that we have to do is to keep the bowels open by moderate laxatives. The patient may take saline draughts, which are grateful and cooling. The citrate of ammonia thus administered is what I frequently prescribe: and if the pulse be without hardness, and feeble, I order an excess of the carbonate of ammonia, so that four or five grains of it in each dose may remain unsaturated by the lemon-juice.

With respect, then, to this form of the complaint, the principles of treatment are, not to interfere unnecessarily; to take blood when certain symptoms require it, but to take no more than seems likely to be sufficient for the purpose in view; to bear in mind that the system is labouring under a morbid poison, which we cannot eliminate from the blood, but the dangerous effects of which we are to watch and obviate.

In that worst form of scarlet fever, the scarlatina maligna, all our care will too often be in vain. There appear to me two main sources of danger. The one arises from the primary impression of the contagious poison upon the body, and particularly upon the nervous system, which is overwhelmed by its influence. The patients sink often at a very early period, with but little affection either of the throat, or of the skin. If we can save such patients at all, it must be by the liberal administration of wine and bark, to sustain the flagging powers until the deadly agency of the poison has in some measure passed away. But another source of danger arises from the gangrenous ulceration which is apt to ensue in the fauces, when the patient is not killed by the first violence of the contagion. The system is *re-inoculated*, I believe, with the poisonous secretion from the throat. Now under these circumstances also, quina, or wine, and upon the whole I should give the preference to wine, are to be diligently, though watchfully given. And something may be done, by way of gargles, to correct the state of the throat, and to prevent the distressing and perilous consequences which would otherwise be likely to flow from it. A weak solution of the chloride of soda may be employed for this purpose; or, what I believe to be better still, a solution of the nitrate of silver. If the disease occur in a child that is not able to gargle, this solution may be injected into the nostrils, and against the fauces, by means of a syringe or elastic bottle: or a little mop, charged with the same solution, may generally be used without much difficulty. The effect of this application is sometimes most encouraging. A quantity of offensive sloughy matter is brought away; the acrid discharge is rendered harmless; the running from the nose, and the diarrhoea, cease; and the disease is converted into a form which approximates to the scarlatina anginosa. This is a great improvement upon the old plan of ordering capsicum gargles.

Of late I have been in the habit of directing a solution of the chlorate of potass in water (a drachm to a pint), as a *drink* for patients in scarlatina, as well as in typhus fever. This practice was suggested to me by Dr. Hunt, who tells me he has long employed it with advantage. Under the use of a pint, or pint and half, of this solution daily, I have remarked, in many instances, a speedy improvement of the tongue, which, from being furred, or brown and dry, has become cleaner, and moist.

From several distinct and highly respectable sources, *chlorine* itself has been strongly pressed upon my notice, as a most valuable remedy in the severest forms of

scarlet fever. My informants have stated, that whereas they formerly dreaded to be summoned to cases of that disease, they now, having had experience of the virtues of chlorine, felt no misgivings in undertaking its treatment. Since these representations were made to me, I have not had opportunities enough of trying this drug to enable me to speak confidently of its sanative power: but I shall certainly employ it in future. I presume that its disinfecting properties may, in part, account for the good it does. It probably deprives the foul secretions of their noxious quality.

In the fourth volume of the *Medical Gazette*, Messrs. Taynton and Williams, of Bromley, write in high praise of this remedy, and give a formula for its preparation.

You may make it, for extemporaneous use, in this way.

Put eight grains of the chlorate of potass into a pint bottle, and pour upon them one drachm of strong hydrochloric acid. Keep the mouth of the bottle closed until the violent action has ceased; then add an ounce of water, and shake the mixture well; then add another ounce of water, and again agitate well; and so on until the bottle is full. The chlorate should be pulverized; and in cold weather the bottle should first be warmed.

A table spoonful, or two, of this mixture, according to the age of the patient, may be given for a dose, frequently. An adult may take the whole pint in the day.

We must remember, in this, as well as in the other forms of the complaint, to pay attention to the state of the bowels, and by no means to allow them to remain costive.

I have seldom used blisters in this disease; but an experienced physician has told me that, when applied *early* to the neck and throat, they seem to render the affection of the fauces mild.

When the patient is at length convalescent, he will require careful watching till that period has gone by at which the dropsical symptoms are apt to appear. Very often it is by neglect or imprudence that these symptoms are brought on. The patient should be kept long in bed, and sedulously protected afterwards from all exposure to cold, wet, or fatigue; indeed he ought not to be permitted to go out of the house until the process of desquamation is fairly over; and I would not willingly let a patient go out till some little time *after* this. When dropsical symptoms *do* occur, if they be very slight, they may be removed in general by purgatives, and by digitalis. These patients are always pallid and exsanguine; for this reason, and still more because that remedy has been found actually serviceable in such cases, I would advise a combination of the muriated tincture of iron with the tincture of foxglove. Diaphoretic remedies, too, are beneficial; and in aid of them, or rather as one of the most efficient of them, the warm bath, which may be repeated every night. And inasmuch as we know that the kidneys are at least congested in all these cases, I would always take a small quantity of blood from their neighbourhood, by leeching or cupping the loins. Upon the same ground I should refrain from prescribing stimulating diuretics.

But if there be any indication of *inflammatory* disease of any other internal part, we must adopt more active measures. We have not, *now*, so much to contend with the depressing influence of the original poison, as to dread the consequences of acute inflammation; or of the sudden effusion of fluid, the mere presence and pressure of which may fatally oppress vital organs. We should have for our object to arrest the inflammation—or to promote the removal of the effused fluid—by blood-letting, and by the exhibition of purgative medicines, and of *mercury*. The worst case of this kind that I ever witnessed occurred in a boy of fifteen, the son of a tradesman in my neighbourhood. He had had scarlet fever, *mildly*, and had got well, or nearly well, of it, as he believed; and he went, one evening, into his father's stable, and staid there some time in the cold, during the period of desquamation. A day or two afterwards he began to have headache, and in a few hours more was seized with convulsions of one side of the body, coma, and at length hemiplegia; and his face and extremities became at the same time anasarcous. A considerable quantity of blood was taken from his arm, he was cupped on the temples, and took mercury, till in a short space of time he was profusely salivated. Under this treatment the coma and dropsy rapidly disappeared, and he presently recovered the use of his palsied limbs, and got quite well. I conclude that some effusion took place within the cranium, as well as into the subcutaneous areolar tissue. The plan of treatment followed in this case, modified according to particular circumstances, is that which I should

again pursue, and therefore is what I should recommend you to pursue, in similar emergencies.

You are probably aware that *belladonna* is believed by many to exert a preventive and protecting influence upon the body against the contagion of scarlet fever. Hahnemann, the author of the Homœopathic hypothesis (and thereby of much mischief to mankind), was the first to assert this. The notion was evidently suggested by that hypothesis; for *belladonna*, administered in small doses, sometimes produces a scarlet efflorescence on the skin, and certainly tends to cause dryness and redness of the fauces. This is but a poor foundation on which to rest its prophylactic power. To test that alleged power is not very easy. Other precautions are commonly employed at the same time; there are great natural differences in different individuals with respect to the susceptibility of the contagion of scarlet fever; the prepared extracts of *belladonna* are but seldom worthless and inert. The conservative property, however, of that vegetable has, in my opinion, been completely disproved by the trial of it made in George Watson's Hospital, in 1851, by Mr. Benjamin Bell. Scarlet fever having appeared within the building, *belladonna* was given to fifty-four healthy boys; at first in doses which caused dilatation of their pupils, and impaired vision. The drug therefore was not inert. After this plan had been in operation for a month — after full time allowed, therefore, for the development of the protecting influence of the *belladonna*, if it really possessed any — twenty-three of these fifty-four boys took the disease.

It is sometimes prescribed, by men who have little or no faith in its preventive virtues, in order to give confidence to those employed in nursing the sick, and comfort to anxious parents. But in proportion to the confidence thus produced will be the risk of the neglect of other and better safeguards. For my own part, totally discrediting the defensive power ascribed to the drug, abominating all shams, and believing that so poisonous a substance can scarcely be taken for some time together, even in small doses, without prejudice to the general health — I not only never propose it, but I think myself bound to state plainly my opinion of it, whenever its use is proposed to me.

[The question in relation to the prophylactic properties of the *belladonna* in this disease is a highly interesting one. The severe and fatal character of scarlet fever, particularly when it occurs as an epidemic, renders every means of prevention that can be depended upon with any degree of certainty, of vast importance. In proof of the power of the *belladonna* to guard those who have been placed under its influence from an attack of scarlatina, we have certainly some very strong and respectable testimony. In a paper published by Bayle, in 1830, it is stated that of 2027 individuals to whom the *belladonna* was administered, 1948 were preserved from scarlet fever, and 79 were attacked. Dusterberg reports, that all who were placed under the influence of the *belladonna* for the space of two weeks, were preserved from the scarlet fever. In order to ascertain the real value of the article, he purposely omitted to administer it to one child in each family, and this one alone, according to his report, was seized with the disease. He adds, however, that occasionally a child who had only been taking the remedy during three or four days, was attacked, but the fever was in such cases always mild, and often only manifested its presence by the occurrence of desquamation. Zeuch, physician to the Military Hospital for Children, in Tyrol, after 84 of the children were attacked with scarlet fever, was induced to try the prophylactic powers of *belladonna* on the remaining 61 children: with a single exception, all of these were preserved from the fever, although it prevailed all around them. Schenk, Berndt, Köhler, Meglin, De Lens, Massius, Bayle, Godelle of Soissons, and many other respectable practitioners, speak in equally high terms of the preservative properties of the *belladonna*. We have ourselves given the *belladonna* to a number of children, on several occasions when the scarlet fever was prevailing epidemically, with a view of testing its prophylactic powers; but although redness and dryness of the throat, and a diffuse scarlet efflorescence were produced by it, yet in the majority of cases, we never found it in any instance to exhibit the slightest influence in shielding those who took it from an attack of the disease, or in mitigating in any degree its severity. In one case, the efflorescence produced by the *belladonna* was kept up for forty-eight hours; in a week afterwards the child was attacked by the fever in its

most violent form, and died on the fourth day. Recently, however, Dr. Stievenart, of Valenciennes, has published the results of a very extensive trial of the belladonna, as a prophylactic during the prevalence of epidemic scarlatina: which results, if they are correctly stated, and all sources of error have been carefully guarded against in the performance of the experiments upon which they are founded, go very far to prove that the belladonna does actually possess the property of shielding the constitution from an attack of scarlet fever. An epidemic of the disease ravaged, during the winter of 1840-1, several villages in the neighborhood of Valenciennes, when Dr. Stievenart was induced to try the prophylactic properties of belladonna. The fatality of the epidemic was such that 30 had already died out of 96 attacked; consequently any means of prevention was a subject of vast importance. In a small village, out of 250 individuals, 200 took the belladonna, and were all preserved from the attack of scarlet fever. Of the fifty others, 14 were seized with the fever, and four of them died. At the village of Curgies, Dr. Stievenart administered the belladonna to the children at the public school, and allowed them to continue at their lessons, and have free communication with the other children of the village. All to whom the belladonna was administered escaped the scarlet fever, while a few who refused to take it were seized with the disease.

The belladonna was administered in two forms—in solution, or as a powder. Two grains of the recent alcoholic extract of belladonna were dissolved in an ounce of any aromatic infusion, and of this two drops were given to a child of one year old, daily, for nine or ten days: an additional drop being given for every additional year of age. The largest daily dose was, however, limited to twelve drops. When the belladonna was used in the form of powder, half a grain of the powdered root was mixed with a small quantity of sugar, and divided into ten doses. One of these was given—morning and evening—to children of from one to two years old; two powders, at the same periods, to those from three to five; three powders to those from six to nine; four to those from ten to fifteen, and five to adults. These small doses never produced the toxicological effects of belladonna; in fact, they scarcely exhibited any marked action. In five or six cases Dr. Stievenart observed a rash similar to that of measles; and, in a few other cases, headache, with dilatation of the pupils, dryness of the fauces, and a slight soreness of the throat, but which had no resemblance to that of *scarlatina anginosa*. In all the other cases no sensible or apparent effect resulted from the administration of the remedy. Dr. Stievenart, generally, continued the use of the belladonna for from nine to ten days; in some cases, it was given for fifteen days. He thinks this period sufficiently long to put the system under the influence of the preservative powers of the remedy: but recommends to resume its use if the epidemic returns or breaks out again with renewed violence. In an epidemic of scarlatina which occurred in South Carolina, Dr. Irwin made a very extensive trial of the prophylactic properties of belladonna. Of two hundred and fifty children who were placed under the influence of the drug, less than half a dozen had the disease, and that very mildly. In the families, the members of which were not placed under the influence of the belladonna, the disease occurred with scarcely an exception. Dr. McKee, in the extension of the same epidemic, made use of the belladonna, and derived from it the same protective influence. Dr. Rufus Hammond, of Indiana, also bears testimony in favour of the prophylactic powers of belladonna, based upon ample opportunities for observation.—C.]

LECTURE LXXXIX.

The Plague. Erysipelas. Erythema nodosum. Urticaria. Prurigo. Scabies.

OF that group of *contagious* exanthemata of which I undertook to give you some general account, two only remain to be noticed; viz. the plague and erysipelas.

Concerning one of these, the plague—as I have never seen, and hope never to see it; and as, with Cullen, I “think it unfit for a person who has never seen the disease

to attempt its particular history' — I shall not presume to offer you any observations in detail. It is a very malignant kind of contagious fever; prevailing, at certain times and places, epidemically; attended with a sort of eruption, namely with buboes and carbuncles; and not furnishing, apparently, any sure or permanent security against its future recurrence. In Sir J. Forbes' *Select Medical Bibliography* you will find a long list of works on the plague. I would second Dr. Cullen's recommendation, that you should consult those authors only who have themselves had personal experience of the disease. Among the publications that fall within this rule may be mentioned Dr. Russell's *History of the Plague* as he saw it in Aleppo; Sir James M'Grigor's *Medical Sketches*; Sir Arthur Brooke Faulkner's *Account of the Plague* which occurred at Malta in 1813; Desgenettes' *Histoire Médicale de l'Armée d'Orient*; and Assalini's description of the malady as he witnessed it when in attendance upon the French army in Egypt.

I proceed, therefore, to *erysipelas*. And I wish, in the first place, to fix and define that specific complaint of which *alone* I propose at present to speak. The term *erysipelas* has been employed by medical men in a very loose and vague manner. Any diffused redness and inflammation of the skin is apt to be set down as *erysipelas*; and hence we have disputes as to the distinction between *erysipelas* and *erythema*. But it would tend, in my humble judgment, to the formation of more settled opinions in respect to *erysipelas*, if the term were restricted to that disease in which the integuments of the *face* and *head* become diffusely inflamed. The phrase *erysipelatos inflammation* may properly enough be applied to other cases, similar to this in so far as the condition of the *skin* is concerned; but in what I should consider true *erysipelas*, in the medical sense of the word, there are other characters belonging to the disorder quite as important as, and more distinctive than, the cutaneous affection. What is usually spoken of as *erysipelas* of the face and head, and what I would call simply *erysipelas*, falls naturally within that group of exanthematous diseases which includes small-pox, measles, scarlet fever, the plague, and continued fevers. It is an idiopathic inflammatory disorder, running a *tolerably regular and definite course*; attended by inflammation of the integuments of the body, or in other words, by an *eruption*; often *prevailing epidemically*; and capable of being *communicated*, under circumstances favourable to its propagation, from one person to another. Its power to protect the constitution from its own recurrence is less certain; but in truth, so many different affections have been lumped together under a common name, that the proper phenomena of true *erysipelas* have not been made sufficiently an object of separate study to enable us to speak with any confidence on this point. I recollect, however, a certain female who has been three or four times my patient in the hospital with *erysipelas*: and one of the night nurses there, whom I treated for that complaint some time ago, is now lying ill of the same disorder under the care of one of my colleagues.

Erysipelas, in the sense now explained, called in Scotland the *rose*, and in this country *St. Anthony's fire*, resembles other disorders of the same group in these points also, that the fever *precedes* the local inflammation, that certain premonitory symptoms frequently go before the outbreak of the disease, and that *sore throat* is an early, and almost a constant, accompaniment of the complaint. The patient feels ill — shivery, feeble, languid, and often drowsy. The actual attack generally sets in with distinct rigors; and the pulse is often very frequent from the first, for many hours, perhaps, before the redness commences. Very commonly there is also manifest disturbance of the alimentary canal, marked by nausea and vomiting, and not unfrequently by diarrhœa. Then some part of the face, usually one side of the *nose*, or one *cheek*, or the rim of one of the *ears*, begins to feel, hot, stiff, and tingling: and upon examining it you find it to be of a deep continuous red colour, and to be swelled and hard. The redness and swelling gradually, and sometimes rapidly, extend themselves: they are defined by a distinct elevated margin; which advances, and invades progressively the neighbouring healthy surface, until the whole of the face, or of the scalp, or of both, is occupied with the inflammation. The lips swell enormously, the cheeks enlarge, the eyes are sealed up by their œdematous and prominent lids, and all traces of the natural countenance are effaced. I know of no disease, except perhaps the confluent small-pox, by which the human face divine is

so completely and speedily deformed and disguised. A stranger seeing a young female in the height of the disorder, and revisiting her after her recovery, is astonished at the change. It seems as if, by some magic process, such as we read of in our nursery tales, a hideous monster has been metamorphosed into a comely damsel.

The inflammation frequently spreads from the face and forehead, or ears, to the hairy scalp; and from the head it travels backwards, in some cases, to the neck and the shoulders. Sometimes — and in this it exactly resembles a scald — the inflamed surface becomes covered with irregular bullæ, or blisters; but often there is *no* vesication. This circumstance, therefore, which has been mentioned by some as furnishing a point of distinction between erythema and erysipelas, fails of that purpose.

In many cases the inflammation is quite superficial: in others it dips, as it were, through the skin, and affects the subcutaneous areolar tissue, and then and there supuration and even sloughing of that tissue, are apt to take place. We find this to be the case often in the loose tissue of the eyelids; and it is more common on the scalp perhaps than on the face.

After the redness has lasted three or four days, it fades, the swollen surface subsides, and desquamation ensues; and as the inflammation creeps, perhaps, gradually from one part of the surface to another, you may find the face becoming pale, and covered with patches of dead cuticle, while the scalp, or the upper part of the neck, is becoming red. Sometimes those parts of the inflamed surface on which blisters had formed are covered with *crusts*, rather than with merely dead and dry cuticle. In almost all these cases of erysipelas of the head and face, there will be found to be redness and soreness of the *throat* also; although this is not always inquired into, or complained of.

There is considerable variety in the intensity and complication of the symptoms. Sometimes the sufferer lies patiently still, yet apparently conscious and rational, till the tumefaction diminishes, and he is again able to open his eyes. Generally there is some wandering of the mind, especially at night; and in bad cases there is much delirium, and at length complete coma, and the patient dies at the end of a few days. In some of these cases the inflammation has extended to the encephalon; in others it is probable that the functions of the brain are disturbed through the febrile derangement of the circulation. When death takes place, and the head is examined, serous fluid is usually discovered beneath the arachnoid, or in the cerebral ventricles; and the veins of the pia mater are turgid. I have stated before that I doubt whether such appearances are always to be attributed to inflammation. Sometimes there are no morbid appearances at all within the skull.

It is said that the erysipelas does, now and then, suddenly desert the surface; and that inflammation of some internal part, and particularly of the brain, is apt to follow such rapid subsidence of the external malady. I presume that this metastasis is rare. I do not recollect to have seen it. But the *extension* of the disease, the supervention of delirium and coma, while the external inflammation *continues*, is of common occurrence.

This, then, is one way in which erysipelas is accustomed to prove fatal; by effusion within the head, and *coma*.

And there is another mode in which death is not unfrequently brought about, and which has not been so much attended to; I mean by the affection of the throat. The patient dies sometimes almost suddenly; unexpectedly; you cannot account for the unlooked-for dissolution. But if the throat be examined you may (sometimes at least) there discover the solution of the mystery. The sub-mucous tissue of the glottis and epiglottis is filled with serum, or pus, the chink of the larynx has been nearly or completely closed; and the patient has died of *apnoea*. This is just analogous to what takes place externally: the enormous swelling of the eyelids, and lips, and face, is owing, in a great degree, to serous fluid poured out into the *subcutaneous* areolar membrane.

Another way in which erysipelas may kill, is by gradual *asthenia*. Without any stupor or much wandering, without any marked affection of the breath, the pulse becomes weaker and weaker, the surface cold, and the heart at length ceases to pulsate. This mode of dying is less common in this disorder than the other two.

The causes of erysipelas are various, and often obscure. I have stated that it is

communicable, by contagion, from person to person: yet this contagious property is so feebly marked, that it is denied by many. It is more active at certain times, at certain seasons, at certain places, than at others; which is the same thing as to say that there are *predisposing* causes of the disease; that there are influences which augment the susceptibility of the body to the agency of the poison.

I believe that on the Continent they do not allow erysipelas to be contagious at all: but very satisfactory evidence of the fact has been collected by several of our own practitioners. In the second volume of the *Transactions of a Society for the Improvement of Medical and Chirurgical Knowledge*, Dr. Wells has brought together several examples in which the complaint appeared to be unequivocally propagated by contagion. "I visited (says he), on the 8th of August, 1796, in Vine Street, Clerkenwell, an elderly man, named Skelton, who had been attacked several days before with *erysipelas of the face*. In about a week afterwards he died. On the 19th of the following month, I saw a Mrs. Dyke, of about seventy years of age, the landlady of the house in which Skelton had been a lodger, and found *her* labouring under an erysipelas of her face. I inquired whether any other person in her house had been ill of the same disease since the death of Skelton, and was told that his wife had been seized with it a few days after his decease, and had died in about a week. During my attendance upon Mrs. Dyke, an old woman, her nurse, was attacked with the same disorder, and was sent to her parish-workhouse, where *she* died. Mrs. Dyke has since informed me that a young man, the nephew of Skelton, was taken with the disease of which his uncle had died, shortly after visiting him, and survived the attack only a few days. That she herself had been several times with Skelton and his wife during their sickness, and after their death had removed some furniture from the room they had occupied to her own apartment." Dr. Wells relates other histories of the same kind, all occurring when there was no particular *epidemic* of erysipelas prevailing to account for them. Professor Arnott has given some other examples, still more striking, of the propagation of erysipelas from one person to another, not only under the same roof, and in the same locality, but also when the parties lived at a distance from each other, and the intercourse between them had been casual and temporary. These cases are stated, I think, in the fifty-seventh volume of the *London Medical and Physical Journal*. The following incident has been told me upon good authority. A man living somewhere in Westminster fell ill of idiopathic erysipelas. In that state, for some reason or other, he was removed thence: and his brother, who was a servant in or near Portland Place, received him clandestinely into his master's house, and allowed him (for two nights, I believe) to share his bed. That brother was soon attacked with erysipelas; and in the course of his illness was visited by his master. The master also was attacked; and it is worthy of remark, that in both master and servant, the disease showed itself just seven days after they had respectively come near another who was affected with it. Dr. Elliotson gives an account of having suffered the disease in his own person. It began in him five days after the breath from one of his patients, over whom he was stooping to examine the skin, and who had erysipelas badly, and died of it, had come upon his face. "I turned away (he says) disgusted, and said, *I hope I have not caught it*; but five days afterwards, having forgotten the circumstance, I was seized with it." More than once I have had occasion to remark that successive tenants of the same bed in a hospital have been seized with erysipelas after their admission.

But allowing, as I think we cannot but allow, that contagion is one of the exciting causes of erysipelas, there are others which *more frequently* excite it. At least there are many instances of the disorder in which we can trace no exposure to contagion, and in which we can perceive some other probable reason for its occurrence. Sometimes, no doubt, it comes on without any obvious cause. The application of cold often gives rise to it. Irregularity of diet is said to do the same. Violent mental emotions are also accused of being occasionally its cause: it is said to have been brought on both by anger, which is an exciting, and by fear, which is a depressing, passion. It is incidental to what I have called purulent infection of the blood, and in all probability there has been a previous morbid condition of the blood in all those instances of the disorder which appear to be produced by exposure to cold, or by mental agitation. Many cases, not to be distinguished in their appearances and effects from idiopathic erysipelas, result from local injury. I have already told you

that there is no inconsistency or absurdity in supposing that a disorder which originates in some common cause, may be capable of spreading in the way of contagion. And the testimony of Mr. Lawrence (who is not prone to admit of contagion on light grounds) goes to the effect that such is the case with the disease in question. He mentions an instance in which erysipelas of the head and face, which commenced after the insertion of a seton in the neck, appeared to him to have affected two individuals by contagion.

I say that erysipelas further resembles the disorders of the group in which I have placed it, in that it sometimes prevails *epidemically*: and on those occasions, like the rest of the group, its occurrence is promoted by all circumstances that tend to debilitate the body: by intemperance; by previous disease; by low spirits and anxiety; by insufficient nourishment; and by foul air. It used to be much more common formerly in hospitals than it is at present; when less attention was paid to cleanliness and ventilation.

Erysipelas is another of the diseases concerning the *treatment* of which there has been, and perhaps there may still be, a most embarrassing difference of opinion. When a student many years ago at St. Bartholomew's Hospital, I observed that in the physicians' wards nearly every case of erysipelas was treated at once with bark and wine; in the surgeons' wards nearly every case with depletion and tartarized antimony. Recoveries took place under both methods; and mutual sneers were not wanting. However puzzling this opposition of opinion was to me at the time, I have since learned to side with the physicians. Not that I hold as commendable the facile, untroublesome method, which prescribes the lavish and indiscriminating use of stimulants in every instance. In this, as in many other disorders, it is our safest and best policy to watch the symptoms very narrowly, and to regulate, and it may be to trim, our practice according to their course and character.

I think, the more you see of this disease the more convinced you will be that it is not to be *cut short* by any particular mode of treatment; that it will run a certain course; and that it will *generally* terminate, sooner or later, by resolution, whether remedies are employed or not. It does not follow from this that remedies are of no use; but it does follow that we are to exhibit them, not with the view of *curing* the disorder, but with the view of *conducting* it safely to its termination. Our objects must be, on the one hand, to repress (if we can) any tendency to the spoiling, by inflammation, of vital organs; on the other, to sustain (if we can) the general strength until the disturbance has passed off.

If you look at the history of erysipelas, and of the notions which have *prevailed* respecting it, you will find that the opinions in favour of giving support, and of abstaining, as much as possible, from the abstraction of blood, greatly preponderate. In the outset of the complaint, if the pulse be *hard*, as well as frequent, and there be much headache, and active delirium, it may be right to take blood cautiously either from the arm, or from the neck by cupping, or by leeches from behind the ears. But (I speak of the disease as it occurs in *London*) the time for this kind of practice is soon over: and whenever it may appear to be requisite, the desired effect must be sought by the abstraction of the smallest available quantity of blood.

In all cases it will be right to empty the bowels at the commencement. A dose of neutral salts, or of rhubarb and magnesia, will answer better, I think, in these cases, than calomel and senna. If you see the patient very early, and if there be any nausea or oppression of stomach, it will be proper to administer an emetic.

In most instances you will soon perceive evidence of great debility: a *feeble* as well as a *frequent* pulse; tremors; a dry and brown tongue often. And these symptoms increase, if you *then* persist in drawing blood. The carbonate of ammonia does great good sometimes in such a condition: and this I am very much in the habit of giving; with animal broths and wine. The bad cases of erysipelas are apt to baffle us all. I am not aware that I lose more of them than my neighbours. Speaking generally, a large majority of my patients get well; but I do not bleed one patient in a hundred, from the arm.

I am bound, however, to set before you the kind of evidence which exists in favour of the bark; or rather of the sulphate of quina, which is what I mean when I speak of the bark.

Dr. Fordyce, Dr. Wells, Dr. Heberden — all men of great sagacity and experience

—recommend the treatment by bark. Dr. Jackson, an American physician, advocates, I see, the same plan. He says, that after a purge, and, if necessary, an emetic, the sulphate of quina should be given in as large doses as the patient will bear; that from twelve to twenty-five grains in the twenty-four hours will generally suffice; and that we may know when the dose is sufficient by a buzzing which comes on in the ears. Dr. Elliotson also—whom I here quote the more willingly because I think he is rather of an antiphlogistic turn than otherwise, in general—says, that he has never seen quina do harm, even in active tonic erysipelas; and that in doubtful cases, when you hesitate whether to bleed and put the antiphlogistic plan in force, or to stimulate and support, the quina is *always* a safe and *eligible* medicine. Dr. Robert Williams, of St. Thomas's Hospital, thinks better still of *wine*, which he gives in *all cases from the very beginning*.

Taking the disease as I see it in London, I should say that *many cases* do well with but little care or interference from medicine; that *many also*, but a fewer number than the former, prove fatal under whatever plan of treatment may be adopted; and that many patients are to be saved, by judicious management, who would otherwise die.

The first requisite for rescuing these perilous, yet recoverable, cases, is that they should be perpetually watched and tended. The indications of treatment may alter from one hour to another, and it is only by great vigilance on the part of the medical attendant, and on the part of an intelligent and obedient nurse, that medicine obtains its full chance of bringing the patient through.

After clearing out the alimentary canal, then, I would not be active in either way, unless I saw some plain indication for activity. If the pulse became weaker, and I did not feel sure about the propriety of stimulating, I would give five or six grains of the carbonate of ammonia every four hours, and beef-tea. If the disease went on smoothly under that treatment, well and good. If the powers still continued to sink, I should have recourse to wine, or to both bark and wine; but of the two, I am more friendly to wine; and the patients like it better, you may be more sure of their taking it. If, on the other hand, there were much headache, and the pulse were hard, and the febrile distress great, I should apply a few leeches, and prescribe the tartarized antimony in a saline draught. The bowels should not be allowed to become confined; but the mild aperients are better than the drastic in these cases.

I must not omit to tell you, although as yet I have been unable to put the remedy to a practical test, that the muriated tincture of iron has been very strongly recommended in this disease. I should have no scruple in trying it where there was no great cerebral disturbance. Dr. Balfour, after treating twenty cases with from ten to twenty minims of the tincture every two hours, is so thoroughly satisfied of its efficacy as to record (in the *Monthly Journal* for 1853) his belief that we have now “a certain and unfailing remedy, whether the erysipelas be infantile or adult, idiopathic or traumatic.”

Various external medicaments have been used and recommended for the inflamed part. Putting leeches upon it; puncturing it with needles, or lancets; covering, or surrounding it with a blister, or with the lunar caustic in substance or in strong solution; dusting the surface over with magnesia, or with flour; smearing it with various unguents, and particularly with mercurial ointment; keeping it wet with some cold lotion; or fomenting it with hot flannels.

Now of all these local appliances, that which, according to my own observation, is the most useful, and which affords the greatest comfort to the patient, is the last that I mentioned; fomentation by flannels wrung out of a hot decoction of poppy-heads. But in order to give *this*, also, fair play, it should be *continual*: not used for half an hour, and then intermitted; but it should be one person's business to apply the fomentation assiduously, as long as it is soothing and grateful to the patient. The local treatment most in favour with our apothecary at the hospital is that of covering the inflamed face and head with flour, by means of a dredging-box. The patients declare that the flour cools, soothes, and comforts them. This is a more convenient, and, in some respects, a more eligible, application than that of hot flannels. It is less likely to fail of its purpose through the negligence of the nurse.

[Keeping the inflamed surface constantly covered with perfectly fresh hog's lard,

has a very soothing effect, and tends to reduce the violence of the inflammation. We have occasionally employed an ointment formed of acetate of lead fifteen grains, rubbed up with one ounce of hog's lard, and have been pleased with its effects.—C.]

So much for erysipelas, as it usually comes under the notice and the management of the physician. You are aware that an affection of the skin very similar to that which I have been describing, and called also by the same name of erysipelas, is very common in other parts of the body; on the extremities especially, and occasionally on the trunk: and it will travel sometimes from an extremity till it reaches the head. These varieties of cutaneous inflammation are, in most instances, the indirect consequences of some local injury: of punctured wounds; of the stings of insects, or the bites of venomous reptiles; of mere scratches sometimes. Or the cutaneous inflammation will spread from old sores; or supervene upon dropsical limbs. It is curious that these complaints also are much more apt to occur, and even to multiply by a sort of contagion, or in virtue of some epidemic atmospheric influence, at certain times and places than at others. You will find that there are periods when the surgeons of hospitals dread to perform any operation, lest it should be followed by this spreading inflammation of the skin. There are many points of great interest connected with these diversified forms of what is called erysipelas: they are more liable to be attended with gangrene than erysipelas of the face and scalp: on the other hand, they are more liable also to be complicated with inflammation of the subcutaneous areolar tissue, and with suppuration; and to require incisions to relieve the great tension of the inflamed parts, and to facilitate the escape of the pus, or of sloughy dead portions of areolar tissue: but all these matters belong rather to surgery, and have been discussed, I make no doubt, by the professor of surgery.

[Erysipelas is of frequent occurrence in the earlier period of infancy, and is among the most fatal of the diseases which occur at that period. It very generally terminates in death, especially when it occurs during the first month. Its danger gradually diminishes as the child advances in age, but even when it attacks after the fourth month, the greater portion of the cases terminate fatally.]

The invasion of the disease is always extremely insidious. The child appears somewhat morose; his sleep is slightly diminished, and he sucks rather less than before; with these trifling symptoms, there is associated a small patch of redness upon the pubes, which is painful upon pressure. The redness gradually spreads over the abdomen and thighs, and is, occasionally, disseminated. When the redness extends to the hands and feet, these parts acquire a degree of redness and swelling far greater than that of any other part. The genital organs, in some cases, sphacelate, in consequence of the local inflammation, and in many acquire an emphysematous appearance. In place of appearing first upon the pubes, the erysipelas has been known to extend from the areola of the vaccine vesicle, less frequently from any accidental laceration of the skin or from the excoriations so common in the cutaneous folds of the groin and other parts.

The disease at first appears perfectly local—it is not until several days have elapsed, that general uneasiness and fretfulness present themselves. The colour of the skin, and the expression of the countenance, often remain for some days without exhibiting any striking change, when, suddenly, an ashy cadaverous paleness is observed; the child cries incessantly, there is constant jactitation, and complete loss of sleep. These symptoms are succeeded by deep stupor and death. The pulse is at first frequent, and the heat of the skin sinks only during the fatal stupor which precedes death. Convulsions, diarrhœa, and vomiting are seldom observed. When the progress is such as we have described, peritonitis has occurred—which is, according to M. Trousseau, a frequent disease in children, and one which has not hitherto been accurately described. The duration of infantile erysipelas varies considerably, sometimes being extremely short, in others, on the contrary, being prolonged to the end of the week.

In post-mortem examinations, the cutaneous disease is occasionally the only morbid change detected; but when peritonitis (a frequent complication) has occurred, the umbilical vein is often found inflamed, and filled with pus, as far as the transverse furrow of the liver, while inflammatory exudations are found on the peritoneal surface of the abdominal viscera.

According to M. Trousseau infantile erysipelas is principally observed when puerperal fever prevails in the wards of the lying-in hospitals of Paris. The infants appear to inherit from their mother a *purulent diathesis*, and seem to be still, within certain limits, subject to the same maladies as the mother, whose constitution has so lately been theirs. The peritonitis of the infant may be, therefore, as properly termed puerperal as that of the mother, because its general cause is to be sought for in the circumstances which have accompanied the last stage of child-bearing and parturition. It is natural that the skin should be the seat of disease, from its having been so recently called upon to the performance of functions as new as they are important.

In the infants predisposed to erysipelas, the umbilical cicatrix does not form readily, and the ulceration which results is sometimes the occasional—the local cause—of the cutaneous disease.

Dr. Friebe (*Journal für Kinderkrankheiten*) describes a form of erysipelas which commences about the umbilicus, within a few weeks after birth, and is frequently accompanied with ulceration of the navel, and infiltration of lymph or pus into the subcutaneous cellular tissue, and deposits of a similar nature in the partially obliterated umbilical vessels. It is attended with great and rapidly increasing exhaustion, and occasionally convulsive symptoms of more or less severity. It is most generally fatal; often within the course of forty-eight hours, and without having extended more than three fingers' breadth around the navel.

Dr. Friebe is inclined to view this form of infantile erysipelas as a variety of partial induration of the cellular substance, in consequence of the cachectic condition of the infants in whom it usually occurs, and the peculiar character of the inflammation—while the circumstance of the umbilical vessels being, in fact, converted into fibrous cords, previously to the commencement of the disease, leads him to believe that it cannot arise from the umbilical phlebitis.

We have met very frequently with this form of infantile erysipelas among the children of the poor, and our observations have convinced us that, in the majority of cases at least, it is dependent upon phlebitis of the umbilical vessels; the evidences of inflammation of the latter were unquestionably present in almost every instance in which we have made an examination.

In regard to the treatment of infantile erysipelas, although in those cases in which the disease occurs soon after birth there is but little chance of saving the life of the child, yet when the patient is a few months old, we may do much in arresting the progress of the disease, and conducting it to a favourable termination.

We may remark, however, that erysipelas is always to be considered a dangerous disease during infancy, but more particularly when it occurs in infants of a weakly and unhealthy habit of body.

The bowels should be freely opened by a grain or two of calomel, followed in a few hours by a teaspoonful of castor oil, or a simple laxative enema, and they should be kept in a free condition subsequently, by the exhibition, every three hours, of divided doses of calomel, combined with minute doses of ipecacuanha and extract of hyosciamus—the addition of the latter is calculated to prevent or allay irritation, without interfering with the aperient action of the calomel. When the skin is warm or dry, the liquor acetatis ammoniæ alone, or combined with antimonial wine, will often prove beneficial, as will also the occasional use of the warm or tepid bath.

If symptoms of cerebral disease should occur, a leech or two should be applied to the temples,—cold applications to the scalp,—the bowels should be freely opened by calomel, followed by castor oil, or purgative enemata, and warm sinapised pediluvia should be resorted to.

In a few cases where the parts occupied by the disease are decidedly red and swollen, a few leeches applied upon the sound skin, in the neighbourhood of the inflammation, will often prove beneficial. Much judgment, however, is required, to discriminate the kind of cases which are likely to be benefited by local depletion. As a general rule, a very moderate abstraction of blood will be sufficient.

When a tendency to gangrene is obvious, and the disease assumes a low typhoid character, it will be necessary to resort at once to the use of the carbonate of ammonia, in doses adapted to the age of the patient, and the urgency of the symptoms; and at the same time, the child should be nourished at the breast of a healthy nurse, or if weaned, its diet should consist of animal jellies dissolved in water, beef tea,

chicken water, or sago. It is all important in these cases that the patient be removed to a healthy situation, where he can enjoy the tonic influence of a pure, free atmosphere. When the symptoms of prostration are more considerable, we may combine the use of the ammonia with the sulphate of quinia and extract of hyosciamus.

Should diarrhoea ensue, particularly after suppuration has taken place, it should be immediately checked by the cretaceous mixture, with the addition of tincture of kino and laudanum. When the discharges from the bowels are of a vitiated character, or attended with griping or colicky pains, a few drops of turpentine, three times a day, will be found a very valuable remedy. The turpentine may be combined with the tincture of kino and laudanum.

With respect to the local treatment—washing the inflamed parts frequently with tepid water, or some mucilaginous fluid, will, in many cases, be productive of very great relief to the patient, and occasionally will produce an abatement of the disease; in the intervals, the parts may be dusted with powdered starch, or covered with carded cotton.

When there is decided redness, swelling and tension of the skin, a lotion, composed of a solution of equal parts of the acetate of lead and subcarbonate of ammonia, has been highly recommended. Covering the parts with rags wet with the camphorated tincture, is said to prove often very beneficial. Others recommend a wash composed of a solution of corrosive sublimate, three grains to the ounce of water, or of nitrate of silver of the same strength. It is said that the progress of the disease has been promptly arrested by applying once or twice a very strong solution of the nitrate of silver, by means of a pencil, upon the sound skin, for about an inch around the margin of the inflammation. One of the best applications in cases which exhibit an early tendency to gangrene, is the chloride of lime in solution, in the proportion of half a drachm to a pint of water. Velpeau has employed with advantage a solution of half an ounce of the sulphate of iron to eight ounces of water.

The local remedies just recited are to be employed in the early stages, previous to the rupture of the vesications, and to the occurrence of suppuration or gangrene.

Among the local remedies which appear to be the most generally applicable to infantile erysipelas, is a blister sufficiently large to extend over the inflamed skin, and for a short distance beyond it; after vesication has been produced, the serum should be evacuated, and the vesicated surface dressed with fresh lard. When the erysipelas occurs upon the extremities, a blister applied around the limb, beyond the inflamed surface, will frequently arrest the progress of the disease in that direction. When we have resorted to blisters, which are only proper in the early stages of the disease, we have applied them along the edges of the inflamed portion, partly upon it, and partly upon the sound skin, removing them at the end of three hours, and immediately covering the parts with a soft emollient poultice, with the addition of lard. The most important result which we have derived from the use of blisters is to prevent the spread of the disease.

The mercurial ointment has been strongly recommended in the treatment of infantile erysipelas. It is to be spread on a piece of soft linen, with which the affected parts are to be covered.

In cases attended with an intolerable sensation of burning, we have often derived benefit from the local application of a watery solution of opium.

In a large number of instances, however, no benefit whatever will be derived from any local application. M. Trousseau states that he has tried every imaginable one—ointments, lotions, blisters, even the actual cautery—without suspending the progress of the disease. Three cases only, occurring within the month, has he seen recover under the use of the ethereal solution of camphor, and baths containing corrosive sublimate. The disease is certainly a very unmanageable one when it occurs in very young infants; we have nevertheless seen it in children a few months old in many instances promptly arrested by the remedies already detailed.

Whenever the cellular membrane is involved in the disease, free incisions should be early made, as well to unload the vessels, and relieve the swelling and distension, as to give exit to the purulent fluid and dead portions of cellular structure. The pus, if allowed to remain, will travel beneath the skin, and between the muscles and tendons, and thus increase the danger and extent of the disease. After the incision,

the part should be covered with soft emollient poultices, and kept as much as possible at rest, and in an elevated position.

When the erysipelas terminates in gangrene, washes of a solution of the chloride of lime, of the chloride of soda, of a few drops of creasote diffused in water, or of a strong decoction of black-oak bark, should be immediately employed, or the sphacelated surface may be covered with the charcoal or yeast poultice. A wash of a very strong solution of the sulphate of copper will in some cases arrest the progress of the gangrene.

When convalescence has been established, some light mineral tonic or the mineral acids, largely diluted, should be administered. The child should be allowed a nourishing diet; he should be daily immersed in a warm or tepid bath, and enjoy a dry, fresh, wholesome atmosphere.—C.]

Very closely connected with erysipelas, and continually confounded with it, is *erythema*. It also consists in superficial redness of some portion of the skin; but it is not attended with inflammation of the areolar texture under the skin: nor with vesication; nor, in general, with fever; nor is it peculiar to the face and head.

There are numerous varieties of erythema described by writers on cutaneous disorders, to whom I must refer you for an account of them. Willan and Bateman; Wilson; Willis; Rayer; Alibert; and Biett, as his practice and lessons are reported by two of his pupils.

The only variety on which I am disposed to say a word, is one which is attended with more or less febrile disturbance. I mean what is called *erythema nodosum*.

This curious affection occurs much more often in young women than in any other persons: sometimes in feeble boys. The eruption is commonly preceded for a few days by indisposition, and some slight degree of fever. Then red elevated spots come out, on the fore part of the legs, and occasionally, but very rarely, on the arms. The redness appears in oval patches, of which the long diameter is parallel to the axis of the limb. They are pretty large patches, an inch and half long, and an inch broad perhaps, and they evidently project and form bumps upon the anterior surface of the leg. From their look, you would suppose abscesses were about to form; but after lasting a few days, the red colour fades, or rather changes to a blue, and the protuberances gradually subside. This eruption seems sometimes connected with disturbance of the menstrual functions. Rayer has seen it occur in connexion with acute rheumatism. So have I. A patient of mine in the hospital was attacked with acute rheumatism of the joints immediately upon the cessation of *erythema nodosum*. In another this order was reversed.

Now I am persuaded that, after an aperient, *rest, the horizontal posture, and quina*, constitute the proper treatment of this affection. I had once a housemaid in whom the disorder appeared, and was attended with unusually high fever, and much indisposition. I treated her, therefore, antiphlogistically; *i. e.*, I kept her on low diet, and gave purgatives; but the disease went on. Fresh knots came out as the old ones faded. At length, I do not remember why, I prescribed some quina for her; and the improvement was immediate, and very striking. She relapsed, however, once or twice, upon leaving off the bark; but by persisting subsequently in its use for some days after she appeared to be well—a permanent cure was effected. Since that time—now nearly twenty years ago—I have seen a good many examples of *erythema nodosum*, and I have treated them all alike; *viz.*, first with an aperient, and then with the sulphate of quina; and they have all rapidly got well. Probably they would have recovered nearly as soon under some other tonic treatment; but I have been so well satisfied with this, since I began it, that I have felt no temptation to try any other.

There is a rash which is well known, and very tormenting, and therefore, not without interest, although it is almost always without danger: I mean *urticaria*. It is arranged by Cullen among the exanthemata; but it does not properly belong to the group to which I would restrict that name, for it is not contagious, and it may happen to the same person a hundred times over. The eruption consists of what, from analogy, are called wheals: *i. e.*, of little solid eminences, of irregular outline, but generally roundish or oblong, and either white or red, or (which is most common)

both red and white; the whiteness occupying steadily the central and most projecting part of the spot, or becoming manifest there when the integuments are put upon the stretch. The rash is accompanied with intense heat, a burning and tingling in the affected spots, and great itching and irritation. In truth, both the appearances upon the skin, and the sensations that attend them, are very much like the appearances and feelings produced by the stinging of nettles. Hence its trivial name, *nettle-rash*: which is, indeed, the same, in meaning, with the scientific appellation, *urtica* being the Latin for a nettle. Similar appearances follow almost immediately upon a smart blow with a cane, or with the lash of a whip, on the skin. Red stripes or *wheals* arise, and within the reddened surface one or more elevated spots of a white colour are visible.

There are two varieties of urticaria: one in which the complaint runs a short course, and soon subsides, and may be considered acute: another in which it is chronic, and either persistent or intermittent. The acute form is attended with feverishness, which sometimes begins two or three days, but commonly not more than a few hours, before the eruption appears; or the fever and the rash may commence together. In most cases, perhaps in all, the disorder is intimately linked with some derangement, manifest or latent, of the stomach, and it may often be traced to the imperfect digestion of particular articles of food. It is very curious that the contact of certain substances with the mucous membrane of the alimentary canal should affect the external tegument precisely in the same manner as the virus of the nettle, and some other irritants, when these are applied to the skin itself. The stomach may be healthy, yet incapable of digesting some particular substance, which then acts like a poison; or it may be habitually weak, and unequal to the digestion of matters which the sound stomach disposes of with ease. The offending articles of food do not produce the cutaneous affection in all persons, nor even necessarily in the same person at all times. But there are some edible substances which are much more likely than others to be followed by nettle-rash. Certain vegetable matters are very apt to excite the disorder in some persons: oat-meal; almonds, especially the bitter almonds; any bitter kernels; particular species of strawberries; raw cucumbers; mushrooms. Some of the vegetable substances used in medicine are known to have, frequently, the same effect; capivi, for example; the cubeb pepper; valerian. Urticaria has been brought on by drinking porter, or, most probably, by some of the drugs with which our porter is sophisticated. These effects are not confined to vegetable substances. Shell-fish is a common source of nettle-rash. I have known it to be occasioned by prawns; crabs sometimes have the same unpleasant consequence; and muscles still more often. An hour or two after some one of these substances has been swallowed, and perhaps much sooner, nausea is felt, and oppression about the epigastrium; the patient becomes giddy, his face and head sometimes swell, his skin begins here and there to burn and tingle, and presently the eruption, as I have already described it, breaks forth. It is attended with intolerable itching and pricking sensations, especially at night, when the patient is warm in bed; or when the affected surface is exposed to the air. Vomiting and diarrhoea often supervene, and prove the natural cure of the attack.

We read that this disorder has sometimes proved fatal; but this must be under very unusual circumstances of weakness in the patient, or of some peculiar virulence in the exciting cause.

The chronic form of the complaint is apt to be very obstinate and teasing. It comes and goes, and comes again. The evening is one of its favourite periods. In those who are subject to it, the itching and the wheals are readily brought on by scratching or rubbing the surface. This is the urticaria *evanida* of Willan. Dr. Heberden had known persons afflicted in this way for ten years together. I have observed nettle-rash to occur in connexion with sudden and violent paroxysms of dyspnoea, resembling asthma fits; so that I could not help suspecting that the mucous membrane of the respiratory passages was irritated after the same fashion with the external skin.

Even the chronic variety of urticaria is, in some instances at least, produced by certain *ingesta*; and the peccant substance may often be detected, and the tiresome malady be cured, by following the simple and judicious plan recommended by Willan; namely, that of instructing the patient to abstain, for a while, from all his customary

articles of diet, one by one, in their turns. This experiment does not, indeed, always answer. The urticaria will sometimes abide, notwithstanding: so that although it probably depends in all cases upon some disordered condition of the stomach or bowels, we cannot say that such disorder is *always* the consequence of something that has been swallowed.

The treatment to be adopted in the acute or febrile nettle-rash, when it depends upon something recently received into the stomach, is that which common sense would suggest, and which nature often plainly indicates. We seek to expel the offending material by an emetic, and by purgatives: and this being done, the cure is completed. In the more chronic and recurring varieties, we endeavour, in the first place, by making the experiment recommended by Dr. Willan, to detect, that we may thenceforth interdict, any article of diet which may have caused the disorder. If we fail in this attempt, our object must be to correct that faulty state of the digestive organs, or to neutralize that inbred poison, upon which the cutaneous affection depends. Laxatives and antacids are found to be the most successful means of attaining these ends. They may be given together, or separately. A few grains of rhubarb taken daily just before breakfast, and just before dinner, have cured a chronic case of long standing. Or rhubarb and magnesia may be taken together; the carbonate and sulphate of magnesia; castor oil. The snake-root has obtained some repute as a remedy for urticaria. You may prescribe, therefore, if you please, a scruple each of the carbonates of magnesia and of soda, in the infusion of serpentaria.

External applications seem to be of but little avail in this disease; and those which do appear to be of service, act uncertainly, and produce different effects in different persons. The warm bath sometimes gives ease in the severer cases. In the more chronic form of the disorder, spiritous washes, vinegar, sea-bathing, are things to be tried. And cases are related in which, when every other expedient has failed to give permanent relief, removal to a warmer climate has been successful. Dusting the itching surface with flour has, in my experience, afforded much temporary comfort. Still more useful perhaps is a lotion (first recommended by Wilkinson, in a little work on skin diseases) composed of a drachm of the carbonate of ammonia, a drachm of the acetate of lead, and eight ounces of rose water. Its efficacy may be increased by the addition of half an ounce of laudanum.

I should add, that Dr. Elliotson has found *bleeding* very efficacious in relieving patients affected with acute or febrile urticaria. And you may have recourse to the lancet if the patient be strong and plethoric, and his pulse warrant it; and if he be so impatient of the irritation as not to be willing to wait the effect of other treatment, which would probably be quite as effectual, though not quite so rapid.

Prurigo—itching—is a cutaneous affection bearing some analogy to urticaria, at least in the sensations which accompany it. And a most terrible and melancholy affection it often proves to be. Sometimes the parts of the skin which are the seat of the itching do not present any perceptible deviation from the condition of health; but in the majority of instances, you will find, upon close inspection, that they are covered with papulæ, which are nearly of the same colour with the skin itself. Willan therefore places prurigo in the order of Papulæ. He describes several varieties of this troublesome complaint: prurigo *mitis*; prurigo *formicans*; prurigo *senilis*. The torment experienced by patients suffering under the severer forms of the malady is scarcely describable; they scratch and tear themselves incessantly till the blood flows, their sleep is broken, and their lives are rendered perfectly miserable. Sometimes this itching is diffused irregularly here and there over the surface; sometimes it affects the extremities only; and frequently it has a still more limited habitat, occurring round the anus, when it is called prurigo *podicis*; or on the scrotum, prurigo *scroti*; or, worst form of all, the prurigo *puerendi muliebris*.

All these forms of prurigo are apt to be aggravated by heat, and by exposure to the air; they are, therefore, especially distressing when the patient undresses and goes to bed. The scratching tears away the summits of the papulæ, and some watery fluid mixed with blood escapes, and concretes into small thin, black scabs. In the prurigo formicans, the itching is combined with other painful and disagreeable sensations, which different patients describe in different terms: the feeling is like the

creeping of ants, or the stinging of insects, or as if hot needles were thrust into the skin. The *prurigo senilis*, occurring, as that name implies, in old persons, is usually very obstinate, and often effectually destroys all comfort for the rest of the patient's life.

In such cases as I have now been mentioning, great care should be taken thoroughly to cleanse the surface of the body: and the diet should be rigidly plain. All kinds of rich sauces, hot condiments, pickles, and indigestible substances, should be peremptorily forbidden. Various local applications have been praised; but they are, in most cases, used in vain: vinegar, lime water, decoction of *dulcamara*, lotions composed of prussic acid in an emulsion of bitter almonds, a dilute solution of creasote, decoctions of *stavesacre*, and of *digitalis*, ointments containing mercury, tar ointment, and a hundred others. In one instance lately, where the ingenuity of another practitioner had been fruitlessly exhausted, I was fortunate enough to effect perfect relief by smearing the itching surface with an ointment containing a small quantity of *aconitine*. Mr. Gabb has found a weak dilution of the *Liquor Sodæ Chlorinatæ* very serviceable. Of internal remedies, *sarsaparilla*, alkalies, arsenic, the iodide of potassium, *dulcamara*, are the most hopeful. When these means fail, opium is our best, and indeed our only resource.¹

The local forms of *prurigo* are frequently connected with local disease, and are most likely to be relieved by measures directed against the primary disorder. *Prurigo podicis* is sometimes dependent on the presence of *ascarides* in the rectum. The same troublesome affection is not an uncommon symptom of internal piles: and it sometimes accompanies stone in the bladder.

The *prurigo pudendi muliebris*—itching of the genitals in females—is sometimes so constant and tormenting, and the impulse to scratch the itching part so urgent, as to drive the unhappy patient from society. It even gives rise, in some severe cases, to *nymphomania*. It may proceed from *leucorrhœa*: it is frequently a sign of uterine disease. It most commonly affects women in whom the menstrual discharge has ceased to appear.

[We have met with it just as frequently in the menstruating female as in those in whom the period of menstruation had ceased. Some females are troubled with it during pregnancy, but at no other period.—C.]

I have never had an opportunity of trying the *aconitine* in such cases. One local application which has been found very serviceable is the *yellow wash*, which, as you probably know, is a solution of corrosive sublimate in lime water, in the proportion of a drachm to a pint. A saturated solution of borax, first recommended, I believe, by an American physician, Dr. Dewees, has also much testimony in favour of its efficacy.

You will sometimes be consulted—at least I have been, on more than one occasion—about itching of the pubes and scrotum, produced by the presence of the pediculi that are vulgarly called crab-lice. The patients are sometimes quite unaware of the cause of the itching. You may relieve them by the wash I have just mentioned; or, by a more elegant lotion, made by dissolving corrosive sublimate in a little spirit, and adding rose water. A single washing with such a lotion will destroy the whole colony: and the vermin become much more visible after this violent death, turning black, and relaxing their hold upon the skin.

¹ Since the republication of these lectures in America, I have been favoured by Dr. Bowling, of Adairville, in Kentucky, with an account of a plan of treatment which he has found eminently successful against this most distressing malady, and which ought, therefore, to be made generally known. I extract that portion of Dr. Bowling's obliging letter which relates to this subject:—

"I have, in the last fifteen years, prescribed for a great number of cases of *prurigo senilis*, and I can say, with a most rigid adherence to truth, that I have not failed in a single instance to effect a permanent cure.

"I direct that the affected parts be sponged for a minute or so with good apple vinegar, and then be allowed time to dry. After this they are to be smeared over with the citrine ointment (*unguentum hydrargyri nitratis*). The applications are to be made twice a day. The cure is usually effected in a week. I have never known the constitutional effects of the mercury to be developed in this treatment, save in a single instance, and then but very slightly."

Prurigo is a convenient generic name for these cutaneous affections, of which the prominent feature is the teasing sensation that accompanies them. But, besides all these, there is a specific disorder, which, from the intensity of that sensation, is emphatically termed *the itch*, and which deserves a short notice; for it is exceedingly common, and exceedingly distressing, and (what is more interesting still) it is easy of cure. It is one of the very few complaints for which we possess a specific or infallible remedy.

Scabies, or the itch, is, as everybody knows, contagious; but it is contagious only in that particular sense which implies contact. It is not producible by any effluvia which the atmosphere can convey: it requires, for its propagation, that the healthy person should touch the diseased person, or some substance which has been in contact with his unhealthy skin. Certain parts of the skin are more liable to it than others. It is most common at the roots of the fingers and thumbs, between them, as it were; on the wrists; between the toes; in the flexures of the joints. It may spread to almost every part of the trunk or of the extremities; but all observers agree in stating that it is seldom or never seen upon the face and head; a curious but unaccountable exemption.

The eruption is at first papular and then vesicular, presenting a number of pointed watery heads. When the inflammation is aggravated by intemperate habits, or by the scratching from which the patient is unable to refrain, the vesicles are liable to be converted into pustules: and this has needlessly been made a separate *species* of itch, scabies *purulenta*, pocky itch: you see large pustules, filled with a yellow viscid matter, standing on an inflamed base. If you are not aware of these varieties and changes, you may make unlucky errors of diagnosis: affront your patient by telling him he has the itch when he has it not; or suffer him unconsciously to betray and shame himself by communicating it to others, when he has. You will easily understand how it has come to be considered a disgrace to have the itch: for it is fostered and propagated in most unfashionable places, amidst poverty, vulgarity, and filth. Yet the most delicate and high-bred lady may contract the distemper; and when once it is contracted, it will go on indefinitely, through life, unless proper means are adopted for its cure. It never gets well if left to itself.

The most curious point in this discreditable malady, is its connexion with a peculiar insect, called accordingly the *acarus scabiei*. The existence of this ectozoon had long been affirmed and denied; but the vexed question has at length been set at rest by the public demonstration of the acarus, by a M. Renucci, to a number of medical practitioners in Paris. It has since been often detected and exhibited here. Dr. Nevinsou assured me that he furnished Shaw the naturalist, Dr. Wollaston, and others, with living itch-mites for examination, more than forty years ago. It is described, and its form is depicted, in a letter written by Dr. Bonomo, and communicated to the Royal Society, by Dr. Mead, in the year 1703. One reason, probably, why it has often been searched for in vain is, that the acari are not equally numerous with the vesicles; there is not an insect for every vesicle. Another reason is, that the hunters have not known exactly where to look for the insect. It is not *in* the pustules or vesicles, but *near* them; at the extremity of a short, small, superficial tunnel or furrow which runs from them. A third reason why the insect had so long and so often escaped detection, is to be found in its minuteness. It is barely visible by the naked eye; but under the microscope it is seen to be a most formidable monster, in outline like a tortoise, and having eight legs. I show you here its portrait; not in little, but enormously magnified. I hope to procure for you the privilege of seeing the creature itself. The first that I can catch I will ask Professor Rymer Jones to show you by means of his microscope.

There is good reason for believing that this parasitic animal is, not merely a casual companion, but the veritable cause, of scabies. Various attempts have been made, and made in vain, to produce the disease by inoculation of the fluid from the vesicles. On the other hand, transportation of the acarus has always excited the eruption.

These facts explain how it is that the itch, though readily communicable by direct contact, or by fomites, is not communicable through the medium of the air; that fomites long retain the contagious property; and that the disease is curable by whatever destroys the acari. I believe that the complaint called *the mange*, in dogs,

camels, and sheep, has the same, or a similar origin : and I think it extremely probable that certain other varieties of prurigo or pruritus, in the human subject, may depend upon a like cause.

Now *sulphur* is as sure to cure the itch, as quina is to stop an ague. I presume that it kills the acarus ; but whatever may be its *modus operandi*, I have never known it fail to remove true scabies. It is applied externally ; and the only objections to its use are its disagreeable smell, and the dirtiness that belongs to ointments ; but these inconveniences are far outweighed by its certain efficacy. Although many substitutes for this substance have been recommended, I premit them all, and advise you to employ the sulphur ointment, of which you may disguise the smell by the addition of a little bergamot, and the colour by intermixing a small quantity of vermilion.

The ointment should be carefully rubbed all over the skin, at bed-time, and most especially on parts visibly affected with the eruption. The patient should sleep enveloped in a flannel dress. The rubbing should be repeated night and morning, and in two or three days the complaint will be subdued. Then, thorough ablution with soap and warm water, and the destruction of the contaminated clothes by fire, will complete the purifying process.

LECTURE XC.

Herpes ; Eczema ; Pompholix ; Lepra ; Psoriasis ; Impetigo ; Boils ; Carbuncle ; Purpura ; Scurvy. Conclusion of the Course.

If you look at the list of *genera* and *species* appended to the various works which treat exclusively of cutaneous diseases, you will find that they are exceedingly numerous. But these disorders differ widely in their relative importance : and the principles upon which their remedial management proceeds are not so greatly diversified as these "tables of contents" might lead you to suppose. I have spoken pretty fully of the most serious and interesting of these maladies — I mean of the febrile exanthemata ; but I have no time left for pursuing in detail the host of chronic affections to which the human skin is subject. Nor do I much regret this. To become expert in the diagnosis of these blemishes, and in curing such as are curable by our art, you must see them with your own eyes. Verbal descriptions of their changeful characters are of comparatively little service or interest. They are among the things that require to be "*oculis subjecta fidelibus*." Even pictured representations convey but an inadequate notion of the morbid appearances they are designed to portray. The lecturer on skin diseases should have actual patients before him, to whose bodies he could point.

In this, the final lecture of the course, I can do no more than offer you a few very cursory remarks upon some of the genera into which nosologists have distributed this class of disorders : and I may premise, that the treatment of the genus includes for the most part that of the species.

Vesicles are, as you know, small transparent elevations formed by a drop of aqueous fluid effused beneath the cuticle. Sometimes they are thinly scattered over the surface ; sometimes collected into clusters ; sometimes situated on a red patch of skin ; sometimes quite free from redness. The whole crop comes out at once in some cases ; in others the vesicles appear in irregular succession. They terminate also in various ways : by the reabsorption of the liquid, and slight desquamation ; by the giving way of the cuticle, and the formation of little scabs, under which new cuticle is generated ; and sometimes, though rarely, by ulceration.

Of the vesicular class of cutaneous disorders I have already described the three most important ; namely, cow-pox, chicken-pox, and the itch.

Another not uninteresting genus of this class is *herpes*. This is a transient non-contagious eruption, consisting of red patches of irregular form and variable size, upon each of which stands a crop of vesicles. The eruption runs a definite course; and its several periods—its beginning, its increase, its acme, and its decline—are completed, when its progress is not interfered with, in about ten days. These characters, once known, are easily recognized; and it is of importance that you should recognize them. Not that the disorder itself is of any great moment; nor that it is at all under the influence of remedies; but because it may be confounded with some graver malady. For example, *Herpes præputialis* is a very common and a very trifling species, affecting the foreskin; but it might readily be mistaken for the result of the poison of syphilis, and so cause much alarm and distress to the subject of it, and entail upon him perhaps a needless course of mercury, and bring unmerited suspicion upon the person with whom, whether lawfully or unlawfully, he might have been connected. It has nothing whatever to do with sexual intercourse; and it requires no treatment beyond cold ablation, and the interposition of a piece of lint between the prepuce and the glans penis. In like manner another species, *Herpes circinatus*, the vesicular ring-worm, is liable, when it appears upon the hairy scalp, to be mistaken for that pest of schools and of school-boys, the *favus confertus*, or *porrigo scutulata* of Willan, the common scald-head; a complaint which is naturally shunned as filthy, stubborn, and contagious: and which is in fact a vegetable parasite. *Herpes iris* is a mere curiosity. In this species each group of vesicles is surrounded by four concentric erythematous rings, of different shades of colour. The rings form and fade in succession, one after another, by a slight extension outwards of the inflammation each time. The spots constituting the eruption have been likened to small parti-coloured cockades. But the most singular species of all, and the only species of any serious importance, is the *Herpes zoster*, in which the separate patches lie in the direction of a band that encircles half the circumference of the body. Hence its names, *zoster*, *zona*, *zona ignea*, and in our vernacular tongue, *the shingles*; and even this Dr. Johnson derives from the Latin, *cingulum*, a girdle.

Most commonly the zone is confined to the trunk of the body; has a somewhat oblique direction like a sword belt, and occupies exactly one-half of the circle, lying between the *linea alba* and the spine, on one side only. Sometimes, however, it extends from the trunk to the limbs. Thus it may begin in the loins, pass obliquely across the flank, and terminate at the inner part of the thigh. Or it may commence from the upper part of the spine, cross the shoulder, and end on the arm or forearm. In the year 1833 I saw a lady affected with this strange eruption, in whom the clusters of vesicles began near the spine in the neck, passed over the scapula, then to the shoulder and axilla, whence the main line ran along the outer side of the upper arm till it reached the elbow, where it turned inwards, followed the inner side of the forearm, went across the palm of the hand, and terminated by two or three patches upon the palmar and inner side of the ring finger. Very rarely indeed it appears on the limbs only. Twice I have seen it limited to the thigh and leg, and in both cases its track corresponded with the course of the sciatic nerve. Thrice I have known it spread from the neck, behind, up into the hairy scalp: and in one of these instances a patch fell upon the conjunctiva of the right eye, of which the vision was for some time in jeopardy. The most common situation of the demi-cincture is across the base of the thorax. It is a curious feature of this curious disorder, that, in nineteen cases out of twenty, according to Bielt, it occupies the *right* half of the body. Of this singular preference of the right side, if indeed it be a general fact, I can give you no explanation. I have seen fifteen cases of the complaint since I began to attend to that circumstance, and in ten of these the eruption was on the right side.¹ Rayer, in the first edition of his book, said that eight cases in ten would be found to be on that side; but a longer experience has reduced that proportion. Of fifty-three examples seen by himself, thirty-seven only were on the right, and sixteen on the left. Reil states that he has *always* observed it on the left half of the body; and Mehlis, among twenty-five patients, counted sixteen in whom the left side was affected. This statistical point remains therefore to be settled, if it be worth settling, by a larger induction of

¹ Since this was written many more cases of shingles have come under my notice, but I am sorry to say that I have mislaid the memoranda which I had made of the positions of the patches.

particular cases. The zone seldom transgresses the median line at either extremity, unless perhaps the redness of the extreme patches may extend a little further. It is said, however, in some exceedingly rare instances, to complete the circuit of the body. There is a vulgar but erroneous notion, that the eruption proves fatal when it thus encircles the whole of the trunk; and this notion is as old as the time of Pliny, who says, "*Zoster appellatur, et enecat si cinxerit.*"

The most important, because the most distressful, of the symptoms, is an intense darting pain, described by the patients as being deep-seated, very acute, and shooting through the chest. Fortunately, however, this is by no means a common incident. At least so I formerly thought, and such is the tenor of recorded experience. Yet within the last three years these sufferings have occurred in five or six of my own patients. Sometimes the pain precedes the eruption; more often it accompanies it; and it is apt to last, in spite of remedies, for some time after the eruption has disappeared. Mr. North tells me that, in a female patient of his, this pain continued to be severe and intractable for eighteen weeks. In two instances I have myself known it last for two years; and in one of the two its severity had scarcely abated in that long time. That this is, however, a rare complication of the disorder, we may conclude from the experience of M. Biett, who never once witnessed this symptom in more than 500 cases of shingles. The severe and intermitting character of the pain, and the peculiar direction of the row of herpetic patches, lead to the belief that the whole malady may arise from some fault in the nervous system. I may mention some other curious circumstances which seem corroborative of this belief. One of the three patients in whom the scalp was affected with the herpetic patches, had been plagued for seven years with continual noises in his head. Upon the breaking out of the eruption these noises ceased; and remained absent for a year and a half: then they returned. Another person who had an attack of shingles in February, suddenly lost a cough which had teased him all the previous winter. In July, 1855, I was asked to see an elderly maiden lady, who in a former part of her life had had ague, and neuralgia. Shingles had appeared on the left side of her body two months previously to my visit. A demi-cincture of patches, which almost touched each other, extended from one mesial line to the other. But besides this row, there were scattered spots of herpes, not linear in their direction, on the right side of her body, on the shoulder, in the arm-pit, and one on the right thigh. She had suffered and was suffering intense neuralgic pains in the track of the belt, just along the edge of the left ribs. Any cold fluid taken into her stomach would excite the pain at any time; but it often occurred spontaneously. The right half of this patient's body was sometimes warm and perspiring, while the left was chilly, shivering, and affected with horripilation. The neuralgic pain had somewhat of a periodic character. On one occasion it was banished for twenty-four hours by half a scruple of quinia. But the quinia so disturbed her head that she would not repeat it, although various other remedies had been tried in vain.

Of the cause of herpes zoster we have no certain knowledge. It is said to attack young persons more especially, and those who have fine and delicate skins, and the male more frequently than the female sex. But I suspect that these assertions rest on a very loose foundation. Of the fifteen cases already mentioned, ten occurred in females. One of the patients was a child two years and seven months old; another was an aged man of about seventy-five. In several instances I have found upon inquiry, that the patients, being children, were in the nightly habit of wetting their beds. Whether this has been any thing more than a casual coincidence, I do not know; but my attention was first directed that way, some years ago, by Mr. Wheeler, the apothecary at St. Bartholomew's Hospital, who told me that he had often noticed the same circumstance. According to Bateman, the disorder "seems occasionally to arise from exposure to cold after violent exercise. Sometimes it has appeared critical, when supervening on bowel complaints. Like erysipelas, it has been ascribed by some authors to paroxysms of anger." Schwartz saw three cases which followed violent fits of passion; and Plenck affirms that he had known it occur twice after furious anger — and a copious potation of beer.

The duration of the eruption is from ten days to a fortnight; but it is liable to be considerably prolonged by troublesome ulceration, whenever the vesicles and crusts are prematurely chafed off by friction or pressure.

Very little, as you must perceive, can be done, or is requisite, in the way of treatment. The patient is to be cautioned against rubbing off the heads of the vesicles. Attention should of course be paid to the state of the stomach and bowels; and the diet should be regulated. Our main business is to look on, and to endeavour to set right whatever function may be manifestly wrong.

Should the eruption be attended or followed by the intense shooting pain which sometimes, but not very often, harasses the patient, it will be right to apply opiates, by friction, over the affected region. I would use the aconite ointment in such a case. Warm baths will also be proper; and as the pain is probably neuralgic, the carbonate of iron is a remedy which ought to be tried.

Eczema is another genus of the vesicular class of diseases. It is characterized, in its commencement, by an eruption of very minute vesicles, scarcely prominent, closely crowded together, and requiring a microscope sometimes to render them distinctly visible. They terminate either by the reabsorption of the fluid they contained, or by the formation of superficial moist exoriations. *Eczema* is not contagious.

There are several species or varieties of this form of cutaneous disorder also. It is sometimes produced by great heat, and particularly by the heat of the sun; and this is named *eczema solare*, *heat-spot*; sometimes by the contact of irritating substances with the skin, as in what is vulgarly called the grocer's itch, affecting the hands of those who are much conversant with sugar. *Eczema* often occurs upon the scalp, and constitutes, I believe, the most frequent form of what is commonly named scald-head, *porrigo*, or *tinea capitis*. But the most severe of all its species is that which has received the names of *Hydrargyrium*, *Erythema mercuriale*, and *Eczema rubrum mercuriale*. This, as these names imply, is an occasional consequence of mercury; an unusual consequence, no doubt, and one that happens only in a few peculiar constitutions; but you ought to know it, in case it should follow the use of mercury prescribed by yourselves.

The eruption begins usually in the groins and upon the thighs. It is at first red, and is accompanied by much heat and itching. It soon extends, in the severer cases, over the whole body; and an innumerable multitude of very minute glittering vesicles may be seen, with the aid of a magnifying glass, from the beginning. Like that of erysipelas, the eruption is attended with a good deal of swelling. The intumescence of the face is such as to close up the eyes: and the disorder *becomes* febrile, in its course; for there is seldom much fever at the onset. The vesicles increase in size, turn milky, burst, and pour forth an acrid exudation, that irritates and inflames the skin with which it comes in contact, and thus increases the local complaint. The distress and worry occasioned to the patient by the foetid smell of the discharge, by the stiffening which it causes of his body-linen, and by the heat and itching, are, I conceive, the main causes of the febrile disturbance. The discharged matter is apt to become thick and hard, and to present the appearance of large scabs: and in this state the nature of the disease may very easily be misunderstood, it being impossible to say, when it is seen for the first time under these circumstances, whether it was originally vesicular or not.

The duration of this harassing distemper is variable. It may be over in a fortnight, or it may last several weeks. It terminates by the cessation of the discharge, and then the cuticle detaches itself in large flakes. Sometimes in this disease also the epidermis falls entire from the hand, like a glove.

Without being dangerous to life, this disorder is apt to be obstinate. It is not much within the control of remedies. What little can be done is chiefly palliative. The mildest local applications must be used: tepid water, barley-water, strained gruel. The warm-bath, when circumstances permit. Poultices are sometimes of much service, in preventing the hardening of the matter that exudes, and so obviating one source of irritation. Flour, or powdered charcoal, may be sprinkled over the eruption in the slighter and early cases, for the purpose of absorbing the discharge. Equal parts of olive-oil and lime-water make a soothing liniment, which may be applied by means of a feather. The local remedies may fairly be varied, for sometimes one is found to give relief, and sometimes another. The patient's linen must be frequently changed, especially whenever it becomes stiff and hard with the exudation.

With respect to the general treatment, opiates to procure rest, and to allay irri-

tation, are probably indispensable. The bowels must be kept moderately open, but no severe purging should be employed, for the patient must at all events undergo a long and weakening process, and therefore it must be our care that the *treatment* be as little weakening as possible. In protracted cases, where there is much exhaustion, wine may with propriety be given; and, almost always, good strong broths. As the disorder declines, some of the reputed tonics may be prescribed; the mineral acids, quina, sarsaparilla.

I need not say that, in such cases, you must be scrupulous in seeing that no more mercury be administered or applied.

Eczema, spontaneous in its origin, is a very common, and a very teasing complaint among children. Mr. Erasmus Wilson, after large experience in treating it, expresses his confident belief that almost every case of *eczema infantile* admits of a ready cure. His method is to give calomel at moderate intervals, with the view of clearing and regulating the digestive organs. This preparatory treatment is followed by the administration of arsenic in small doses, as a tonic, internally, and by the external use of well-prepared oxide of zinc ointment.

In many children thus tormented with eczema, I have remarked that the rapid departure, or even the sudden diminution, of the eczematous eruption has been immediately succeeded by wheezing in the chest, and oppressed breathing; which in their turn have subsided upon the re-appearance of the eruption. This alternating affection of the tegumentary membrane and of the mucous membrane of the air passages, has sometimes made me dread the curing, so earnestly desired by mothers, of the unseemly disorder of the skin. Mr. Wilson believes that no such dread need be entertained, when the cure has been preceded by a due course of calomel.

A separate class of cutaneous diseases, very analogous, however, to that which we have been describing, is the class of *bullæ*, or blebs. Anatomically speaking, there is but little difference between the two: *bullæ* are *large vesicles*. When the eruption is at its height, it is composed of hemispherical prominences of various sizes, from that of a pea to that of a hen's egg, and having the shape and appearance of the bubbles raised in a pool of water by a hard shower of rain. They are formed by the effusion of a serous, or a sero-puriform fluid, between the true skin and the cuticle. You can only be sure of the diagnosis when you see the eruption in this stage of its progress.

The best example of this class is that which is called by some writers *Pemphigus*, by others *Pompholix*.

It is characterized by the presence of *bullæ*, varying in their magnitude, commonly distinct, but numerous, springing up in successive crops, on one or more parts of the surface. At first these *bullæ* are nearly transparent, and contain a thin limpid serum; but they become gradually opaque, pearl-coloured, and ultimately many of them acquire a reddish tinge.

Pemphigus has been described as being sometimes acute, sometimes chronic. The acute form is attended with smart fever, the *bullæ* rise spontaneously or in quick succession, run their course, and disappear; and then the disease is over. This is a very rare form. In general the *bullæ* continue to come out; the complaint is spread over weeks, or months, or years; and it is accompanied by little or no febrile reaction. This, on the other hand, is a common form of disease. It is the *Pompholix diutinus* of Willan and Bateman. The eruption often occupies all parts of the body at the same time, or in succession: in other cases it is confined to a limited space. I have most frequently seen it on the fore-arms and legs. When the *bullæ* are very numerous, they may give rise to some febrile symptoms, but not else. The complaint may be indefinitely prolonged by successive crops.

The eruption begins in small red points, the formation of which is attended with a slight pricking sensation. Some patients have likened this sensation to that which accompanies the passage of the electric spark. In the centre of each of these spots, the cuticle becomes lifted, while the circumference of the spot enlarges, so that *bullæ* are rapidly formed, often in the space of a few hours only, as big as a hazel-nut, or a walnut: or the blebs may even be much greater than that. Either in consequence of their distension, or of the pressure made upon them by the movements of the patient, some of these *bullæ* burst, and a straw-coloured serum exudes. Then the

epidermis collapses into folds and wrinkles; or if it be detached at a part of the margin of the bulla, it is rolled back, so as to expose a portion of the red, painful, and smarting surface beneath it. Towards the third or fourth day, when the bullæ lose their transparency, and the liquid they contain becomes reddish, those bullæ which have not been broken sink down and wither; the cuticle is no longer stretched; but, sodden by the serous fluid, it assumes a whitish hue, becomes opaque, and forms at length small brownish flat crusts, of no great thickness.

In the meantime fresh bullæ appear by the side of the former ones, and pursue the same course; so that generally you may see in the same person, tense bullæ containing a transparent and yellowish serum; thin crusts; and irregular patches of various sizes, slightly excoriated.

This is the ordinary course of chronic pemphigus: and I say it may thus go on for months or years.

The disease is most frequently observed in persons of debilitated habit. It is sometimes apparently the result of intemperance; or of the use of bad or insufficient food. In Biett's experience it has often been coincident with the fatty liver. When the disorder is chronic and uncomplicated, the treatment found useful is such as we might expect benefit from, knowing the constitutions in which the disease is most apt to occur, and the causes which seem at least to favour its occurrence, if they do not produce it: regulation of the bowels; good nourishing food; tonic medicines, bitters, and especially quina and the mineral acids. Biett declares this kind of treatment to have been very successful in St. Louis; and that, not only in old and worn-out subjects, but also in the young, especially when the complaint has been chronic. Bateman recommends the same general plan.

Local applications have seldom been much employed, except some mild ointment to the excoriated parts. Biett advises emollient lotions, or even opiate washes when much irritation exists; but a case recorded in the *Medical Gazette*, by Dr. Graves, of Dublin, affords a remarkable instance of a cure by local applications alone: and it is a case worth recollecting, although, as he justly remarks, we ought not to generalize from a single instance.

His patient was a boy, fourteen years old, of slender frame and delicate constitution, yet enjoying uninterrupted health, except the cutaneous disease, which had lasted five years. During that time the succession of bullæ had seldom ceased. The bullæ were very numerous, occupying not merely the face and extremities, but the trunk also: and they were in various stages of their progress, some healing after having burst, some of a larger size and unbroken, others small, and recent.

Dr. Graves observes, that from the descriptions of Bateman, and of Biett, although both authors describe it correctly, we should scarcely form a notion of the occasional severity of this disorder. He had seen two examples of it in young men, where the irritation and suffering produced by the constant exposure of large portions of skin denuded of epidermis, had operated most unfavourably on the general health, almost banishing sleep, and reducing the patients to a state of great debility. These cases did not yield to the methods of treatment recommended by authors; and, therefore, Dr. Graves determined, whenever another opportunity should occur, to have recourse to a new plan.

In the boy in question, therefore, he had all the bullæ opened with a lancet, and the denuded surface of the corium was then touched with a stick of lunar caustic. The nitrate of silver was also applied to the skin around each bulla, for the breadth of a line; and the recent pimples, which indicated the formation of future bullæ, were all treated in the same way. The boy was then washed, and supplied with clean linen.

This single application of the nitrate of silver had not merely the effect of entirely destroying the morbid action in the portions of the skin which were at the time affected, but (what was very remarkable) no fresh bullæ made their appearance afterwards: none at least had appeared for four months, when Dr. Graves wrote his account. The only part where a repetition of the process was required, was the palm of the hand, where the thickness of the cuticle rendered it more difficult to expose the diseased surface of the cutis to the full action of the caustic.

It might strike you from this cure of a long standing disorder, so readily, by mere local means, that the disease propagated itself from one part of the surface to an-

other, by a sort of re-inoculation. But it has been fully proved that the disease is not contagious. A Mr. Gaitskill engrafted himself, with impunity, with the fluid; and analyzed it, and found it apparently like the thin serum of hydrocephalus. Dr. Graves, therefore, supposes that the cure was owing to the simultaneous destruction of all the parts of the skin that were in a state of morbid action: a morbid action which would have been otherwise propagated to other portions of the surface, by what is called the sympathy of *continuity*.

The class of scaly eruptions—the *squamæ*—is distinguished by the occurrence of red spots or blotches, upon which laminæ of altered cuticle form, and are thrown off, and constantly renewed. You will perceive that, anatomically, this class of cutaneous disorders has a close analogy with the rashes; and yet it is separated from them by very obvious particulars. In the exanthemata of Willan and Bateman, the redness is *followed* by desquamation; in the *squamæ* these two appearances co-exist: in the exanthemata the sequence of redness and desquamation takes place, in general, once only; in the *squamæ* the morbid cuticle continues for an indefinite time to scale off again and again, in successive fragments, from the abiding red patch of skin.

Lepra, psoriasis, and pityriasis, and some syphilitic eruptions, constitute the principal of the squamous affections.

Lepra is a very common disorder of this class; hence its name, *lepra vulgaris*. It consists in red scaly patches, of various dimensions, but always affecting a circular or elliptical shape, and scattered over different parts of the body. It commonly begins on the limbs, most usually near the joints; just below the knees, or the elbows; and Dr. William Budd has pointed out the curious fact that these patches, especially when they are few, and the disease is recent, are distributed symmetrically, each spot on the one limb answering in situation to a similar spot on the fellow limb. This shows that the disease is a blood disease; that it depends upon some poison, introduced from without, or, more probably, bred within the body. By degrees the patches both enlarge in size, and multiply in number, and extend along the extremities to the trunk. The eruption is seldom seen upon the hairy scalp, or upon the hands. As the patches enlarge they sometimes become confluent; but even then, the outline of the confluent scaly space is defined by arcs of circles, and the disorder is sufficiently distinguishable from *psoriasis*. It is not easy to set these things before you in mere verbal description. To have *seen* lepra once, is to know it for ever.

When the patches begin to get well, the restoration of the altered surface to its natural condition and appearance commences in the centre—*i. e.* in the spot first affected—and proceeds outwardly towards the circumference: so that the scaly redness assumes a ring-like arrangement. This ring becomes gradually narrower and narrower; at length its continuity is here and there broken; and at last it vanishes entirely.

The eruption does not, however, run any definite course. Sometimes it goes rapidly through its phases; in other cases it persists for a very long period. It is not at all contagious.

Neither is lepra attended, in general, with much local inconvenience, nor with much constitutional disturbance. When the eruption is very copious and extensive, and especially when it is plentiful or almost continuous around the larger joints, it renders the movements of the limbs stiff and difficult; and even sometimes painful, from the cracking of the inflamed surface as it is stretched in the bending of the joint.

But I have seldom found lepra to exist *unconnected* with some disorder of the digestive organs. Usually the connexion is that of alternation, and not of coexistence. The patient is dyspeptic till the eruption comes out, and then the dyspepsia is relieved: and it often returns as the leprous patches disappear. The eruption is the more unsightly; the dyspepsia is the more troublesome. This alternation would seem to mark the shifting location of the *materies morbi*.

When the patches are small, and chronic, and white, that variety is no longer called *lepra vulgaris*, but *lepra alphoides*; and there certainly is another distinct variety, of a more blue, or livid, or copper colour than the ordinary; and a result of the poison of syphilis. It is named accordingly syphilitic lepra. This species will

get well under the influence of mercury; which, so far as my observations go, does not cure the others.

Psoriasis is closely allied to *lepra*. When it occurs in distinct patches it is often difficult to say to which genus the eruption belongs. In general the patches of *psoriasis* are not so broad as those of *lepra*; their edges are less raised, and their centres less depressed; the scales adhere more firmly; and the patches are less uniform and less circular.

But, *psoriasis* frequently spreads itself over large portions of the skin, and it may come to occupy nearly the whole surface of the body. It is then called *psoriasis diffusa*. It often renders the patient hideous to look at. The scaly incrustation is interspersed with chaps, furrowing the skin in all directions, and following particularly its natural folds and angles. These cracks, when the skin is put upon the stretch by the movements of the patient, are apt to bleed. In these severer cases (which are said to be examples of *psoriasis inveterata*) the laminae of altered cuticle are thick, and very abundant. They fall off perpetually, or are rubbed off, and may be shaken from the patient's clothes, or collected in handfuls from his bed.

Both these scaly disorders, *lepra* and *psoriasis*, require the same kind of treatment.

I believe that external applications are of but little use. I have tried a good many, and have lost all confidence in them, with the exception of the warm bath. Whatever tends to improve the general health, will hasten the departure of these eruptions. I believe that they sometimes depend upon the presence, or the generation, of an excess of acid in the system; and that they are often to be cured by alkaline remedies I am sure. I have seen many cases of *psoriasis* rapidly improve, and get ultimately well, under full doses of the *liquor potassæ*; from half a drachm to a drachm, three or four times daily, in a glass of milk, or of water, or of beer, or of ginger tea. Another internal remedy from which I have seen manifest improvement result, is arsenic; given with the cautious, and in the doses, which I have more than once spoken of. These are the two remedies of which I have the most experience; but neither of them is infallible; and you will have to try many things in succession, for patients are very desirous of getting rid of the disfiguring eruption, even when it does not interfere with their health or comfort. Now the Harrowgate waters, a strong decoction of *dulcamara*, pitch-pills (and if pitch-pills, I should suppose *à fortiori* creasote), tincture of *cantharides*, and the iodide of potassium, are remedies of some renown for these scaly diseases. Of the syphilitic *lepra* I repeat that mercury will prove a cure. In all cases the diet must be regulated, and all kinds of stimulating food abstained from. Dr. Bateman knew a man who was always attacked with *lepra* if he took spices with his food, or drank ardent spirits: and a patient of my own got rid of long-standing and very troublesome *psoriasis* of the scrotum, upon adopting, for other reasons, a very abstemious and simple mode of living.

Parts of the surface of the body—the chest, the neck, the shoulders, the abdomen, even the forehead—are marked sometimes by irregular brown patches of what is called *Pityriasis*; from *πυρρον*, bran. The discoloured portions are in fact covered with small bran-like films, which fall off, and are succeeded by others. I call your attention to one variety of *pityriasis*, the *pityriasis versicolor*, chiefly because it is (like the *favus confertus* or scald-head) a sample of the *vegetable parasites* with which the human integuments are liable to be defaced. Viewed through a microscope, these bran-like scales present the spores and filaments of a minute cryptogamous plant or fungus, the *microsporum furfurans*. *Pityriasis* is an eyesore or blemish rather than a disease: but it sometimes excites the apprehension of some syphilitic or other constitutional taint. Whether the skin upon which this fungus clings and grows must be in an unhealthy state to admit of its first invasion, has not been learned. Drugs are often diligently administered to cure the disfigurement: but I suspect that they are always useless and superfluous. Some years ago, before I was aware of the true character of these blotches, I tried various means, in vain, to remove a large one from the neck of a young lady whose beauty it was marring. At length it yielded at once to a couple of sulphur baths. The medication must be external. A saturated solution of sulphurous acid gas in water is an effectual remedy; or the parasitic plant may be killed and dislodged by a wash containing corrosive sublimate.

Among the *pustular* diseases of the skin there is one which assumes many forms,

and is termed *impetigo*. Whatever may be the minuter peculiarities of this eruption, its general characters are the following. It consists of crops of pustules, sometimes scattered irregularly, sometimes collected into groups. The pustules burst, or are broken, dry up, and scab over. The crusts are yellowish, and very friable, and resemble in appearance little masses of candied honey; or sometimes they look like small pieces of dirty plaster. From beneath these crusts a considerable discharge continues to take place; the crusts become thicker and larger, and around their margins the skin is red and raw, as it is also beneath them.

We have, I say, various forms of this complaint; *impetigo figurata*, *impetigo sparsa*, &c. It often borders closely on eczema, so that authors describe an eczema *impetiginodes*, or an *impetigo eczematodes*. These varieties are delineated by Rayer, by Willan and Bateman, and by others; and knowing their characters, you can examine and study their appearances for yourselves. *Impetigo* is a non-contagious disorder.

Sometimes this complaint occurs in an acute form, and is attended with fever. In such cases its removal will be accelerated by moderate bloodletting; and the blood drawn will be found to present the buffy coat. Whatever local applications are made should not be unctuous. It is seldom that *impetigo* will bear, or be the better for, ointments. Purgatives and alkalies internally, and very weak spirit or alkaline lotions externally, with a scrupulous diet, constitute, I believe, its best treatment. When the complaint is chronic, and the discharge copious, the oxide of zinc has often a very beneficial effect. It may be dusted over the affected surface, from a thin muslin bag; or it may be applied in the shape of a lotion—fifteen grains to an ounce of rose-water. You will find this a most useful lotion for that disfiguring impetiginous or eczematous eruption which sometimes covers the faces of children like a mask, and is called *crusta lactea*. The phrase *crusta lactea* is, however, very loosely employed by medical men.

There is a very common, and a very teasing pustular disease of the skin, usually called a *boil*, in some parts of England a *push*, and by the learned *furunculus*.

First, there is a slight degree and extent of hardness to be felt, a tender knot, just beneath the surface, which soon begins to look red, and a small swelling arises, which gradually increases up to a certain size, that of a large pea, or of a hazel-nut, or of a walnut. The tumour is painful, and undergoes a process of slow suppuration. Some time from the fourth to the eighth day it acquires a conical or pointed form, and its apex becomes of a white or yellow colour. At last the cuticle gives way, and the patient begins to congratulate himself that the little abscess is ripe, and that his troubles are nearly ended. But he is disappointed; an insignificant quantity of pus mixed with blood escapes, and leaves visible a mass of dead areolar tissue—a *core*, as it is called—of greater diameter than the opening, which is commonly small. At last, two or three days perhaps after this, the slough is expelled, in company with more pus, and a deep cup-like cavity remains, which soon, however, fills up, and the boil is really over.

These little phlegmons frequent the buttocks, the thighs, the arm-pits, the nape of the neck, the abdomen. They may occur almost anywhere. They are apt to come in crops, or in a series: and any kind of irritation suffices to cause them when a constitutional tendency to their formation exists. I have known a piece of soap plaster applied to the skin give occasion to a long succession of boils. Poultices, applied to promote the suppuration of any existing furunculus, are believed to encourage, by their warmth, the growth of others around it. In truth, these phlegmons belong primarily and essentially to the subcutaneous areolar tissue rather than to the skin. Dr. Prout corroborates the statement of Cheselden, that they are often accompanied by a saccharine condition of the urine. You know probably that, in Dr. Prout's theory of assimilation, the areolar tissue represents the saccharine element.

Boils have been very prevalent for some few years past in this country: and not here only, but on the continent of Europe also, and throughout America. It is stated—by an anonymous contributor to the *Medical Times* (December 2, 1854), who founds his conclusions upon the observation of several hundred cases among the out-patients of St. Bartholomew's Hospital, and of the Hospital for Skin Diseases—that the number of males affected with boils in a given time is rather more than double the number of females: also, generally, that boils are twice as common during the first four months, as during other parts of the year.

The individual boils are intractable: the state of system which engenders them, or

which favours their formation, may often, I believe, be corrected. Some dab them, when nascent, with a solution of corrosive sublimate in spirit; some support them with sticking-plaster; some paint them with the compound tincture of iodine; others apply poultices, or what is better, and perhaps the best local application, lint wetted with water and covered with oiled silk; and others again cut the hard tumour through, while it is yet crude. Do what you will, you can seldom prevent or accelerate their deliberate course; but I believe that by applying leeches, or cold, you may prolong, though you cannot arrest that course.

From the time of John Hunter, who cured himself of a disposition to boils by taking "the fossil caustic alkali, night and morning, in milk for two months," it has been the fashion to prescribe alkalies for persons so troubled; particularly the liquor potassæ, in combination with sarsaparilla: but I have satisfied myself that a better remedy — of opposite chemical quality — is to be found in the dilute sulphuric acid. For some three years, acting upon a hint received from Dr. Bullar, of Southampton, I have given this acid, twice daily, before meals, in doses of ten or fifteen minims, to a great number of persons who were infested with boils, and the instances have been very few in which it has failed to check the tendency to their formation. When the system is below par, the sulphate of quina, and a generous regimen, may be added: meanwhile sugar, and saccharine food of all kinds should be scrupulously avoided.

Carbuncle, *alias* anthrax, is a gigantic boil, and something more. It constitutes a far more serious disorder than the common furuncle, not only in respect of its magnitude, and of the amount of suffering which it occasions, but also on account of the constitutional vice that it betokens. A carbuncle is a large, flat, circumscribed, very hard, and very painful tumour, of a purplish red colour, and attended with a sensation of burning heat. Its ultimate diameter may be three or four inches or more. It ends in the formation of a deep slough, of more than corresponding dimensions, and the destruction of the skin above it. A number of pinhole openings at length present themselves on the dark red surface, and disclose the immense core beneath.

That carbuncles and boils are *kindred* disorders appears distinctly from this — that occasionally a carbuncle results from the confluence of two or three boils which had arisen near each other: and not less distinctly from the simultaneous prevalency of the two. The recent increase of the carbuncular disease has been even more marked than that of the furuncular. Of this I may offer you proof from the records of the Registrar-General. These records deal, indeed, with fatal instances only. Of course the whole number of cases that occurred in the same periods must have been far greater. In the five years ending with 1845; the average number of deaths from carbuncle in London alone was 5; in the next five years, ending with 1850, the number increased to 14: in 1851, it amounted to 19: in 1852, to 50: in 1853, to 70: and in 1854, to 89. In this year, which you will recollect was the cholera year, the deaths from carbuncle in England, exclusive of London, were no fewer than 300.

The cause of this vast increase of these disorders has not, I think, been ascertained. Professor Laycock indeed imputes it to their *contagious* properties — classing together boils, carbuncles, whitlow, and the charbon and pustule maligne of the French, which are less frequently seen in this country, under the common title of the *contagious furunculoid*. He suggests the question whether that disease may not have had an epizootic origin, and whether its present wider and wider diffusion may not be derived from the imported hair and hides of animals affected with a carbuncular distemper, which has been epidemic among cattle in the South of France, Italy, Germany, Poland, Hungary, and Russia. I am bound to tell you that, in my judgment, this alleged quality of contagiousness, with respect at least to the two forms of disease with which we are most familiar here, the boil and the carbuncle, is "not proven."

Carbuncle is met with chiefly in advanced life, in corpulent males, and in persons who have lived fully: chiefly, but not exclusively. The writer in the *Medical Times*, to whom I have already referred, gives a tabular account of 35 cases of carbuncle, of which 25 were noted, within six months, among the patients at St Bartholomew's Hospital. Instances of it occurred at various period of life from 15

years of age to 80; among the ill-fed and the well-fed, the temperate and the intemperate, and more than twice as often in males as in females. Though the carbuncles may appear in almost any place, they most commonly affect the more brawny portions of the skin, and the hinder parts of the body; the nape of the neck, the shoulders, the buttocks. I have, however myself seen a large carbuncle on the belly. A virulent form, resembling the pustule maligne of the French, has been described as occurring upon the face, at St. Bartholomew's, by Dr. Harvey Ludlow. Carbuncle is then perhaps most dangerous, though not necessarily fatal, when it attacks the scalp.

The local disease is productive of high constitutional disturbance and irritation. Surgeons are in the habit of dividing the firm mass into quarters, by deep crucial incisions. This is a sharp remedy, but it purchases speedy ease, by removing that tension of the inflamed parts whereupon the pain chiefly depends. I am persuaded, however, that this severe operation has been done too indiscriminately. When there is no evident tension, when there is not much complaint of pain, and the inflammation is not extending, you had better, in my opinion, leave these tumours to the care of nature, and address your remedies to the system at large. Support is almost always needed; and opiates are sometimes indispensable; and the bowels must be kept clear by purgatives. When the interference of surgery is requisite, Mr. Travers, Junr., advocates the destruction of the central integuments by caustic, rather than their division crosswise by the knife; for this, among other reasons, that less hazard is so incurred of subsequent phlebitis.

I shall not attempt to discuss, even in this cursory and disjointed manner, any more of the inflammatory affections of the skin, whether acute or chronic: but I wish, before I conclude, to direct your attention to a peculiar morbid condition, of much greater consequence and interest than many of those which I have just been describing. I mean the malady which is best known by the appellation of *purpura*, or the purples, and which usually, though it must be confessed very incorrectly, is ranked among cutaneous disorders. It is strictly a hæmorrhage. Its external phenomena are so obvious, and so well known, that I need not dwell upon them. Small round spots appear on various parts of the surface, generally upon the legs first and most plentifully, of a dull crimson, or of a deep purple colour. They are accompanied by no local pain, by no sensation of any kind. Pressure upon them does not efface the colour, nor render it fainter, as it does that of inflammatory spots of the skin. There is scarcely ever any prominence of the purple stigmata; but they are sometimes intermixed with livid blotches, with appearances exactly resembling bruises: and both the circular spots and the ill-defined vibices undergo, before they disappear, the same changes of colour, from red or blue to a greenish yellow, which a bruise undergoes. In fact the anatomical condition of a bruise is exactly the same with the condition of the diffused livid blotches of purpura. In each case the colour is the result of ecchymosis. With all this, passive hæmorrhages from various parts, and particularly from the mucous membranes, are common.

It is clear, therefore, that this complaint cannot be regarded as a cutaneous complaint, even in the loose sense in which that epithet is sometimes applied to affections which are really *beneath* the skin, but visible *through* it. The hæmorrhage takes the form of red or purple spots when the quantity of blood extravasated in the same place is only a drop. And the spots are not peculiar to the skin, nor to the subcutaneous tissues, but are found, occasionally, upon all the internal surfaces also, and within the substance of the several viscera. I have seen these purple spots on the mucous surface of the mouth, the throat, the stomach, and the intestines, on the pleuræ and pericardium in the chest, on the peritoneal investment of the abdominal organs, in the substance of the muscles, and even upon the membranes of the brain, and in the sheaths of the larger nerves; and I have known them to be accompanied with large extravasations of blood in most of the vital organs of the body.

The superficial markings of purpura, the red and purple spots and livid blotches, exactly resemble the spots and bruise-like stains which characterize sea-scurvy: and I confess that I formerly regarded the two affections as being identical, or as mere varieties of the same disorder. But it is not so. For a very full and interesting account of scurvy, I must refer you to an essay, by Dr. Budd, in the *Library of Practical Medicine*. He has there collected from various sources, and exhibited in

a clear light, convincing evidence that scurvy is caused — neither by contagion, nor by cold weather, nor by impurity of the air, nor by the continued use of salt provisions, all of which have been alleged as sources of the disease, but — by the privation, for a considerable length of time, of fresh succulent vegetables. Now purpura often makes its appearance when there has been no deficiency of such food, and no remarkable abstinence from it. Scurvy is most common in winter, or in the beginning of spring; purpura in the fruit seasons, in summer and autumn. In scurvy the gums are uniformly soft, and swelled, and spongy, and bleed readily; this is no necessary feature in purpura. Scurvy is marked by extreme debility and dejection of spirits; it is always rendered worse by blood-letting and by mercury; and it is infallibly and rapidly cured by the administration of lemon-juice, or of other fresh fruits and vegetables. Purpura, on the other hand, often requires venæsection for its cure; it is not constantly nor surely, if ever, benefitted by the antiscorbutic juices; it is not always attended by sponginess of the gums, nor by feebleness of the mind and body; and I have seen it clear speedily away upon the supervention of mercurial salivation, and hypercatharsis.

Lemon-juice is really a specific against scurvy, whether it be employed as a preventive or as a remedy. It supplies something to the blood which is essential to its healthy properties. Its virtues were known in this country more than two hundred years ago, as appears from the work entitled *The Surgeon's Mate, or Military and Domestic Medicine*, by John Woodall, Master in Surgery: London, 1636. But the merit of making the fact generally known, and of procuring the systematic introduction of lemon-juice into nautical diet, by an order from the Admiralty, is due to Dr. Blair, and Sir Gilbert Blane, in their capacity of Commissioners of the Board for Sick and Wounded Seamen, in 1795. "The effect (says Sir John Herschel) of this wise measure may be estimated from the following facts. In 1780 the number of cases of scurvy received into Haslar Hospital was 1457: in 1806 *one* only, and in 1807 *one*." He adds, "there are now many surgeons in the navy who have never seen the disease."

Dr. Budd, however, has assured me that the Dreadnought Hospital-ship, at Greenwich, is often full of cases of scurvy; most of the patients so affected having just arrived in *merchant-ships*, from a long voyage. This surely ought not to be. It could not be if the owners of these vessels knew how easily, surely, and cheaply, this truly dreadful scourge may be averted.

Scarcely less — if indeed less at all — of antiscorbutic virtue, belongs, fortunately, to that common esculent root, the potatoe. *Raw* potatoes have long been in good repute, both for the cure and for the prevention of scurvy: but raw potatoes are neither palatable, nor easy of digestion; and it is a great discovery, which we owe to the sagacity of Dr. William Baly, that this vegetable is equally effective for these purposes, when cooked. During some months of continued observation of the prisoners confined in the Penitentiary at Millbank, I had remarked, without being able to account for it, that among the small number of *soldiers*, committed for comparatively short periods, for offences against military discipline, scurvy was not uncommon; whereas I noticed it in one instance only among the much more numerous class of *convicts*, whose term of imprisonment was considerably longer. Dr. Baly was afterwards appointed Physician to the Penitentiary, and the same curious fact soon caught his attention: and he has traced the cause. By the examination and comparison of various dietaries — those, namely, which have been adopted at different periods in the Penitentiary itself, those which, at the same period, were prescribed respectively for the military offenders, and for the ordinary convicts, and those in use in sundry other jails in which scurvy has occurred with different degrees of frequency — he has shown, most satisfactorily, that the liability to that malady has a strict relation to the amount of succulent vegetables consumed by the prisoners, and especially of potatoes. "Wherever this disease has prevailed, there the diet of the prisoners, though often abundant in other respects, has contained no potatoes, or only a very small quantity. In several prisons, the occurrence of scurvy has wholly ceased on the addition of a few pounds of potatoes being made to the weekly dietary. There are many prisons in which the diet, from its unvaried character, and the absence of animal food, as well as green vegetables, is apparently most inadequate to the main-

tenance of health; and where nevertheless, from its containing abundance of potatoes, scurvy is not produced."

In corroboration of these views may now be adduced the remarkable prevalence of scorbutic complaints in these islands, subsequently to the potatoe rot of 1846.

Now potatoes are *food* as well as medicine, and they are a cheap kind of food, and it may be hoped that a more general knowledge of their antiscorbutic properties, even when cooked, will abolish this wretched complaint, whenever a good supply of them is attainable. Dr. Baly believes that from three to six pounds, weekly, for each person, would suffice. He thus accounts for their salutary influence.

"A glance at the chemical analysis of the potatoe at once explains its antiscorbutic virtue. The various fruits, succulent roots, and herbs, which have the property of preventing and curing scurvy, all contain, dissolved in their juices, one or more organic acids—such as the citric, tartaric, and malic acids. Sometimes these acids exist in the free state, but more generally they are combined with potass, or lime, or with both these bases. Now potatoes have been submitted to most elaborate chemical examination by Einhoff and Vauquelin; and by both these chemists they have been found to contain a vegetable acid in considerable quantity. According to Einhoff, this acid is the tartaric combined with potass and lime. According to Vauquelin it is the citric, partly in combination with those bases, and partly in the free state. The farinaceous seeds, as wheat, barley, oats, and rye, which are destitute of antiscorbutic property, contain no organic or vegetable acids."

My friend Dr. Martin, of Ventnor, believes that the *cruciferae*, and water-cress especially, have more speedy and sure effect in removing sea-scurvy, than even lemon-juice. He assures me that, at St. Helena, he has seen the worst forms of the disease cured in the space of three days by an abundant ingestion of water-cresses.

Quite recently, (1848,) Dr. Garrod has re-investigated this subject, both chemically and clinically, and finds reason to distrust the "acid" theory, so long and so generally received. The disorder being clearly attributable to the *absence* of some essential ingredient in the food, and not to the *presence* of any noxious substance, he believes, and brings forward strong grounds for his belief, that *potass* is the deficient ingredient. If this be so, it is a most important discovery.

According to Dr. Garrod, the acids themselves, when separated from the antiscorbutic fruits and vegetables,—the citric and the acetic acids for example,—manifest no antiscorbutic virtues.

The following propositions form the substance of his very interesting announcement.

"1. That in all scorbutic diets, *potass* exists in much smaller quantities than in those which are capable of maintaining health.

"2. That all substances proved to act as antiscorbutics contain a large amount of *potass*.

"3. That in scurvy the blood is deficient in *potass*, and the amount of that substance thrown out by the kidneys is less than that which occurs in health.

"4. That scorbutic patients will recover when *potass* is added to their food, the other constituents remaining as before both in quantity and quality, and without the use of succulent vegetables, or of milk.

"5. That the theory which ascribes the cause of scurvy to a deficiency of *potass* in the food, is also capable of rationally explaining many symptoms of that disease."

The last of these propositions requires a little further unfolding. "Both soda and potass are constant constituents of the animal body, and it appears that they are not capable of replacing each other. For example, we always find the potass to exist in large quantities in the ash of muscle, soda in very small quantities. (Berzelius, Liebig). In the ash of the blood we find the relation reversed. It appears also that the muscular system requires the presence of potass; and we should therefore expect to find that where there is a deficient supply of this base, the effect would soon be manifested in the functions of that system. This we find to be the case in scurvy. Without any amount of wasting of the body we find marked muscular debility; and this perhaps is one of the earliest symptoms of the disease."

I say, if this theory of Dr. Garrod's prove true, its importance is great and manifest. Scurvy, whether on land or at sea, may henceforward be cured, or prevented, by a remedy at once simpler, cheaper, more easily portable, and more imperishable even than lemon-juice. At all times, and in every place, it may be procured from

the ashes of wood, or of plants,—especially (as Dr. Garrod has suggested) from that ubiquitous weed, tobacco, which contains it in abundance. A few grains of some salt of potass,—the neutral tartrate, for instance, the chlorate, or the phosphate,—might be mixed with the daily food, or administered as physic. Dr. Garrod concludes his paper by remarking, that even should his anticipation of the sanative quality of this substance be disappointed, it will still remain an interesting matter of fact, “that *potass* always accompanies the *real* antiscorbutic principle, was found deficient in scorbutic blood, and that several cases of scurvy rapidly recovered under the use of some of its salts, without the administration of any other remedy, dietetical or medicinal.”

The same causes which give rise to *sea* scurvy will produce precisely the same effect on land. Of this I must give you one illustration from my own case-book. In August, 1830, I admitted into the Middlesex Hospital a blacksmith, thirty-five years old, covered with round purple spots of various sizes, and with irregular blotches of ecchymosis. He had vomited blood on the preceding day. He was continually coughing up blood at the time of his admission, and his wife estimated the whole quantity that he had then lost to be more than half a pailful. The interior of his mouth and palate was pouring forth blood from a number of livid fungous tumours, formed by the extravasation of blood into the areolar tissue beneath the membrane, and the subsequent rupture of that membrane. He was passing blood by the bowels also; and his urine was loaded with blood.

Here were the *symptoms* of scurvy strongly marked. In the man's history we could trace its peculiar *cause*. He had long been subsisting on very poor and insufficient nutriment, seldom eating any meat, but living almost entirely on tea, coffee, and bread and butter. He had been too ill and weak to work regularly, yet he had been obliged occasionally to over-exert himself to obtain a scanty supply of food for himself, his wife, and a large family of children. He had been a settled dram-drinker, but for some time had taken much less of that stimulus; merely because he had not the means of procuring it. His pulse was frequent and feeble.

I had not much hope that this patient could be saved by any treatment. He was immediately put upon a diet of roast meat, and began to take daily half a pint of fresh lemon-juice diluted with a pint and half of water. This plan, with some tonic medicine, was commenced on the 3d of August. He improved at once. On the 8th all hæmorrhage had ceased; the fungous tumours in the mouth had disappeared, leaving small scars in the places they had occupied; and the discolouration of the skin was almost gone. The amendment was so striking and rapid, and so immediately consequent upon the institution of the treatment, that no room was left for mistaking recovery for cure.

It is chiefly by investigating the previous history of the patient, and by noting the degree of strength that he possesses, and the condition of his pulse, that we are guided in our diagnosis of ambiguous cases. The late Dr. Parry, of Bath, was one of the first to point out the efficacy of abstinence, venæsection, and purgatives, in some instances, at least, of purpura. I may refer you to an example of this kind detailed in the *Medical Gazette* for the 5th of April, 1828. It occurred in one of Dr. Latham's hospital patients; and several of the symptoms were very like those I have just been relating. In particular the whole tongue was livid, one half of it presenting the appearance of a large, black, bleeding fungus; and on the inner surface of each cheek were several black fungoid patches. The patient was voiding also unmixed blood from the bowels. In this case there was no evidence of the operation of any debilitating cause, and the pulse, though frequent, was *hard*. Bleeding from the arm always gave relief to his uneasy sensations: he was purged also, and put upon low diet. Under this plan he steadily improved, and in four or five days no vestige of the complaint remained except the fading spots. For some time afterwards, however, “the frequent use of active purgatives, and a rigid restriction to low diet, were necessary to obviate costiveness, and to keep down the circulation, which had a tendency to become over-active.”

You are not to suppose that all cases of purpura bear this sthenic character, or require these heroic remedies. Your treatment must be guided by the previous circumstances and habits of the patient, by the state of his pulse, and by the other symp-

toms which accompany the purple spots. And when you are in doubt what plan to pursue, make a cautious tentative bleeding. Take away a couple of ounces at a time, into a wine-glass, note carefully the appearance of the blood itself, and the effect of the bloodletting upon the patient; and then go on more boldly, or abstain thenceforth altogether from the lancet, according to circumstances. In many cases your chief reliance will be placed in the watchful employment of purgatives. These have been highly recommended by Dr. Harty, of Dublin, as having proved eminently successful in his practice. The late Dr. Whitlock Nicholl, and others, have spoken in terms of strong praise of the oil of turpentine, administered in moderate and repeated doses, as a remedy in purpura.

I have adverted to one peculiar source of danger in purpura, the hazard that blood may be effused in some vital organ where even a slight amount of hæmorrhage suffices to extinguish life. Dr. Bateman states that he had seen three instances in which persons were carried off, while affected with purpura, by hæmorrhage into the lungs. During the course of one week, in the year 1825, I was present at two inspections in the dead house of St. Bartholomew's Hospital, illustrative of the same point in respect of another vital organ, and involving a question in forensic medicine. The subjects of examination were both of them women of middle age who had been brought into the hospital covered with purple spots and bruise-like discolourations, and suffering hæmorrhage from the mucous membranes. Each of these women declared that the apparent bruises were marks of beatings received from her husband. One of them became suddenly hemiplegic a little while before she died. Of the manner of dissolution in the other case I am not sure. In both instances a considerable quantity of blood was spread over the surface of the brain, between its membranes: and in one of them, blood had been shed also into the cerebral substance, which it had extensively lacerated.

It may be worth mentioning that in one of these corpses there were indications, either of unusually rapid putrefaction after death, or (what I think more probable) of some degree of decomposition even before life was extinct. This woman died in the evening, and the body was examined the next day, twelve or fourteen hours afterwards. A quantity of fœtid gas escaped from the cavity of the abdomen as soon as it was opened, and small bubbles of air were seen to ooze from the areolar tissue of various parts of the body. Even when incisions were made into the *liver*, air frothed up, as it might do, under ordinary circumstances, from a section of the lungs.

I have no time left for discussing the pathology of these complaints. They are eminently *blood-diseases*. In scurvy the blood is starved of some essential ingredient, (probably potass,) which the juice of lemons, or other fresh succulent vegetable food, readily supplies and renews. When drawn from a vein the blood is often visibly unnatural. A very small quantity was taken before I saw him, from the arm of the blacksmith whose case I just now mentioned. After standing for some time, it continued to fill the whole area of the vessel in which it had been received, without any apparent contraction, or separation of serum. On its flat upper surface was a thick, gray, semitransparent jelly, and beneath this there was, strictly speaking, no coagulum, but a black, semi-fluid substance of the consistence of syrup. Huxham describes similar appearances. "The blood of such persons (says he), when it hath been drawn off, always appears a mere gore, as it were, not separating into crassamentum and serum as usual, but remaining in a uniform half coagulated mass, generally of a livid or darker colour than usual, though sometimes it continues long very florid; but it always putrefies very soon." In another place, when describing a particular case, he says, "I found that neither of the portions of the blood that had been drawn had separated into serum and crassamentum as usual, though it had stood many hours; but continued, as it were, half coagulated, and of a bluish livid colour on the top. It was most easily divided on the slightest touch, and seemed a purulent sanies rather than blood, with a kind of sooty powder at bottom."

Dr. Budd, however, states that in some cases of scurvy the separation of blood into serum and clot is as perfect, and takes place as readily, as in healthy blood.

When you recognize the disease as genuine scurvy, and trace a previous abstinence, whether forced or voluntary, from fresh vegetables, the treatment is plain; you must

supply the kind of nutriment which has been defective, and support your patient's strength in such other ways as the circumstances of the case may dictate.

And now, gentlemen, I must needs stop. Here ends my course. And if this were all I had to say, I should say it with something like glee, and you, no less than myself, would rejoice that at length a breathing-space and holiday had arrived. But I cannot feel so when I add that this is the last lecture, not of this course merely, but the last of any kind, that I am ever likely to deliver in King's College. I cannot say this without concern and regret.

I am quite aware that my lectures have been in many respects imperfect. They have been very unequal to my own wishes. But they have been as full, and as carefully weighed, as my broken leisure, and irregular opportunities, and slender ability, would permit. I can only hope that at any rate I have not misled you. If I have been intelligible, if I have drawn such a sketch of a great and difficult subject as may help you in studying it for yourselves, I have achieved my task. I told you, in the outset, that I could not here teach you the *practice* of physic, but only its *principles*. It would be idle for me to speculate further upon the success of my endeavours. You are to be the judges of that matter. Whatever rules and precepts I have laid down, you will soon test by your own experience, and adopt or reject them accordingly. The well-known maxims of Bacon apply with especial truth to medical instructors and their pupils. "*Etsi non displiceat regula, oportet discentem credere; huic tamen conjungendum est, oportet jam edoctum judicio suo uti: discipuli enim debent magistris temporariam solum fidem, judicii que suspensionem, donec penitus imbibierint artes: non autem plenam libertatis ejurationem, perpetuamque ingenii servitutem.*"

Retiring reluctantly from this place, in obedience to the force of circumstances, there are yet many things to comfort and console me. It is a great satisfaction to reflect that I have never had any serious disagreement with yourselves; have never experienced any but the most respectful and kind reception, either from my present or from any preceding class. I have reason to thank you—and I do thank you—for the courtesy and attention you have at all times shown me. It is a source of gratification also that I carry with me the good will, as I believe, of my excellent colleagues; and that I go without having forfeited any of that confidence which the Council first reposed in me as their servant, when they offered me, without solicitation, the chair I now resign.

Had I been a few years younger, unembarrassed by previous official engagements, and somewhat more at leisure than I am, I should have been glad and proud to have attached myself to the new hospital, and to have laboured still in the cause of King's College, and of its Medical School. But it is otherwise ordered: and I will mention as the last source of consolation in taking leave of you, my conviction that to you my loss (if without presumption I may so venture to speak of my resignation) will be more than supplied by my successor. I know that gentleman well. I know, indeed the world knows, his talents. He was highly distinguished in the Senate House at Cambridge. He has since devoted, and will continue to devote, the powers of a very strong intellect, to the investigation of disease. Dr. Budd is one of the most strenuous cultivators of our science that I am acquainted with: and I am confident — without any affectation of modesty — that he will soon give a much better course of lectures than you have heard from me. That you may prosper under his instruction, and afterwards; that by the humane exercise of our noble calling, you may do good in your generation, to others, and so to yourselves; is my earnest desire and prayer. I hope it is unnecessary for me to assure you that I shall always continue to take a lively interest in your welfare individually; and that it will give me sincere pleasure if I shall find any future opportunity of rendering you any service. Gentlemen, I do not like this sort of parting, and I will not further protract the pain that belongs to it; but bid you finally, and most cordially—Farewell.

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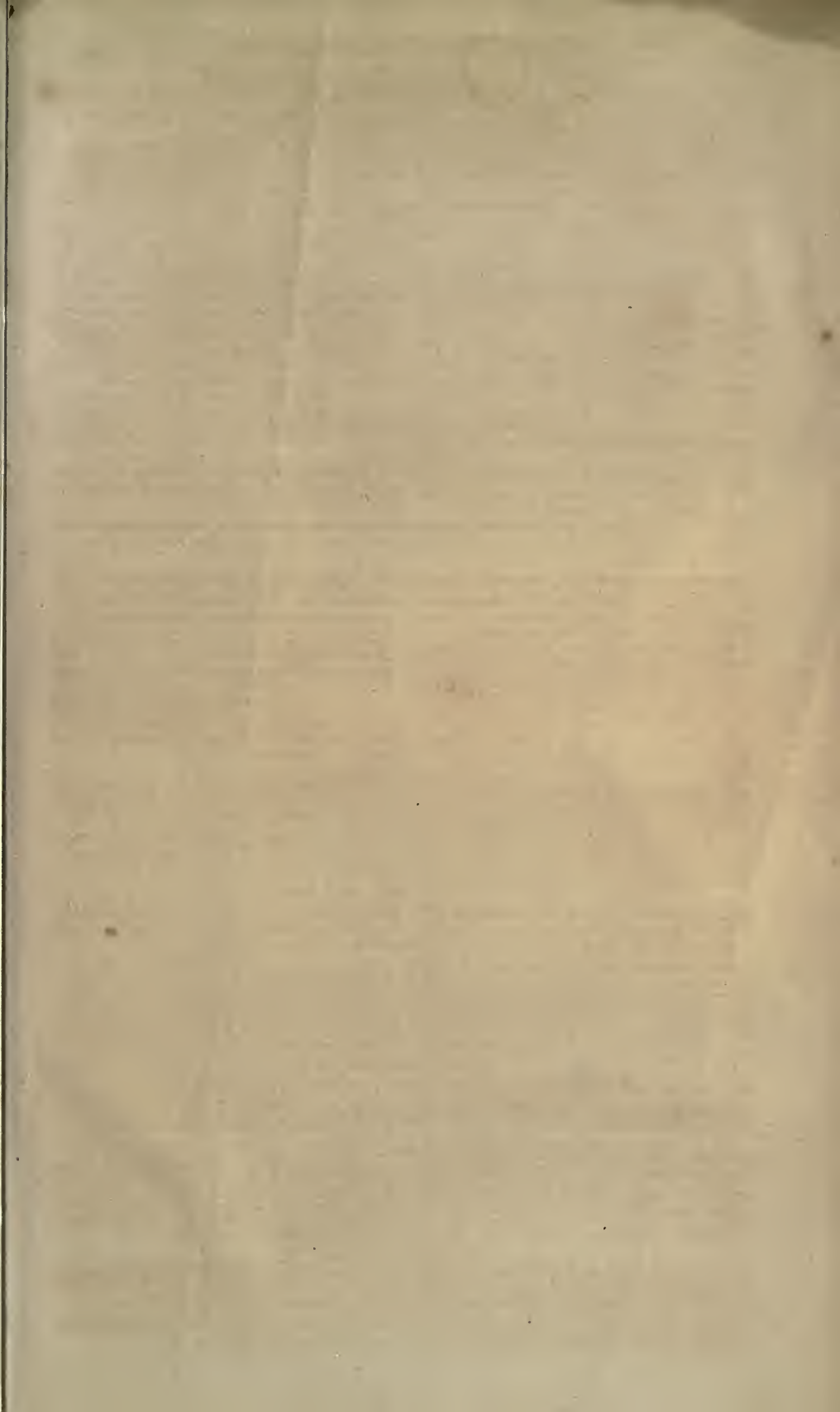
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